Testimony

To the House Science Committee

Geoengineering III Hearings

Dr. Jane C. S. Long

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Mr. Chairman, members of the committee, thank you for this opportunity to add my comments about geoengineering to the record. This is a difficult and complex topic and your willingness to organize these sessions is both courageous and admirable. I hope I can add a little to the dialogue.

My academic background is geohydrology; I have worked in environmental and resource problems for over 35 years. My experience includes nuclear waste storage, geothermal energy, oil and gas reservoirs, environmental remediation, sustainable mining, climate science, energy efficiency, energy systems and policy, adaptation and recent attention to geoengineering. I have worked at two national laboratories, Lawrence Berkeley National Lab and Lawrence Livermore National Lab and have been a dean of engineering and science at University of Nevada, Reno. I am a Senior Fellow of the California Council on Science and Technology (CCST) and an Associate of the National Academy of Sciences. In my current position, I am a fellow in Lawrence Livermore National Laboratory's Center for Global Strategic Research and Associate Director at Large for the laboratory. I work in developing strategies for a new, climate friendly energy system and currently chair the CCST's California's Energy Future committee which is charged with examining how California could meet 80% reductions in greenhouse gas emissions by 2050. I am also a member of the State of California's Climate Change Adaptation Advisory Council. I currently serve as co-chair of the National Commission on Energy Policy's (NCEP) Task Force on Geoengineering. I work to understand and advance a full spectrum of management choices in the face of climate change: mitigation, adaption and now geoengineering.

My comments today reflect the perspective of my experience. They are my own opinions and do not reflect positions taken by my laboratory (Lawrence Livermore National Laboratory) or the NCEP task force on geoengineering I co-chair.

Introduction

Our climate is changing in response to massive emission of greenhouse gases. First, we have to stop causing this problem. We have to change our energy system, food system, transportation system, industries and land use patterns. Even with mandatory concerted effort, such massive change will take decades. During these same decades we will continue to burn fossil fuels and add to the greenhouse gases we have already emitted. This atmospheric perturbation will last for centuries and will continue to warm our planet. We have created, and will continue to create unavoidable risk of disruptions to our way of life which may force us to spend more on protection (*resistance*), change our way of life to accommodate the change (*resilience*), or perhaps even to abandon parts of the Earth that are no longer habitable by virtue of being under water or having too little fresh water (*retreat*).

Because the carbon dioxide we have already emitted will be with us for centuries, the problem of climate change cannot be "solved" in the same sense that other pollution problems -- such as ozone depletion -- have been solved by phasing out emissions over time. Climate change is like a chronic disease that must be managed with an arsenal of tools for many years while we struggle with a long term cure. In this future, if climate sensitivity (the magnitude of temperature change resulting from a doubling of CO2 concentrations in the atmosphere) turns out to be larger than we hope or mitigation proceeds too slowly, we cannot rule out the possibility that climate change will come upon us faster and harder than we -- or the ecosystems we depend on -- can manage. No one knows what will happen, but we face an uncertain future where catastrophic changes are within the realm of the possible.

In the face of this existential threat, prudence dictates we try to create more options to help manage the problem and learn whether these are good options or bad options. I believe this is the most fundamental of ethical issues associated with our climate condition. We must continue to strive to correct the problem. This is why scientists today have become interested in a group of technologies commonly called geoengineering that are aimed at ameliorating the harmful effects of climate change directly and intentionally. Intentional modification of the climate carries risks and responsibilities that are entirely new to mankind. (We accept unintended but certain harm to climate from energy production much more easily that we accept unintended harm through intentional climate modification.) As we consider geoengineering, we have to recognize that society has not been able to quickly or easily respond to the climate change challenge. Consequently, the geoengineering option isn't just a matter of developing new science and technologies. It is also a matter of developing new social and political capacities and skills.

As much as I think we should research geoengineering possibilities, I think we should remain deeply concerned by the prospect of geoengineering. We will not be able to perfectly predict the consequences of geoengineering. Some effects may be irreversible and unequally distributed with harm to some even if there is benefit to many. Geoengineering could be a cause for conflict and a challenge for representative government. Geoengineering might be necessary in the future, but as we proceed to investigate this topic, we will need extremely good judgment and a very large dose of hubris.

Three different classes of geoengineering have been identified (American Meteorological Society, http://www.ametsoc.org/POLICY/2009geoengineeringclimate_amsstatement.html). The first is actively removing greenhouse gases from the atmosphere. This has been called "***Climate remediation***" or carbon dioxide removal (CDR) or "carbon management". Climate remediation is similar in concept to cleaning up contamination in our water or soil. The first problem is to stop polluting (mitigation) and the second is to remove the contaminants (remediation) and put them somewhere – for example filter CO2 out of the air and pump it underground.

The second set of technologies has been called "***Climate intervention***" where we act to modify the energy balance of the atmosphere in order to restore the climate closer to a prior state. Climate intervention has also been called solar radiation management (SRM) or sun-block technology and some consider the technologies to be a radical form of adaptation. If we cannot find a way to live with the altered climate, we intervene to roll back the change.

The third is a ***catch-all category*** that includes technologies to manage heat flows in the ocean or actions to prevent massive release of methane in the melting Arctic. These technologies are less well understood and developed, but the classification recognizes that not all the ideas are in and, as well, we may wish to address some very specific global or sub-global scale emergencies caused by climate change.

