challenge paper

CLIMATE CHANGE, CLIMATE ENGINEERING R&D

James Eric Bickel
Lee Lane

COPENHAGEN CONSENSUS 2012
solving the world's challenges

Climate Change: Climate Engineering Research

J. Eric Bickel[1]

Graduate Program in Operations Research

The Center for International Energy and Environmental Policy

The University of Texas at Austin

Lee Lane[2]

Hudson Institute

May 2012

Acknowledgements

The authors thank Samuel Fankhauser, Anil Markandya, and Kasper Anderskov for their comments on a earlier draft of this paper.

---

[1] Graduate Program Operations Research and Industrial Engineering, The University of Texas, Austin, Texas, 78712-0292, Tel: +1 512.232.8316, Fax: 512.232.1489, ebickel@mail.utexas.edu

[2] Hudson Institute, 1015 15th Street, N.W., Washington, DC 20005, Tel: +1 202.974.2401, llane@hudson.org

Introduction

This paper seeks to answer a question that has been posed as part of the Copenhagen Consensus 2012 (CC12) exploration of global policy. That question is:

> "If the global community wants to spend up to, say, $75 billion over the next four years to do most good for the world, which solutions would yield the greatest net benefits?"

To address this question, we agreed to update our Copenhagen Consensus 2009 (CC09) paper (Bickel and Lane 2010), hereafter BL10. That paper estimated the net benefit of a research and development (R&D) program to explore the safety and efficacy of climate engineering (CE). The current paper extends those estimates. BL10 considered two different CE approaches, solar radiation management (SRM) and air capture. In this paper, however, we restrict our attention to SRM. The paper is intended to be self-contained. The interested reader will, however, find many supporting details and further discussion in BL10.

We begin by first acknowledging that the potential benefit of SRM is so obvious that one hardly needs a formal economic assessment to prove that researching its merits could pay large dividends. The logic is simple: if global warming will cause large damages and require costly abatement measures, then having a relatively low cost SRM technique to offset warming, even partially, would pay large dividends. Furthermore, initial studies estimate the cost of an SRM R&D program as being on the order of a billion dollars. This sum is a small fraction of the CC12 budget. It is an even smaller fraction of what the United States alone is spending on climate-change research each year.

Thus, we believe that the case for including SRM R&D in a portfolio of responses to climate change is strong. Others, such as the Royal Society, agree (Royal Society 2009). Yet, the CC12 process requires numeric benefit-cost ratio (BCR) estimates. A truly comprehensive benefit cost analysis of R&D into SRM would require quantifying many factors that are highly uncertain. Such an analysis might create the illusion of rigor, but its extreme complexity would be more likely to obscure the policy choices at hand than to clarify them. We have therefore not carried out the most technically detailed analysis that we could imagine. In fact, as discussed below, we decided not to perform a "value of information" or an "options analysis". We made this choice precisely because we believe that given the current state of knowledge, such analysis would have offered very little in the way of additional insight. Thus, our SRM R&D BCR estimates are necessarily incomplete. We offer them in hopes that initial R&D will produce the new knowledge needed for more refined analysis.

While R&D into SRM might produce some useful spinoffs its main value clearly depends on the possibility that SRM might actually be deployed. Thus, our BCRs necessarily depend on the estimated benefits of using SRM. These estimates, in turn, hinge upon the way in which SRM may be used and on how events might unfold were no SRM capability developed. Certainty about such matters is impossible; hence we explore the benefits and costs of using SRM across a number of disparate policy options and climate futures.

Our previous paper, BL10, looked at the impacts of deploying SRM with economically-efficient greenhouse gas (GHG) controls, no controls, controls aimed at achieving a temperature cap of 2°C, and Stern-Report-like controls. It also compared the impacts of earlier and later deployment of SRM.

