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The Need for Climate Engineering Research

Like it or not, a climate emergency is a possibility, and geoengineering could be the only affordable and fast-acting option to avoid a global catastrophe. Climate change triggered by the accumulation of greenhouse gases emitted into the atmosphere has the potential of causing serious and lasting damage to human and natural systems. At today's atmospheric concentrations, the risk of catastrophic damage is slight—though not zero. The risk will probably rise in coming years if atmospheric concentrations continue to increase. Although not everyone agrees with this assessment, it is supported by the bulk of the scientific evidence.

For the moment, the United States and other nations are trying to address this risk by controlling emissions of carbon dioxide (CO_2) and other greenhouse gases into the atmosphere, with mixed success at best. The time may well come, however, when nations judge the risk of climate change to be sufficiently large and immediate that they must "do something" to prevent further warming. But since "doing something" will probably involve intervening in Earth's climate system on a grand scale, the potential for doing harm is great.

The United States needs to mount a coordinated research program to study various options for mitigating climate change in order to ensure that damaging options are not deployed in haste. The United Kingdom and Germany have initiated research programs on such climate intervention technologies, and many U.S. scientists are already engaged in this topic, funded by a hodgepodge of private funds and the redirection of federal research grants. Some senior managers at federal agencies such as the National Science Foundation (NSF), Department of Energy (DOE), and National Aeronautics and Space Administration would like to initiate research funding, but they cannot act without political cover, given the understandably controversial nature of the technology. Given the rapid pace at which the research debate about governance is moving in the United States and abroad, delay in establishing a federal program will make it progressively harder for the U.S. government to guide these efforts in the public interest. There is, therefore, a need to establish a coordinated program with deliberate speed.

Of course, it remains critically important that the United States and other nations continue efforts to reduce emissions of greenhouse gases into the atmosphere. Indeed, much deeper cuts are needed. Reducing emissions will require, first and foremost, the development and deployment of low-carbon–emission energy systems. But even with improved technology, reducing emissions might not be enough to suf-

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ficiently reduce the risk of climate change.

Scientists have identified a range of engineering options, collectively called geoengineering, to address the control of greenhouse gases and reduce the risks of climate change. One class of geoengineering strategies is carbon dioxide removal (CDR), which removes greenhouse gases from the atmosphere after they have already been released. This approach may involve the use of biological agents (such as land plants or aquatic algae) or industrial chemical processes to remove CO_2 from the atmosphere. Some CDR operations may span large geographic areas, whereas other operations may take place at centralized facilities operating in a relatively small area. Another class of strategies is solar radiation management (SRM), which involves a variety of methods for deflecting sunlight away from Earth or otherwise reducing the levels of solar energy in the atmosphere.

These two strategies are radically different. CDR seeks to address the underlying cause of the climate problem: elevated greenhouse gas concentrations. These approaches are not inexpensive and take time to implement at scale. The more promising of these approaches introduce no unprecedented new environmental or political risks and introduce no fundamentally new issues in governance or regulation. Some CDR approaches, such as the planting of forests, are already considered in international climate negotiations.

SRM seeks to diminish the adverse climate effects of elevated greenhouse gas concentrations without addressing the root cause of the problem. The best of these approaches are shockingly inexpensive (at least with respect to direct financial costs of deployment) and can be deployed rapidly. However, they do introduce unprecedented environmental and political risks, and they pose formidable challenges for governance and regulation. No SRM proposal has yet been seriously considered in an international climate negotiation.

Both approaches may contribute to cost-effective environmental risk reduction, yet there are no federal research programs systematically addressing these options. How should such programs be structured? Given that the two strategies are so different, it would make sense for the government to develop at least two research program areas. One should focus on CDR and other options to reduce the concentrations of greenhouse gases that have already been released to the atmosphere. Another program area should focus on SRM and other options to diminish the climate consequences of increased greenhouse gas concentrations. Each of these strategies is examined below.

CDR

Because of the longevity of atmospheric CO_2, managing the long-term risk of climate change will require us to reduce the atmospheric concentration from current levels. Managing emissions is necessary but not sufficient. But CDR can make a difference only if CO_2 is captured on a huge (gigaton) scale. The sheer scale of the challenge means that CDR always will be relatively slow and expensive.