I do not view any of these methods as stand-alone solutions, but some or all of these could be integrated in a comprehensive climate change strategy that starts with mitigation. A comprehensive climate change strategy might include:

- A steady, but aggressive transformation of the global energy system to eliminate emissions with concurrent elimination of air pollution in a few decades (mitigation)
- Carbon removal over perhaps 50 to 100 years to return to the "safe zone" of greenhouse gas concentrations (climate remediation)
- Time limited climate intervention to counteract prior emissions and reductions in air pollution, tapering off until greenhouse gases fall to a "safe" level (climate intervention).
- Specific focused actions to reverse regional climate impacts such as preventing methane burps or melting Arctic ice (technologies from the "catch-all" category)

My remarks below do not discuss the technologies themselves in any depth as that has been done by others nor are they comprehensive. I will discuss some of the implications for research and experimentation. Where possible I will comment on existing US research programs and their capacity or suitability to expand into geoengineering research. As well, I will try to point to specific research topics that I have not seen in the geoengineering discourse up to now which are critical for any future geoengineering capability. I will bring out specific issues related to governance and international relations and some possible approaches for dealing with these. Discussion of governance and international relationships will focus mainly on climate intervention methods which are in general a more difficult societal and research problem. I will also some important research needed in climate science which is also critical for geoengineering.

Climate remediation technologies

Climate remediation technologies are with some exceptions relatively safe and non controversial. They address the root cause of the problem, but these methods are slow to act. It would take years if not decades to reduce the concentration of CO_2 in the atmosphere through air capture and sequestration. These technologies are expensive when compared to the option of not emitting CO_2 in the first place. It costs less to capture concentrated streams of CO_2 in flue gas or to use non-emitting sources of energy in lieu of burning fossil fuel, so many carbon removal technologies are likely to remain uneconomical until we have exhausted the opportunities for mitigation. However, research into these ideas is important because at some point we may decide that the atmospheric concentrations must be brought down below stabilized levels. If we don't want to wait many hundreds of years for this to happen through natural processes, we may have to actively remove greenhouse gases. As we begin to understand more about the costs of adapting to unavoidable climate change, remediation technologies may become a cost effective option. Developing carbon removal technology that is reliable, safe, scalable and inexpensive should be the goal of a research program.

Some of the more promising technologies in carbon removal are closely related to carbon capture and storage (CCS) technologies. CCS offers the most, if not only promise for preventing greenhouse gas emissions from fossil fuel-fired electricity generation. For CCS, we contemplate separating out CO_2 after combustion of coal and then pumping it deep underground into abandoned oil or gas fields or saline aquifers. The technologies for removing CO_2 from air (air capture) and flue gas are similar.

In general, CCS is expected to be much less expensive than air capture, but air capture does have some possible advantages over CCS. It may be possible to site air capture facilities near a stranded source of energy (remote geothermal or wind power for example, or in the middle of the ocean) and also near geologic formations that are capable of holding the separated gases. This arrangement might obviate some of the infrastructure costs associated with capturing CO_2 at a power plant and having to choose between locating the power plant near the geologic storage reservoir and transmitting the power to the load, or conversely locating the power plant near load and conveying the CO_2 to the storage facility. Further the cost of capture is likely to decline. In the long-run these considerations may become dominant.

After capturing the CO_2, it has to be put somewhere isolated from the atmosphere. Currently, we are considering geologic disposal: pumping the CO_2 deep underground. There are important policy and legal issues associated with geologic storage. The implementer must obtain rights to the underground pore space and be able to assign liability for accidents and leakage *etc*. These same issues exist for storage of CO_2 in a CCS project and the US CCS project currently deals with them. However, Keeling (R. Keeling, Triage in the greenhouse, Nature Geoscience, 2, 820-822, 2009) has suggested that the amount of CO_2 we may need to remove from the atmosphere is such that we will have to consider disposal in the deep ocean as a form of environmental triage. Ocean dumping would clearly involve much more serious governance issues, similar to climate intervention which are discussed below.

Because of the similarities with CCS, it makes some sense to augment current research by DOE's Fossil Energy program in CCS to include separation technology related to air capture of CO_2. There are technical synergies in the chemical engineering of these processes and the researchers are in some cases the same. The research is complementary. The governance issues related to geologic storage are exactly the same.

A second governance issue has to do with intellectual property (IP). If there is no significant price for carbon, and carbon removal becomes a function of the government (like picking up the garbage) we might consider making any air capture technology we develop freely available throughout the world as it is in our interest to have anyone who is able and willing help clean up the atmosphere. If however, there is a price for carbon, then IP could help to motivate innovation to gain a competitive edge which is also in the interest of society. Unfortunately, we don't have a price for carbon now, and we are not sure whether we will, so the choice is difficult.

Beyond air capture, the Royal Society report on Geoengineering (J. Shepherd et al., Geoengineering the Climate: Science, Governance and Uncertainty, The Royal Society, London, 2009 http://royalsociety.org/geoengineeringclimate/) lists a number of other carbon removal technologies. Among these, augmentation of natural geologic weathering processes and biological methods would fit well within either NSF's science programs or in DOE's Office of Science program. For the near term, research will involve the kind of modeling studies and field experiments that are already a mainstay of these programs. NSF is focused on university researchers and is extremely competitive which means that high risk ideas will likely not be funded. In the DOE program, there is more focus on mission, high risk research, and national laboratory researchers. There should be room for both. The US Geological Survey will certainly have highly applicable expertise.