This paper broadens BL10 in two important ways. First, it extends our modification of the DICE-2007 model (Nordhaus 2008) to include the possibility of SRM-caused climate damages and determine how large these damages would have to be for SRM deployment to incur net costs. Second, we focus on using SRM to avoid severe harm from climate change. As a framework for this analysis, we assume that states might use SRM in conjunction with GHG controls to limit temperature changes to no more than 2°C.

Thus, between the two papers, we have used a widely-cited integrated assessment model to examine futures in which climate policy, climate change damage, discount rates, SRM start dates, and SRM side-effects all vary. Across these diverse scenarios, one finding remains constant: a workable SRM option would produce very large net benefits.

Summary of Findings

We continue to support the creation of a formal SRM R&D program, endorsed by the CC09 expert panel (Stokey et al. 2010). We roughly estimate that the BCR of such R&D is on the order of 1000 to 1. The following findings support this recommendation:

1. SRM holds the potential of reducing the economic damages caused by both warming and costly abatement measures. The early costs of GHG controls tend to be higher than those of climate change; so by lessening the stringency of controls, SRM may provide *near-term benefits*—compared to strategies relying exclusively on either emissions reductions or unchecked climate change.

2. Current climate policy efforts and existing R&D programs fail to address directly the single largest risk we face: uncertainty about climate sensitivity. Indeed, our current plan seems to be one of hoping emissions can be reduced and praying the climate sensitivity is not too large. This approach is worrisome. The uncertainty surrounding this parameter is almost an order of magnitude (IPCC 2007). This large range of uncertainty implies on the one hand, a risk of paying too much for GHG abatement should actual climate sensitivity be on the low end of the possible range. On the other hand, it implies high climate damages and maybe large abatement costs should actual sensitivity be high. In fact, SRM appears to be the *only technology that could quickly cool the Earth* should the need arise to do so. This feature would allow it to *play an important risk management role* despite this so far intractable source of uncertainty.

3. SRM use may cause climate damages, but, in order to negate its benefits, these damages would need to be at least as costly, and potentially twice as costly, as those from unchecked climate change. In other words, *SRM need not be damage free* because its use could possibly offset climate damages and abatement costs. The latter are also not damage free.

4. *Relatively small amounts of SRM appear to be able* to meet the 2°C increase target. To meet this goal, SRM need not be deployed until 2075. Through the end of this century, SRM would require less than about 1.4 Watts per square meter (W m^{-2}), or 0.75 terragrams of sulfur (Tg S) per year. As a point of reference, the IPCC (2007) estimates that aerosols are *currently* offsetting about 1.2 W m^{-2} of the forcing caused by the increase in greenhouse gas concentrations; this amounts to about a 40% reduction in forcing. In addition, human activity presently injects about 55 Tg S per year into the atmosphere (Stern 2005).

5. The *net benefit of SRM*, even after accounting for possible damages, *is on the same order as a technology that could costlessly eliminate all CO2 emissions over 80 years*. Given that annual "clean energy R&D," currently totals about $15 billion globally (Chiavari and Tam 2011), it seems reasonable to believe that SRM deserves some modest formal research funding.

The remainder of this paper expands upon these findings. It is organized as follows. In the next section, we discuss the rationale for and possible necessity of climate engineering. We then briefly summarize the most recognized SRM technologies. After that section, we discuss the feasibility and potential cost of a climate engineering research program. With this background, we then frame the SRM

R&D decision, compute the possible net benefits of SRM deployment, and the possible returns to R&D investment. Finally, we discuss our results and conclude.

The Need for Climate Engineering Research

Greenhouse gases (GHGs) in Earth's atmosphere, such as carbon dioxide (CO2), methane, and water vapor, cause the planet's surface to be about 30°C warmer than would otherwise be the case (Stocker 2003). All else being equal, although all else may not be equal, higher GHG concentrations will raise global mean temperatures (IPCC 2007). Higher temperatures, and climate changes that may follow in their wake, are likely to lead to a mix of costs and benefits. As warming proceeds, its net effects will grow more negative.