Research on CDR should be divided into four different research programs. Little coordination is needed among these different research activities; they are so different that there is little to be gained by combining the research under a single umbrella. The research programs would focus on:

Biomass with carbon capture and storage. Plants remove CO_2 from the atmosphere when they grow. When burned in power plants to produce energy, plants release their accumulated CO_2, producing power that is roughly carbon-neutral. If the plants are burned in power plants that capture CO_2 and store it underground in geologic reservoirs, then the net effect is to move carbon from the active biosphere to the deep geosphere, reversing the effect of producing and burning fossil fuels This approach is already being investigated within DOE and the U.S. Department of Agriculture (USDA), and the interagency cooperation seems to be working well.

Chemical capturing of CO_2 from air. Laboratory tests have demonstrated that chemical engineering approaches can be used to remove CO_2 from ambient air. This CO_2 can then be compressed and stored underground in geologic reservoirs. Because the concentration of CO_2 in air is much lower than the concentration in power-plant exhaust gases, capturing CO_2 from air normally would be more expensive than capturing it from power plants. But there are ways around this problem. For example, facilities to remove CO_2 from ambient air could be made more cost-efficient by locating them near cheap but isolated sources of energy, such as natural gas fields located in remote areas. Furthermore, we may be willing to pay high prices to remove CO_2 from the atmosphere should the consequences of high atmospheric CO_2 concentrations prove worse than anticipated. For example, industrial CDR might be seen as preferable to SRM. [Full disclosure: One of us (Keith) runs a start-up company developing this technology.] DOE is the logical choice to lead this research.

Increasing carbon storage in biological systems. A number of approaches have been suggested for increasing carbon storage in biological systems. These approaches include encouraging the growth of forests and promoting the use of agricultural practices, such as "no-till" agriculture, that foster the storage of carbon in soils. DOE, USDA, and NSF have supported research on some of these methods, and this approach has received some attention in international climate negotiations. However, biological systems are relatively inefficient in their ability to capture CO_2. It is estimated that it would take approximately 2.5 acres of crop land to remove the CO_2 emission from just one U.S. resident—an impractical requirement. But even though these approaches are unlikely to play a leading role in climate mitigation, some techniques may prove cost-effective, especially when the land can be used for multiple purposes or when other benefits may accrue.

It also has been suggested that the biomass accumulated in plant matter could be buried, either on land or at sea, in a way that would ensure long-term storage. Advocates of such methods argue that they would confer a considerable advantage over, for example, growing a forest and leaving it in place. With biomass burial, the same land could be used repeatedly to capture CO_2, whereas a forest grows only once and does not significantly increase its carbon store after it has reached maturity. Farm waste might be another source of material that might be suitable for burial. Overall, however, current evidence suggests that it would make more environmental sense not to bury biomass but to use it in place of coal in electric power plants, which are notorious CO_2 emitters.

In another biological approach, carbon storage in the ocean could perhaps be increased somewhat by fertilizing the ocean with nutrients, such as iron, nitrogen, and phosphorus, which would encourage tiny organisms to bind the carbon in their physical structures. However, most observers have concluded that ocean fertilization is unlikely to be an attractive option that can be deployed at large scale. Fertilizing the ocean with iron to promote storage has received the most attention, because in areas where iron is a limiting nutrient for biological growth, this would probably be the most cost-effective option. However, there are many questions regarding the effectiveness of these approaches in storing carbon for long periods. Furthermore, because the oceans are a global commons, ocean fertilization options, unlike nearly every other CO_2 removal method, raise a range of thorny problems related to governance and regulation. NSF and DOE have funded some studies of ocean fertilization, but the research is now largely dormant. Also, some of the governance issues are being addressed under the London Convention and Protocol, an international effort to protect the marine environment from human activities, and the Convention on Biological Diversity, an international agreement to protect the planet's wealth of living organisms.

Distributed chemical approaches. In general, these approaches involve using massive amounts of basic minerals that react with acidic CO_2 to form new stable minerals. These approaches amount to an acceleration of the natural weathering cycle that in the very long run removes CO_2 from the biosphere. One such approach is based on the fact that the CO_2 in seawater is eventually incorporated into solid carbonate minerals within bottom sediments. The rate of these chemical processes can be accelerated by sprinkling finely crushed limestone over certain parts of the ocean. Alternatively, calcium or magnesium oxides can be added to seawater, increasing the water's capacity to hold CO_2 in storage and prevent it from ever returning to the atmosphere. These approaches would also neutralize carbon acidity in the ocean, helping to alleviate a problem known as ocean acidification.