A climate remediation program should also provide money to investigate issues such as the possibility of putting out coal mine and peat fires that continually burn underground and emit large amounts of CO2 and other greenhouse gasses. With the demise of the US Bureau of Mines, there is no clear place for this research, but might be best done through the Mine Safety and Health Administration (MSHA). Biological methods of remediation might include genetically modified organisms (GMO) that would raise governance issues. Early stage research would likely be covered under existing review and governance mechanisms in place by NIH or NSF for other GMO research. Any large scale experimentation would also raise governance issues similar to those associated with climate interventions which are discussed below. Simlarly, ocean iron fertilization methods have governance issues similar to climate intervention methods and may also be governed by existing treaties such as the London Convention or the Law of the Sea.

Climate intervention

Climate model simulations have shown that it is possible to change the global heat balance and reduce temperatures on a global basis very quickly with aerosol injection in the stratosphere for example. We also have experience with natural analogues in the form of volcanic eruptions which emit massive amounts of sulfates that cause colder temperatures for months afterwards. So we have a pretty good idea that some methods could be effective at reducing global temperatures.

Climate intervention techniques include a variety of controversial methods aimed at changing the heat balance of the atmosphere by either reducing the amount of radiation reaching the Earth or reflecting more into outer space. The common features of these technologies are that they are inexpensive (especially compared to mitigation), they are fast acting, and they are risky. Some could lower temperatures within months of implementation, but they do not "solve" the problem in that they do nothing to reduce the excess greenhouse gases in the atmosphere. So, if we reflect more sunlight and don't reduce CO2 in the atmosphere, the oceans will continue to acidify, severely stressing the ocean ecosystems that support life on Earth. And if we keep adding CO2 the atmosphere we will eventually overwhelm our capacity to do anything about it with geoengineering intervention. So, climate intervention cannot be a stand-alone solution. It is at best only a part of an overall strategy to reduce atmospheric concentrations of greenhouse gases and adapt to the unavoidable climate change coming down the pike. Climate interventions are unlikely to be deployed until or unless we become convinced that the risks of climate change plus climate intervention are less than the risks of climate change alone.

There are ideas for putting reflectors in space and increasing the reflectance of the oceans, land or atmosphere (see the Royal Society Report on Geoengineering). Some propose global interventions such as injection of aerosols (sulfate particles or engineered particles) in the stratosphere and the Novim report spells out the required technical research in some detail (J.J. Blackstock et al., Climate Engineering Responses to Climate Emergencies, Novim, Santa Barbara, CA 2009 http://arxiv.org/pdf/0907.5140). Others propose more regional or local interventions, such as injecting aerosols in the Arctic atmosphere only in the summer to prevent the ice from melting (On the possible use of geoengineering to moderate specific climate change impacts, M. MacCracken, Env. Res. Letters, 4/2009, 045107). Even more local and perhaps the most benign is the idea of painting rooftops and roadways white to reflect heat.

The more global and effective these methods, the more they harbor the possibility of unintended negative consequences which may be unequally distributed over the planet and extremely difficult to predict. We can expect few if any unintended consequences from painting roofs white, the benefit will be real and a cost-effective part of our arsenal. However, this action alone is not enough of an intervention to hold back runaway climate change. On the other hand, we could reverse several degrees of temperature rise by injecting relatively small amounts of aerosols in the stratosphere (because a few pounds of aerosols will offset the warming of a few tons of CO2), but it may be difficult to predict exactly how the weather patterns will change as a result. Although the net outcome may be positive, certain regions may experience deleterious conditions. It will be very difficult to determine whether these deleterious conditions arise simply from climate variability or are due to the intentional intervention. In general, methods with high potential benefits also have higher risks of unintended negative consequences.

Climate intervention might be part of an overall climate strategy in ways and with difficulties that we have only begun to contemplate. Climate model simulations have shown that if we were to suddenly stop a global intervention, then the global mean temperature will quickly return to the trajectory it was following before the intervention. This means that temperatures could increase very rapidly upon cessation of the intervention which would likely to be devastating. Climate intervention may only provide temporary respite, and ironically would be difficult to stop. However, we already emit millions of tons of aerosols now in the form of air pollution which is masking an unknown amount of global warming, perhaps as much as 5 or 10 degrees C. So, as we clean up this air pollution to protect human health or stop emitting air pollution as we shut down coal-fired electricity generation in mitigation efforts, we will also cause a significant increase in short-term warming. (Long term warming remains largely a function of the concentration of CO_2.) We may want to offset this additional warming by injecting some aerosols in the stratosphere where they are even more effective at reflecting radiation. This plan might cause much less acid rain and improve human health impacts compared to the power plant and automobile emissions while continuing to mask undesirable warming. It is possible that the "drug" of aerosol injection could be a type of "methadone" as we withdraw from fossil fuels.