CO2, once in the atmosphere, will remain there for a century or more. Attempts to abate GHG emissions are also subject to long time lags. They will, for one thing, demand far-reaching changes in technology. Developing much of that technology, according to U.S. Secretary of Energy Steven Chu, must await the appearance of multiple major breakthroughs in basic science (Broder and Wald 2009).

CC09 authors Galiana and Green (2010) argue convincingly that the Intergovernmental Panel on Climate Change (IPCC) has underestimated the scale of this challenge. Pielke, Wigley, and Green (2008) show that the IPCC's GHG emissions scenarios assume major reductions in carbon emissions, absent any climate policy. By doing so, they understate the scale of the changes that must occur. For example,

> "The median of the reference scenarios considered by the IPCC AR4 requires 2,011 gigatonnes of carbon in cumulative emissions reductions to stabilize atmospheric carbon-dioxide concentrations at around 500 parts per million…*This scenario also assumes that 77% of this reduction occurs spontaneously*, while the remaining 25% would require explicit policies focused on decarbonization [emphasis added]."

Such major technological changes are often slow to mature, and they can take much longer still to disseminate globally (Edgerton 2007). Electrification of the global economy has been in train for over one hundred years, and it is still incomplete. That process, moreover, has advanced because large net benefits accrued to those who invested in it. Most low-GHG technologies cost more than those that they seek to replace. Government action will be required to spur their adoption.

Effective GHG controls, moreover, will require that the policy changes be nearly world-wide (Jacoby et al. 2008). GHG control requires many states to cooperate. Each of them must take costly affirmative steps. Collective action problems of this type often defeat attempts to solve them (Barrett

2003). Little wonder then that the UN talks have gone on now for twenty years without yielding a tangible fall in GHG emissions. Where emissions have from time to time declined, "underlying changes in economic structure may have played a bigger role than climate policy" (Lane and Montgomery 2008). Neither the UN climate talks in Durban South Africa nor trends in the major emitters seem to presage any near term change is this pattern.

Thus, a serious accord on GHG control is, as yet, nowhere in sight. Even if it were, a great deal of time is certain to separate the onset of serious GHG controls and climate stabilization. The slow speed with which GHG controls can take effect adds to the risk posed by rapid climate change. Should such change appear, speeding up GHG cuts would carry a hefty cost penalty (Richels et al. 2004). Even then, controls might do little to stabilize the situation.

Climate Engineering

With these challenges as a backdrop, it is easy to understand why proposals to more intensively study climate engineering are gaining adherents. Both the National Academy of Sciences in the US and the Royal Society in Britain have explored the concept. After considering potential benefits and highlighting significant unknowns, the Royal Society recommended a formal research program be undertaken (Royal Society 2009).

CE is composed of two distinct technology families: air capture and solar radiation management. We briefly cover each of the concepts here. The reader is directed to BL10 for additional detail and discussion.

Air Capture

Air capture (AC) removes CO2 from ambient air and sequesters it away from the atmosphere. The primary attractions of AC are that it (1) separates CO2 production from capture, adding flexibility and reduced CO2 transportation costs and (2) it might reverse the rise in CO2 concentrations. AC, however, currently suffers from two major defects. These flaws also affect GHG controls, but they do so to a lesser degree. The first is cost. Pielke (2009) estimates that the cost to reduce CO2 concentrations by 1 part per million (ppm) is on the order of $1 trillion. The second is the fact that CO2 removal may not be able to act upon the climate system as quickly as may be required. As Blackstock et al. (2009), hereafter B09, note:

> "Significant technical difficulties are associated with rapidly removing large quantities CO2 for the atmosphere. Proposed approaches for removing carbon dioxide from the atmosphere include concepts such as enhancing biological uptake through fertilization or chemical removal and sequestration with engineered systems. However, even if such efforts were to become economically competitive with low-carbon energy systems, the scale required to make a significant impact on the existing stock of carbon in the surface ocean-atmosphere system is so large that reducing the atmospheric carbon dioxide concentration by a significant amount more quickly than several decades is essentially impossible."