None of these distributed chemical approaches is a magic bullet. There also are a number of environmental concerns, including the scale of mining that would be required. Nevertheless, such approaches might prove cost-effective relative to conventional carbon capture and storage from power plants.

As envisioned, the research programs on CDR might best be housed within DOE, where they would fit neatly into the agency's current carbon capture and storage research program. Preliminary research should focus on assessing the barriers and potential of each proposed approach, including costs and benefits.

Indeed, cost is a primary critical issue with many proposed CDR methods. Thus, as a critical complement to research focused on the technical hurdles facing CDR, research also must focus on economic questions. To be quantitatively important, CDR methods would need to be deployed at large scales, but none of these methods is likely to be both scalable and inexpensive. Several of these options, however, could potentially play an important role as part of a portfolio of climate response options, and there may be particular niches or market segments for which these approaches represent the most cost-effective environmentally acceptable option.

Making an objective analysis of the economics of CDR systems is one area where cross-cutting research is needed. (Such research also can help answer important questions about the environmental impact of the methods.) Yet these specific exceptions prove the rule that there is little to be gained by grouping research efforts together. Such targeted analyses are best performed by independent agencies and investigators, because the government agencies that fund technology R&D typically become advocates of their technology and thus poorly suited to provide objective analysis of its performance.

Solar radiation management

Earth can be cooled by a variety of engineering methods, some of them more practical than others, given current technology. There are four main classes of SRM proposals, which are described below in approximately decreasing likelihood of feasibility at large scale:

Stratospheric or mesospheric aerosols. Small particles high in the atmosphere can potentially scatter or reflect sunlight back to space, exerting a net cooling effect on Earth's climate. However, because of particle aggregation and gravitational settling, it is not clear that such an aerosol layer could be sustained indefinitely. Thus, maintaining this much solar reflection high in the atmosphere could involve spreading material over a broad altitude range or deploying "designer" particles that are less susceptible to aggregation. Further, it would be desirable to be able to control the latitudinal distribution of these particles; ideally, it would be possible to turn them "on" or "off" at will to exert a high degree of geographic and temporal control. The potential for designing such particles is unknown at this time.

Whitening marine clouds. It has been proposed that low clouds in some oceanic regions could be whitened with a fine spray of seawater, and if done on a large enough scale, this could cool Earth considerably. This proposal rests on widely accepted understanding of cloud physics and how that physics is likely to affect climate. Two lines of study—on natural gases that emanate from the oceans and on ship exhausts—indicate that the proposed method should work at some level. Initial calculations suggest that the method could conceivably offset 10 to 100% of the global mean temperature increase from a doubling of atmospheric CO_2 concentration.

Satellites in space. It has been proposed that vast satellites could be constructed in space to deflect sunlight away from Earth. The scale of such an undertaking is so enormous that most observers do not feel that such an effort is likely in this century. Nonetheless, placing a sunblock between Earth and the Sun is a simple and effective conceptual approach to addressing threats from global warming. Such a strategy could potentially be of interest at some point in the distant future if the global community finds the need to construct systems that would deflect sunlight for many centuries.

Whitening the surface. It has been proposed that whitening roofs, crops, or the ocean surface would reflect more sunlight to space, thereby exerting a cooling influence on planetary temperatures. With regard to crops, there is simply not enough crop area or potential for change in reflectivity for this sector to be a game changer. Similarly, there is not enough roof area for changing roof color to make a substantive difference in global climate change, although whitening roofs in some cases may confer co-benefits (such as reducing cooling costs and helping reduce the urban heat island effect). Various proposals have been made to whiten the ocean surface, stemming back to at least the early 1960s, but the ability to do so has not been demonstrated.

In their current form, the best SRM methods have several common properties: They have relatively low direct costs of deployment, they may be deployed rapidly and are fast-acting, and they are imperfect. They are intrinsically imperfect because greenhouse gases and sunlight act differently in Earth's climate system. Nevertheless, every climate model simulation that has applied some "reasonable" reduction in absorption of sunlight has found that these approaches could potentially diminish most climate change in most places most of the time, at least for a doubling of atmospheric CO_2 content.