Beyond technical problems, international strife is possible. State or non-state actors may think it is in their interest to deploy geoengineering without international consensus. Could a country suffering from climate change see a benefit to the technology and not have sufficient concern with disrupting the rainfall in other countries? Any indication that a nation is doing research solely to protect their national interests will be met with appropriate suspicion and hostility. On the other hand, the possibility of reaching of global consensus to deploy these technologies seems utterly impossible. Who gets to determine what intervention we deploy or even what the goal of the intervention should be?

Climate intervention techniques offer tremendous potential benefits to life on Earth, at the same time they are hugely vulnerable to mismanagement and may have severe and unacceptable unintended consequences and risks. For all these reasons, practically no one thinks we should deploy these technologies now if ever and, we should remain skeptical and appropriately fearful of deploying these technologies at any point in time. But many, including me, think we should gain knowledge about them in a research program simply to inform better decisions later and to be sure we have explored all options in light of the enormity of the threat. It would be especially better to know more about what could go wrong and what *not* to do.

In light of these concerns, how should a research program proceed?

The nature of research into climate intervention may call for a focus on public management rather than private sector motivation. There is much at stake – literally the future of the planet. There are distinct problems with letting companies with vested financial interests in intervention technology have a say in the intervention choices we make. For example, when California decided it no longer had to dig up old leaking gas tanks because the bacteria in the soil were able to remediate the contamination if just left alone (intrinsic remediation), the industry that served to dig up leaking gas tanks fought the ruling. Not digging up the tanks was in the interest of society, but the industry was concerned with its financial future. We do not want to place the deliberations about how to modify the climate in a profit making discourse. The role of the private sector and public-private partnerships should be carefully constructed to avoid these problems.

The United States Government should make it absolutely clear we are not planning for deployment of climate intervention technology. Many serious people worry that geoengineering will form a distraction from mitigation. Many are worried because they do not see the societal capacity to make mitigation decisions commensurate with the scale of the climate problem. Others find the very thought of geoengineering abhorrent and unacceptable. However, many people who are against deployment are in favor of research. By making it clear we are not planning to deploy we can take some of the political pressure off the research program and allow more room for honest evaluation.

A very good example of how this might work can be found in the Swedish nuclear waste program. In 1980, Sweden voted to end nuclear power generation in their country in the early part of the 21st century. Then, they began a program to build a repository to dispose of nuclear waste. Opposition to the nuclear waste program was not saddled by the question of the future of nuclear power. The program proceeded in an orderly manner and with extensive public interaction and consultation focused narrowly on solving the nuclear waste problem. They jointly developed a clear *a priori* statement of the requirements for an appropriate site before the site was chosen. Today, Sweden has chosen a repository site which is supported by the local population and is scientifically the best possible site in Sweden. (In contrast, the goal of the American policy was to show that we could store waste in order to have nuclear power, the repository site was chosen by Congress without public consultation. Astonishingly, the site criteria were established after the site was chosen. In the end we do not have a successful nuclear waste storage program. See J. C.S. Long and R. Ewing, Yucca Mountain: Earth-Science Issues at a Geologic Repository for High-Level Nuclear Waste, Annual Review of Earth and Planetary Sciences, Vol. 32: 363-401 May 2004) Likewise for geoengineering, a perception that the purpose of the research program is to plan deployment would saddle the research program with needless controversy. We should be careful to state we are not planning deployment.

Second, as in the Swedish nuclear waste program, we should embed public engagement in the research program from the very beginning. I will discuss science and public engagement from three perpectives: national governance, international interactions, and the requirement for adaptive management.

National research governance:

In constructing a national research program, we have to be concerned with these questions:

1. What constitutes appropriate levels of governance for specific types of research?
2. What are the guiding principles and values that will be used to sanction research?
3. Given these principles, what process will be used to sanction proposed research?
4. How will the governance process engage society?

Types of research

One of the truly difficult problems in climate intervention research has been pointed out by Robock et at (Science 29 Jan 2010, Vol 327, p 530). Namely, it is not possible to fully understand how a specific technology will work on a global scale, over extended periods of time without actual deployment. But we certainly would not want to deploy an intervention without understanding how it works first. We cannot plunge into deployment, so how should research proceed?

The first key point is that there are many types of research that require no new governance. For example computer modeling studies that simulate proposed interventions are clearly completely benign. On the other hand, a proposal for full- or even sub-scale deployment with non-trivial effects would clearly require a very high level of scrutiny. So, the first task is to determine the scale and intensity of experimentation below which research can proceed with impunity. What amount of perturbation, reversibility, duration, impact, etc falls squarely within the existing bounds of normal research? I will call this the "bright line," even though in practice the line is likely to be fuzzy and the characterization of this line is likely to be difficult to express quantitatively. Never-the-less, if research falls under the bright line, essentially no new governance is required.

There is no single bright line for all proposed climate intervention research; the nature of the "bright line" is technology dependent. Although the types of questions might be similar, the specific questions we would ask about aerosol injection in the stratosphere are completely different than the questions we would ask about putting small bubbles on the surface of the ocean. So, when a technology is sufficiently mature to be seriously considered for expanded research, it will become necessary to understand the bright line for that technology. The process and deliberation used by the National Academy of Sciences/ National Research Council (NAS/NRC) is ideal for determining this bright line. They assemble a panel of experts, take testimony, and opine on complex scientific and social issues. Two of the technologies currently under discussion, aerosol injection in the atmosphere and cloud brightening, have probably reached this level. An NAS/NRC panel should be convened now to determine what research projects in these two technologies can proceed with "normal" governance.