Recognizing this issue, the CC09 expert panel (Stokey et al. 2010) gave research into air capture a middling rating. For purposes of the current CC effort, research into AC more closely resembles research into carbon capture and storage and for this reason could be considered part of an energy R&D program. We will, therefore, focus the rest of this paper on solar radiation management.

Solar Radiation Management (SRM)

SRM aims at offsetting the warming caused by the build-up of man-made GHGs in the atmosphere by reducing the amount of solar energy absorbed by the Earth. At least some of the risks of global warming can, thereby, be counteracted (Lenton and Vaughan 2009). SRM would leave the GHGs themselves in the atmosphere. A recent report by prominent SRM researchers (B09), which details the possible structure of an SRM R&D program, summarizes the concept as follows:

> "The basic concept of [SRM] is to reduce the shortwave radiation absorbed [by the Earth] by reflecting more of it back to space (~30% is already reflected by the natural constituents of Earth's atmosphere and surface.) Basic considerations show that an additional 1-2% of reflectivity would balance, in net energy terms, the additional heating caused by a doubling of atmospheric CO2 concentration. The globally average longwave radiative forcing due to a doubling of atmospheric CO2 is approximately 4 W/m^2. As the globally averaged shortwave solar energy reaching the Earth is roughly 342 W/m^2, the reflection of an additional ~1% by [SRM] would roughly restore the energy balance."

Scattering this amount of sunlight appears to be possible. As B09 further note:

> "…basic technical analyses suggest that the scale of the required reduction would be within our current technological capabilities."

For example, as mentioned above, the IPCC (2007) estimates that aerosols currently provide about 1.2 $W\ m^{-2}$ of negative forcing. Past volcanic eruptions have shown that injecting relatively small volumes of matter into the stratosphere can cause discernible cooling. The 1991 eruption of Mt. Pinatubo reduced global mean temperature by about 0.5°C (Lane et al. 2007, B09).

A unique feature of SRM is that it holds the potential of acting on the climate system on a time scale that might prevent abrupt harmful changes (B09). In fact, SRM may be the only human action that can cool the planet in an emergency. As Lenton and Vaughan (2009) note:

> "It would appear that only rapid, repeated, large-scale deployment of potent shortwave geoengineering options (e.g., stratospheric aerosols) could conceivably cool the climate to near its preindustrial state on the 2050 timescale."

As detailed in BL10, several concepts have been proposed for accomplishing SRM. We briefly summarize the most promising here. The interested reader might wish to consult B10 for more detail:

Stratospheric Aerosol Injection. Paul Crutzen (2006), a Nobel Laureate in Chemistry has suggested research into stratospheric aerosol injection (SAI). With SAI, a precursor of sulfur dioxide would be (continuously) injected into the stratosphere. There, it would form a layer of aerosols. This layer would reflect sunlight. The amount of sulfur required to offset global warming is on the order of 2% of the sulfur that humans already inject into the atmosphere. (Today's injections occur mostly in the troposphere via the burning of fossil fuels.)

Marine Cloud Whitening. Marine cloud whitening has been suggested by Steven Salter and John Latham (Salter and Latham 2008, Latham et al. 2008). This technique would inject seawater in the form of a sea salt aerosol into, marine clouds. The aerosol would cause more water droplets and/or ice crystals to form in the clouds. The clouds would, therefore, become whiter and more reflective.