Long-established estimates show that SRM could potentially offset global average temperature increases this century at a direct cost that is several hundred times lower than the cost of methods that achieve the same cooling by reducing greenhouse gas emissions. This is because such a tiny mass is needed: A few grams of sulfate particles in the stratosphere can offset the radiative forcing of a ton of CO_2 in the atmosphere. At a cost of a few thousands of dollars per ton for aerosol delivery to the stratosphere, the direct cost of offsetting the global mean temperature increase from a doubling of atmospheric CO_2 is estimated to be on the order of $10 billion per year. Of course, the need to operate satellite, atmosphere, and ground-based observation systems to monitor outcomes could increase costs substantially.

Although SRM efforts might be able to diminish most climate change in most places most of the time, it is also likely that these approaches will harm some people in some places some of the time. People who suffer harm may seek compensation. If militarily or politically powerful, they could seek to prevent continued deployment and thus could generate military or political conflict. Even if environmental benefits exceed environmental damages overall, indirect costs associated with the possible need to compensate those adversely affected could dominate the overall cost picture. It does not even need to be the case that the climate intervention system actually causes the climate damage; if people in a region believe that they are harmed by such a system, this could be enough to motivate conflict.

The fact that SRM approaches can cool the planet rapidly is known because nature has already performed experiments that scientists have analyzed. After the eruption of Mt. Pinatubo in the Philippines in 1991, Earth cooled nearly 1° Fahrenheit (about 0.5° degree Celsius) in less than a year. The aerosols stayed in the stratosphere for only a year or two, but scientists' calculations suggest that if that amount of reflection of sunlight to space could be sustained—perhaps by injecting a continuous stream of material into the stratosphere—it could compensate for the amount of warming produced by a doubling of atmospheric CO_2 content. The eruption was not without adverse consequences, however, because the Ganges and Amazon Rivers had their lowest flow rates on record.

Of course, a world that is cooled by the diminished absorption of sunlight is not the same as one cooled by a reduction in greenhouse gas concentrations. For the same amount of cooling, an SRM-cooled world would have less rainfall and less evaporation. SRM could affect Earth's great weather systems, including monsoonal rains and winds. Thus, SRM techniques are not a perfect alternative to greenhouse gas emissions reduction and can at best only partially mask the environmental effects of elevated CO_2. Still, SRM may be the only fast-acting approach to slowing or reversing global warming. Therefore, it may have the potential to become a powerful tool to reduce the risks associated with unexpectedly dangerous climate consequences.

Moving to a research plan

Even given the potential benefits, the idea of deliberately manipulating Earth's energy balance to mitigate human-driven climate change will be interpreted by many people as dangerous hubris. Indeed, a common and not entirely inappropriate first reaction to SRM is to reject it out of hand as an effort destined to fail. Society has some memory of past cases where overeager technological optimism led to disastrous harm. But it is also necessary for society to avoid overinterpretation of past experience. Responsible management of climate risks calls for emissions cuts, but also for clear-eyed exploration and evaluation of SRM capability.

Opinions about SRM are changing rapidly. Only a few years ago, the topic generally was not openly discussed. Many people, within and beyond the scientific community, now support model-based research. However, field testing remains controversial and will probably grow even more so. Because of the serious and legitimate concerns about the enormous leverage that SRM technologies may provide in regulating the global climate, it is crucial that the development of these technologies be managed in a manner that is as transparent as possible.

In designing an SRM R&D program, the federal government should follow several basic guidelines. Of key importance, the program should include organizations conducting research aimed at developing and testing systems, and different organizations conducting research aimed at predicting the environmental consequences of their deployment. Further, high-quality observing systems will be needed to support both of these functions. If the organization responsible for developing systems is also performing the environmental assessments, there will be an apparent conflict of interest. There may be an incentive to find that currently funded projects represent environmentally viable strategies. Therefore, in designing an SRM research program, it will be wise to separate systems R&D from environmental assessment efforts. Within this broad framework, research efforts should take a red team/blue team approach, wherein one team is tasked with showing how an approach can be made to work, and another team is tasked with showing why the approach cannot produce a system that can actually diminish environmental risk at an acceptable cost.