More difficult is the area of research above the bright line. The National Environmental Policy Act (NEPA) mandates federal agencies to prepare an Environmental Impact Statement (EIS) for any major federal action that significantly affects the quality of the human environment or to conduct an Environmental Assessment when the effects of the proposed action are uncertain. These and other environmental laws and regulations may directly affect above the line research. Beyond these environmental laws, governance principles and procedures are yet to be developed.

Nanotechnology has attributes in common with climate intervention research. There is great promise but risks that are hard to quantify. How will nano-particles behave in the environment? Will they disrupt natural processes in a way we cannot predict? One approach has been to fund research on the toxicology of nano-particles to find out what might wrong. At least part of a climate intervention research program should be dedicated solely to understanding the potential negative impacts and what might go wrong.

Principles

For research that rises above the bright line, there is a lot to be learned from examining other research governance principles and practices. Human subjects research is particularly apropos. The Nuremburg trials after WWII revealed horrendous medical experiments on human subjects by Nazi "doctors". America's shameful history of research on syphilis in the 1960s and 1970s which horribly mistreated the Tuskegee airman and subjected them to unimaginable suffering is another salient reminder of how dangerous experiments may be when detached from appropriate moral and ethical guidelines. These experiences led to a commission charged with providing guidance for future research governance. The Belmont report written by this commission lays out principles which must be met in order to sanction proposed research where humans are the subject of the research. (From Wikipedia http://en.wikipedia.org/wiki/Belmont_Report: The Belmont Report is a report created by the former United States Department of Health, Education, and Welfare (which was renamed to Health and Human Services) entitled "Ethical Principles and Guidelines for the Protection of Human Subjects of Research," authored by Dan Harms, and is an important historical document in the field of medical ethics.) The principles are quite basic and we can easily see how they might translate to principles that might apply to "Earth subject" research.

The three fundamental principles of the Belmont report are:

1. respect for persons: protecting the autonomy of all people and treating them with courtesy and respect and allowing for informed consent;
2. beneficence: maximizing benefits for the research project while minimizing risks to the research subjects; and
3. justice: ensuring reasonable, non-exploitative, and well-considered procedures are administered fairly (the fair distribution of costs and benefits to *potential* research participants.)

These principles stimulate a good discussion of possible governance principles for geoengineering. For the first principle, there are really two parts, respect and informed consent. The respect part probably translates to "Respect for all persons of the planet." Geoengineering research should not be frivolous, or dismissive of human life. As well, life other than human is also an issue, so perhaps this principle translates to "respect for life on Earth". Does the proposed research exhibit respect for life on Earth?

The informed consent principle is perhaps the most important and most vigorously evaluated principle in human subjects research review. Proposals are rejected based on obfuscation of the research methods. For example, a proposal for research on child molestation was recently rejected. The proposer told parents he would be playing a game of Simon Says with the children. What the proposer failed to tell the parents was that he would ask the children to do things like "suck my thumb". The proposal was denied based on lack of informed consent. The message here is that the researcher obscured the procedure in order to get consent from the parents. What is the moral equivalent of informed consent for geoengineering research? I think it is at least in part that the proposal methods, plans, analysis and even engineering should be open and transparent. We might ask researchers for specific actions to make their work transparent and collaborative. Say posting on a specific website, or advertisements in new media. Beyond this, it is not possible to get the informed consent of all life on Earth or even all countries. The question will be who is informed and who has to consent? How will the public and the democratic process be involved? These are matters for public deliberation.

The beneficence principle applies essentially without change. It is perhaps the most straightforwardly applicable of the three. The benefits of the research should outweigh the risk of unintentional harm to life on Earth. . The research must be aimed at accomplishing a benefit and must not intentionally do harm. To demonstrate this, proposers should take actions such as modeling their results, evaluating natural analogues, assessing potential impacts, and other due-diligence measures that, in the end, must be evaluated by judgment in review. Again the question is who reviews? Who gets to sanction the research? We can examine the review process used for human subjects and other controversial research and learn more about what we should do for climate intervention research.

The third principle, justice, requires somewhat different articulation for geoengineering, but the basic ideas apply. The intent of this principle is to avoid experiments that take unfair advantage of a class of vulnerable people (prisoners or children for example) for the benefit of others. In the case of Earth subject research, the issue might be this: does the proposed activity sacrifice the interests of one group of people for the benefit of everyone else? I would think that at the research level, the answer to this question should be categorically "no", the research does not gain information about a proposed method at the expense of vulnerable populations. Proposers could be required to show how and why they expect their research to be fair. The problem will become more difficult as research reaches subscale or full scale deployment. If some parts of the Earth are harmed by the intervention, will there be compensation, how much and from whom? How will causation be established? Worse, is it fair to deprive some countries of the right to choose the temperature? These questions themselves must be topics for research and public deliberation.