Climate Engineering Research and Development

While SRM appears feasible and potentially promising, many important uncertainties remain about it. B09 note that:

> "Climate engineering science and technology are in their infancy. [SRM] investigations to date have been limited to speculation, paper studies, and preliminary climate simulations of uniform [SRM] with coarse-resolution models. Targeted and directed investigations across a wide range of subjects—from basic climate science to intervention system engineering—could significantly improve our understanding and reduce uncertainty in the climate response to a given [SRM] intervention.
>
> Basic understanding is sufficient to conclude that a simple decrease of the solar constant can compensate for the increase in global average

> temperature caused by anthropogenic GHG emissions, but would not fully eliminate all impacts of climate change. For instance, temperature compensation at the regional scale would not be perfect, and other climate parameters currently perturbed by atmospheric GHGs—most notably ocean acidity—would remain largely unaffected. But beyond these basic observations, we know essentially nothing about the net combined impacts of shortwave climate engineering and elevated GHGs on a wide range of other climate and ecological parameters (e.g. regional precipitation, atmospheric and oceanic circulation, patterns of interannual variability, net ecological productivity, etc) or about the extent to which various [SRM] concepts might be optimized to address them."

In particular, the simple considerations discussed in the previous section regarding the Earth's energy balance and the globally uniform implementation of SRM

> "… ignore the distribution of energy within the climate system. As the incoming shortwave and outgoing longwave energies have different spatial and temporal distributions, the net impact of [greenhouse gases and SRM] on the climate system would not be zero. Climate features such as regional temperatures and precipitation levels, interannual variability, ecological productivity, and many others could all remain impacted. Scientific investigations of these distributional issues using observation of natural experiments, climate modeling, and potentially even field testing of [SRM] are needed to provide insight." (B09)

Research Agenda

To address this lack of understanding, B09 layout a ten-year R&D program. It is divided into two phases: (1) non-invasive laboratory and computational research and (2) field experiments. After the successful completion of Phase 2, presuming the decision to deploy was made, a third phase would follow. It would consist of monitored deployment. *Phase 1* would consist of laboratory experiments and computational modeling. Its goal would be to explore the climate response to differing levels of SRM intervention. This phase would not include any direct intervention in the actual climate. *Phase 2* would begin intentional interventions into the climate system. These interventions would be limited in their duration, magnitude and/or spatial range. They would not aim to offset increased GHG concentrations; rather, they would seek to understand SRM's efficacy. B09 estimate that elements of Phase 1 would take place over the entire ten-year period, but that field experiments would only begin in year five (see their Figure 5).

An R&D effort would seek to answer questions in three research streams. One such stream would be engineering intervention system deployment. The second would be climate science, modeling and experimenting to understand and articulate impacts. The third would be climate monitoring, detecting and assessing the actual impacts of the intervention. It is important to stress that neither we nor B09 envision this R&D effort as only focusing upon a single SRM technology. To the contrary, an important aspect of the research program would be to explore a range of SRM solutions.

A successful R&D program would enable scientists and engineers to specify four critical components of the SRM system. One of these is the material composition of aerosol particles. (For instance, making the cross section of the particles longer than the light wave lengths used in photosynthesis may limit SRM's impact to plant life.). A second system component is the amount dispersed. The Third is the geographic and vertical locations of aerosol dispersion. The fourth key component is the temporal sequencing of aerosol dispersion.

SRM R&D should also entail policy research. Such research should consider how the use of SRM might affect other major policy goals. It should encompass the effects of choices of SRM technology and the timing and extent of its use. It should study effects of disparate national preferences on the timing and use of SRM.

Regime building is a standard response to the challenge of cooperation among states (Keohane 1984). SRM policy research, therefore, should explore options for structuring an SRM regime. This research should be forward looking. That is, it should explore likely future trends in SRM technology, major states' preferences over climate, and global power balances. Such research should seek to define the conditions under which SRM deployment would align with the incentives of the ruling coalitions of the major world powers. Of course, this same question should also be posed with regard to all policies that require the participation or at least the acquiescence of many states.