A government research program on such a consequential and controversial topic needs a formal mechanism to enable input by stakeholders outside government. A research advisory board that includes representatives from major nongovernmental organizations such as think tanks and environmental advocacy groups could serve this purpose.

The research program would have three phases:

Phase 1. This phase would center on exploratory research. Funding for this research should start at $5 million per year and then gradually ramp up to $30 million per year. It would focus on doing "no-brainer" research: high-yield research that can be conducted with computer models or within a laboratory. While this obvious low-lying scientific and technical fruit is being picked, an effort must be initiated to design an SRM research plan for the next decade.

The planning process must include all stakeholders, and the final plan must include a proposed institutional arrangement to ensure its execution. However, it would not be wise to delay getting started with research until the detailed plan and institutional arrangements are in place. In addition to advancing the science, starting research now will start help train a cohort of graduate students who can provide the creativity and leadership that will needed in the next phase.

Phase 2. This phase, funded at a level of $30 million to $100 million per year, will involve doing sustained science with small-scale field experiments. Early tests would focus on understanding processes. Later tests potentially could be large enough to produce barely detectable climate effects and reveal unexpected problems, but be small enough to limit risks. Because experiments could expand gradually toward large-scale deployments, it is important that experiments proceed with effective and appropriate governance and regulation.

Phase 3. This phase, if warranted by earlier studies, would involve the development of a deployable system (or at least in some way prepare for that development). Building the capability for deployment at climate-relevant scales would require substantially larger investments than previous phases. Depending on the systems chosen, this phase of research could take on very different characteristics. Clearly, such development must be performed with as much transparency, democratic control, and international cooperation as possible.

Some of the problems of international governance are almost certain to be new and difficult. In efforts to reduce CO_2 emissions, the key governance challenge is motivating many actors to take collective action toward a common goal. For SRM, in contrast, the main problem is establishing legitimate collective control over an activity that some might try to do unilaterally without prior consultation or international risk assessment.

In building international cooperation and developing standards, it may be best to start from the bottom, by developing knowledge and experience before formalizing universal agreements. A first step could be developing a transparent, loosely coordinated, international program to support research and risk assessments by several independent teams. As part of this process, efforts should be made to engage large groups of experts and interest groups, such as former government officials and leaders of involved nongovernmental organizations. Developing iterative relationships between governance and the emerging current scientific and technical research would be the core of this bottom-up approach.

Reasons for action

A common question is why SRM experiments should be conducted in the field now. The answer is that if a climate emergency, such as widespread and sustained crop failures throughout the tropics or a collapse of large parts of Greenland into the ocean, should arise, it would be reckless to only then begin SRM field tests. If there is at least some risk of a climate emergency and some likelihood that SRM techniques could help relieve the situation, then it seems logical to test the approaches before the time when an emergency is more likely to develop.

Moreover, in the event of a crisis, even if atmospheric CO_2 content were to stabilize, which would require dramatic reductions in greenhouse gas emissions, global mean temperatures would continue increasing. SRM techniques are the only options that could potentially cool Earth quickly, so having them ready at hand (or at least as ready as possible) could provide significant benefit.

Some critics also argue that pursing SRM research at any substantial level will reduce society's resolve to reduce emissions of greenhouse gases. But evidence from other cases suggests that this is unlikely. For example, research that resulted in the development of seatbelts and airbags in cars has still provided an immense benefit even if it, in a small way, influenced people to drive faster and more recklessly. Moreover, if SRM proves to be unworkable or to pose unacceptable environmental risks, the sooner scientists know this, the faster they can take these options off the table. Indeed, if SRM approaches are not subjected to serious research and risk assessment, SRM might incorrectly come to be regarded as a safety net. The stakes are simply too high for us to think that ignorance is a good policy.

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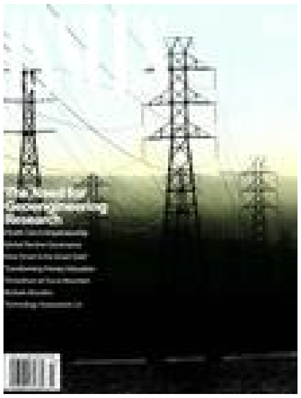

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