There are of course major differences between the ethics governing medical research on human subjects and Earth subject research. One of the most interesting is that the need for research governance is diminished over time for medical research. Eventually, if the research is successful, protocols with statistical results to support them are obtained. The research results can be used to set standards of practice and the ethics become ethics of normal medical practice. The need for research review declines with time. In the case of geoengineering the research aspects are likely to continue indefinitely, and may become more acute with time. We cannot do double-blind studies. We cannot have a statistical sample of Earths. At some level, geoengineering, will always be research and always require research-ethics type governance. And the worst case from a risk perspective is actual implementation. Whereas in medical research, the need for governance subsides over time, for geoengineering, governance will get more and more pronounced over time, until or unless the idea is abandoned.

Review Process

In human subjects research, Institutional Review Boards (IRBs) are vested with the authority to review and sanction research. These boards review the research protocols and procedures to insure they meet ethical standards. If the IRB approves the research, then the institution is free to allow the research to be conducted. If the IRB disapproves, the institution may not conduct the research as proposed. The IRB cannot decide that the research will be done, only that it *may* be done. IF the IRB disapproves, the institution must comply with the ruling and cannot allow the research to continue.

There are perhaps three salient features of the IRBs that control the outcomes. First, they are part of the research institution. They are not an external body. However, once appointed, they are independent. Second, their rulings are not based on specific regulations. They are based on principles which are derived mainly from the Belmont report. Third, the board membership is defined by federal code: http://www.accessdata.fda.gov/scripts/cdrh/cfdocs/cfCFR/CFRSearch.cfm?fr=56.107. This guidance specifies that each IRB must have at least five people, members must include those qualified to review the research and members from the community. So, it is the principles and the board appointments that insure the quality of the IRB decisions.

It is notable that IRB's from around the country meet regularly together and present prior cases without revealing their ultimate decisions until after the cases are discussed. Then the board that presented the case reveals the decision they actually made. In this way, the boards gain insight and skill at making difficult rulings. The point is, their rulings are not prescriptive, they are based on judgment and good judgment requires learning.

The IRB's have public members in order to protect public interests. Even so, dissatisfaction with this process arises from a sense that IRBs end up rubber-stamping research protocols, do not deliberate conflict of interest issues, and do not engage in any real public dialogue about values. Consequently, researchers and social scientists are experimenting with new models to engage the public in human subjects research.

Given the problems with governance of human subjects research, it would be wise to develop a program that seeks to propose and test research governance and engagement models. One of the best ways to learn about what works is to go through exercises in mock governance. For example, an institution or project could try out a governance process in a "moot court" type trial such as this:

- A draft set of guiding principles for research is given to blue and red teams. They might start with the principles outlined above for example. Both teams should include scientists, but also might include members of the public or social scientists.
- Blue teams would prepare mock (or real!) research proposals for geoengineering field tests and gives these to the red teams. For example, a team may propose an Arctic sulfate injection or mid ocean for cloud whitening trial.
- Red teams prepare critiques of the blue team proposals. The job of the red team is to try to find the weaknesses in the blue team proposal and bring these to light.
- Both teams present the research and critique respectively to a mock review board at the meeting following the draft guidelines/principles. . We might choose the people for the mock board as a mix of scientific backgrounds and a strong mix of public interest members as well as ethicists or philosophers – ie far beyond the IRB membership as specified in the federal statute.
- The mock board uses the draft principles to evaluate the proposals. They could issue a mock ruling to sanction the research, turn the proposal down, or perhaps recommend additional measures for due diligence.

- Everyone discusses the process – did the principles cover the important issues? --- was the process appropriate? How might the process go wrong? The goal should be to identify all salient lessons learned.
- Do this again changing the process as appropriate.

Another set of exercises are being tried in the field of nanotechnology research to incorporate the values of society. David Gustin, for example, describes experiments in “anticipatory governance” (Gustin, Innovation policy: not just a jumbo shrimp, Nature, Vol 454/21, August 2008). There are three parts to this process. The first part is designed to educate the public about the nature of the research and to bring public deliberation of values into the open. The second part is to have scientists and the public collaborate on imagining how the future might unfold given new technology and social trends. Gustin calls this “anticipatory knowledge”. Discussions then give voice to public concerns about the future. Finally, the public engagement and anticipatory knowledge are integrated with the research. For example, social scientists and humanists have become “embedded” in nanotechnology research labs. They help the scientists reorient their work in more socially acceptable directions. This could also be a very good model for geoengineering. It would be possible to create a geoengineering forum where publics could be informed and express concerns. Exercises that highlight the possible futures with and without geoengineering would help all to understand how we should focus. Finally, keeping social scientists are part of any scientific research team may help with both guiding the research towards more socially acceptable directions and also help scientists with communication and outreach.

There is no absolute clear answer to the question how to govern geoengineering research. The fact is that we need research and experimentation to understand how to govern this research, *ie* research and experimentation on how to govern research with public engagement. It is likely that research governance models will be different for different types of technologies and there will not be a one-size-fits-all governance model. As technologies reach the stage of research that approaches the “bright line”, specific governance models should be explored and evaluated.

International governance:

Geoengineering research has the potential to cause international conflict. Tensions could easily rise if countries perceive that the research is being conducted solely for national interests. If geoengineering research programs became part of defense research programs, it would certainly convey the message that the goal was to advance national interests. Consequently, research programs should explicitly only develop technology that will have international benefits. Research should not be managed by national defense programs (J. J. Blackstock and J. C. S. Long, The politics of Geoengineering, Science, Vol 327, p. 527, 29 Jane 2010.)