Cost of an R&D Program

B09 do not estimate the cost of their proposed R&D program. However, two of that study's coauthors, Ken Caldeira and David Keith, have made such estimates (Caldeira and Keith 2010). They estimate that Phase 1 would start at $5 million per year; it would gradually ramp up to $30 million per year. Phase 2 would begin at $30 million per year; it would ramp up to $100 million per year as initial field tests began. The program's total, undiscounted, expenditure would be about $500 million. For comparison, today, the U.S. federal government is spending about $16 billion a year on climate-change science and related technologies (Higgins 2011). Caldeira and Keith's estimates of early spending slightly exceed those made by a 2001 George W. Bush Administration interagency panel on R&D for CE. That panel devised a plan based on a gradually rising budget with a total five-year cost of $98 million (U.S. DOE 2002). This program was not funded.

Given the roughness of these estimates, and erring on the side of conservatism, we assume in this paper that SRM R&D might require a total ten-year R&D investment of $5 billion—10 times the amount estimated by Caldeira and Keith. This cost increase could be seen as compensating for potential cost overruns, the R&D program running for longer than 10 years, or the fact that SRM R&D might "crowd out" other productive research (Nordhaus 2002, Popp 2004). In any case, as the reader will see, the potential net benefits of an SRM capability are about 1000 times greater than our R&D estimate. Thus, our cost estimates play almost no role in determining SRM's net benefit.

The Challenge of Assessing the Returns to SRM R&D

Five major uncertainties cloud estimates of CE's benefit-cost ratio. These uncertainties are as follows: First, how will R&D spending affect the capabilities of a future SRM option? Success in this context means that R&D would develop a functional SRM system that society would be willing to deploy. Second, how would SRM be deployed? Some proposals envision global systems, some only in the Arctic, and many other options are possible. Third, what will be the future state of the climate, both with SRM and without it? Four, how does warming relate to damages, and, five, what action, or inaction, will take place if no SRM R&D is done?

Dealing with all these factors in a meaningful way is a severe challenge. One could attempt to create a detailed "value of information" or an "option valuation" model (Bickel 2008). A CC09 Perspective Author (Smith 2010) suggested this approach. At this point, the uncertainties that surround CE are so deep that we believe such an analysis does little more than formalize our perplexity. Yet governments will decide on whether to undertake R&D on CE, and they will do so despite the uncertainties.

CE is, in this regard, hardly a unique problem. Estimating the returns to R&D investment is uncertain even in well-defined areas. Arrow raised the crucial issue:

> "The central economic fact about the processes of invention and research is that they are devoted to the production of information. By the very definition of information, invention must be a risky process, in that the output (information obtained) can never be predicted perfectly from the inputs." (Arrow 1962)

Attempts to quantify the value of R&D in the area of climate change have highlighted the risky nature of R&D and the challenge that it poses to benefit-cost analysis. For example, Baker et al. (2009) asked three experts to assess the likelihood that R&D investments in advanced solar technologies would succeed. The assessed probabilities among the three experts differed by a minimum of two times to a maximum of 640 times. For example, when considering investment in new inorganic solar cell technologies, one expert assessed the chance of success at 0.001, while another thought it was 0.64. In the case of purely organic solar cells, one expert assessed the chance of R&D success at 0.01; another believed the chance as 0.34. Similar divergences were observed in assessments regarding the chance of R&D success in the areas of carbon capture and storage and advanced nuclear power technologies (Baker and Peng 2010). This is not a criticism. It simply acknowledges the point made by Arrow and others: uncertainty is inherent in invention. We suspect that the results would be much the same were one to poll experts regarding the likely success of a large-scale SRM R&D program.