Secrecy also has the potential to create tension and conflict. It is important that geoengineering research be conducted in the open with results published in the open literature. Especially in the early stages, a pattern of trust and consultation will be critical to a future that might well require agreement and collaboration. Inclusion of international scientists in a national research program or the establishment of international research programs would have tremendous benefits in both expanding the knowledge base and as an investment in future collaboration.

In starting down a research path, we must remember that critical decisions about deployment may be needed someday and that these decisions should not be made unilaterally. We should be extremely careful not to increase tensions or misperceptions that would make these decisions even harder. On the other hand, there is less and less confidence that all affected nations would ever be able to come to an agreement and sign a treaty to support a single set of actions. Such a treaty may still be our goal, but there are other strategies that can help us to make good choices together. I am fond of a quotation from the famous French sociologist, Emil Durkheim in which he noted: "Where mores are strong, laws are unnecessary. Where mores are weak, laws are unenforceable." In that spirit, we may hope that good cooperative relationships in geoengineering research and research governance may help to develop common norms of behavior and it may be these norms that provide the capacity to make good collaborative decisions in the future.

Adaptive management

Climate is a complex, non-linear system with many moving parts. When we set about to intentionally intervene in climate outcomes, there will always be uncertainty about whether our chosen actions will result in the desired outcomes. An essential feature of any climate intervention will be the need to provide for adaptive management, also known as "learning by doing". If we are to use adaptive management in a climate intervention it means that we

1. Choose to make an intervention,
2. Predict the results of the intervention,
3. Monitor the results of the intervention,
4. Compare the observations to the predictions,
5. Decide if we are going in the right direction and
6. Make a new set of decisions about what to do.

(See http://en.wikipedia.org/wiki/Adaptive_management.). In the real world it is very hard to actually do adaptive management.

First, it is difficult enough to make a decision to act. To then change this decision becomes confusing and politically negative. Consequently, successful adaptive management establishes a structure for the adaptive modification *a priori*. So, regular intervals and formats are established for comparing observations with predictions and a formal requirement is put in place for deciding whether or not and when to change directions. When this process is specified up front, it can avoid the political fallout of changing direction. Part of a geoengineering research program should examine the potential policy and institutional frameworks for conducting adaptive management. In particular it is important to determine *a priori* how the technical and political parts of the process will interact. Will the deciding entity be a board made up of scientists and policy makers and perhaps members of the public and social scientists?

Or should we structure a hierarchy of decision makers where higher level boards have decisions about overall direction, but less control of specifics?

Second, you must have a very good data base of observations. If you haven't made extensive observations all along, how will you be able to detect what is changing? This is not just a problem for geoenineering, but for all of our climate strategies. The observation network we have for climate related data is far too sparse and in some cases, inadequately calibrated. We need a major commitment for all our climate research to collecting and calibrating data relevant to climate change on a continuous, ubiquitous basis and perpetual basis. This is a *sine qua non* recommendation for any climate solution. We cannot rewind the tape and go back to collect data that we failed to collect over time. The observation network for climate is inadequate to our needs and this is an extremely high priority for research dollars.

Third, you must be able to discern whether a change is attributable to simple climate variability or to the specific intervention. The science of detection and attribution of human effects on climate has advanced tremendously in the past decades. But the challenge of detecting and attributing changes to intentional, fairly short term interventions has not been met. This must be a focus of research. As it is strongly related to the existing climate science program, the expanded work belongs there.

In the simplest terms, the scientific approach to attribution of human induced climate change – whether through unintentional emissions or intentional climate intervention -- is to use climate models to simulate climate behavior with and without the human activity in question. If the results of the simulations including the activity clearly match observations better than the results without the activity, then scientists say they have "fingerprinted" the activity as causing a change in the climate. Perhaps the most famous illustration in the International Panel on Climate Change (IPCC) reports shows two sets of multiple model simulations of mean global temperature over the twentieth century, one with and the other without emitted greenhouse gases. On top of this plot, the actual temperature record lines up squarely in the middle of the model results that included greenhouse gas emissions. This plot is a "fingerprint" for human induced warming. Scientists have gone far beyond mean global temperature as a metric for climate change. Temperature profiles in the atmosphere and ocean, the patterns of temperature around the globe and even recently the time of peak stream flow have been used to fingerprint human induced warming.

Structured climate model intercomparison projects are fundamental to drawing fingerprinting inferences. No single model of the climate gets it all right. Each climate model incorporates slightly different approaches to approximating the complex physics and chemistry that control climate outcomes. So, we use multiple models all running the same problems. We can then examine a statistical sample of results and compare this to data. In a form of "wisdom of the crowd", the mean of all the model results has proven to be a better overall predictor of climate than any single model.

The science of fingerprinting is becoming more and more sophisticated. Increasingly, scientists are looking at patterns of observations rather than a single number like mean temperature. Pattern-matching is a much more robust indicator of causality because it is much harder to explain alternative causality for a geographic or time-series pattern than for a single value of a single parameter. A famous example of this was discerning between global warming caused by emissions versus caused by a change in solar radiation. Solar radiation changes could not account for the observed pattern of cooling of the stratosphere occurring simultaneously with a warming of the troposphere, but this is exactly what models predicted for emission forced climate change. There exist "killer metrics" like this that tightly constrain the possible causes of climate observations.

We are making progress on the "holy grail" of using present observations to predict future climate states. Recently, Santer et al showed that it possible to rank individual models with respect to their particular skill at predicting different aspects of future climate. Interestingly, the models fall into groups. The top ten models that get the mean behavior right are different than the top ten models that get the variability right. (Santer et al.,PNAS 2009, Incorporating model quality information in climate change detection and attribution studies, http://www.pnas.org/content/106/35/14778.full?sid=e20c4c31-5ab1-4f69-b541-5158e62e4baf).

Some think that the ability to detect and attribute intentional climate intervention will be nearly impossible. The fingerprinting of human induced climate change has been based on decades of data under extremely large human induced perturbations. For climate intervention, we contemplate much smaller perturbations and would like proof positive of their consequences in a matter of years. Even though this is clearly a big challenge, it is not hopeless. Neither should we expect a panacea. We will be able to identify specific observations that certain models are better at predicting and we will be able to find some "killer metrics" that constrain the possible causes of the observations. In some respects, conclusive results will not be possible and we will have to learn how to deal with this. Fingerprinting – detection and attribution of human intervention effects on climate -- must be an important area for research if we are to be able to conduct adaptive and successful management of geoengineering. As this topic is closely interconnected to basic climate science, the program to extend research into intentional intervention should belong in the US Climate Science Program.

A geoengineering research program should include the development of technology and capacity for adaptive management.

The "Catch-All" Category

Recent studies have shown vast amounts of methane, a powerful greenhouse gas, are leaking from the Arctic Ocean floor. Billions of tons of methane are stored in permafrost and will be released as the frozen lands thaw. Methane is a green house gas that is approximately 25 times more powerful than CO_2. Abrupt increases in methane emissions have been implicated in mass extinctions observed in the geologic record and could trigger runaway climate change again. (It is the possibility of such runaway climate change that most clearly supports the need for geoengineering research.) James Cascio recently posed an idea for deploying genetically engineered methanotrophic bacteria (bacteria that eat methane) at the East Siberian Ice Shelf (http://ieet.org/index.php/IEET/more/3793/). Is this possible? Could bacteria survive in the Arctic? Could they eat the methane fast enough to make a difference?

What are the risks? Could release of genetically modified methanotropic organisms cause problems to the Arctic ecosystems? Is the idea worth pursuing? This may be an idea with merit –or it may be a very stupid idea.

Somewhere in the geoengineering research program there should be funding to freely explore theoretical ideas and perform the modeling and laboratory studies to determine which concepts are worthy of more work, and which are completely impractical or too dangerous. This should be a "gated" research program wherein small amounts of funding are provided to explore many out-of-the-box ideas with thought experiments, modeling and laboratory experiments as appropriate. At this stage, none of the research ideas should require more than traditional governance mechanisms provided by existing research programs. At the end of this initial funding, the concepts would have to be reviewed and if they are deemed to have promise, then they would become eligible for more funding. If the ideas are found to be lacking in merit, then they would be shelved. Several stages or gates should be set up with increasingly higher bars so that a large number of ideas can be generated at the first gate, but these are increasingly winnowed down as we learn more about their practicality, dangers and effectiveness.

Beyond this "bottom-up" approach, there should be a "top-down" research program that examines potential emergencies that could result from climate change and then attempts to design interventions for these specific situations. The primary climate interventions currently under discussion attempt to reduce temperature. Although higher temperatures that result from climate change will be a severe problem, I would argue that other impacts of climate change might be more critical. For example, one of the major impacts of climate change will be increased water stress – we will need more water because it is hotter and there will be less water because there will be more droughts. Water shortage will lead to problems with food security. A choice to control temperatures with aerosol injection for example might result in reduced precipitation. Volcanic eruptions such as Pinatubo provide a natural analogue for such aerosol interventions. Gillett *et al*. were able to show that a result of these eruptions caused a reduction in precipitation (*Gillett, N.P., A.J. Weaver, F.W. Zwiers, and M.F. Wehner, 2004: Detection of volcanic influence on* global precipitation, Geophysical Research Letters, 31, doi: 10.1029/ 2004GL020044.). So, we might reduce temperatures with aerosols, but make hydrological conditions worse. Reducing precipitation would clearly be a bad thing to do. By looking only at what we know how to do (reduce temperatures) *vs* what problem we want to solve (increase water supply), we could be making conditions worse. Geoengineering research should not only be structured around "hammers" we know about. We should also collect the most important "nails" and see if we can design the right hammer.

Thus, we might try to develop methods that directly attack specific climate impacts. Can we conceive of a way to control the onset, intensity or duration of monsoons to ensure successful crops in India? Can we conceive of a way to stop methane burps, or hold back melting glaciers? Some part of a geoengineering research program should take stock of the possible climate emergencies and then look for ideas that would ameliorate these problems.

Conclusions

The above comments describe a number of measures we might take in establishing a geoengineering research program. If we are to have a successful research program we must be careful about public engagement, principled actions, transparency, international interaction and adaptive management. We will have to build the capacity to develop rational options coupled to the capacity to make rational decisions about deploying them. If we succeed, it may be that these capacities spill over into other difficult climate problems. We may ask in the end: Are we building the capacity to do geoengineering or using geoengineering research to build capacity for any climate solution? If we are lucky, the answer will be the latter.