Before a technical problem has, in fact, been solved, one cannot know how difficult its solution will be (Nelson and Winter 1977). Nonetheless, Hecht et al. (2011) in assessing the BCR of R&D into an AIDS vaccine assumed, with probability one, that current R&D spending will lead to a vaccine and that additional spending would advance the date of discovery. In a similar vein, C09 authors Galiana and Green (2010), hereafter GG10, recognize this problem in their assessment of the BCR of energy-technology R&D. And they express the hope that a diversified enough R&D portfolio will lessen the uncertainties. They assumed that R&D spending of $100 billion per year for the next 100 years, more than a six-fold increase from current spending levels, would accelerate the decarbonization of the global economy via the development carbon-free energy sources (e.g., carbon capture and storage, nuclear fusion, breeder reactors, deep geothermal energy, etc.) and equated benefits with reduced climate damages. They assumed that the R&D would call forth technologies attractive enough to penetrate markets without the benefit of any, but very mild, GHG control policies.

In reality, many innovations that at first seem promising fail to pan out. In other cases, unwanted side-effects erode the benefits of otherwise appealing options. One thinks of nuclear power and proliferation risks. Biofuels have worsened global food shortages just as SRM entails some risks of unwanted effects. In all these cases, the value of the innovation cannot be known without reference to the scale of the risks it poses.

Framing the SRM R&D Decision

This section will describe the scenarios that we have analyzed. The reader should not take our focus on the two scenarios that we are about to describe as a statement that we believe either of them to be very desirable. Still less do we regard them as realistic projections of the future. Rather, the analysis shows that across a very broad spectrum of GHG control policies, climate sensitivities, and assumptions about SRM side-effects an SRM option would have the potential to yield very large net benefits.

Figure 1 describes the analysis performed in this paper. In it, we assume that key governments' overarching goal is to limit temperature change to no more than 2°C. GG10 also measure the benefit of their emissions reduction strategy by this standard and our adopting it here helps make our results more comparable.

Our analysis begins with the decision (represented with a square) about whether or not to conduct R&D on SRM. If SRM R&D is not performed, we assume that SRM is unavailable. In that case, governments enact the most cost-effective emissions controls able to achieve the 2°C temperature increase target. We refer to this emissions control regime as L2C (Limit to 2°C). As we stated in our previous paper, we believe it is unlikely that SRM would ever be used without development and testing. True, we might face a climate emergency that brings calls for deployment, but this possibility is currently believed to be low, but not zero (B09). Furthermore, recognizing that we are facing an emergency is likely to be difficult. Or obvious emergency situations, such as the rapid disintegration of the West Antarctic Ice Sheet, may come with the realization that it is too late to deploy SRM (B09). Allowing for the possibility that an unproven SRM system could be deployed would only amplify our conclusion that research is likely to pay large dividends.

If SRM R&D is pursued, we assume that the uncertainty (represented with a circle) regarding SRM's damages is resolved. For the sake of simplicity, we assume damages will be either 0%, 1%, 2%, or 3% of gross world product (GWP). After the damages are revealed, society implements a less stringent emissions control regime. To make the situation concrete, we investigate either a policy of *no controls* (NC) or some *emissions controls* (EC). As a point of reference, and to maintain simplicity, our emissions control policy is the same as the economically efficient policy that Nordhaus (2008) refers to as *optimal controls* (OC). The level of emissions controls under EC and L2C are shown in Figure 2.

After selecting either NC or EC, government next deploys a level of SRM that holds temperature change to no more than 2°C, which we refer to as SRM2C. The ability to manage the climate system in this way would require deploying a monitoring system that could observe the Earth's energy balance. Developing such a system, therefore, would be part of an R&D program (B09).

It is important to note that by EC we do not mean the level of emissions controls that would be optimal given an SRM capability. We take this approach for two reasons. First, we believe it is much too early to determine the "optimal" use of SRM and the degree to which it should substitute for emissions reductions. Such an analysis would require an estimate of the damage SRM would cause for different levels of usage. Understanding this relationship would be a fundamental part of an R&D program. Second, in this paper, we treat SRM damage as a variable to which we test sensitivity. Thus, the reader should envision EC as a possible emissions control scenario, in much the same way that the IPCC considers emission controls scenarios, without claiming that these scenarios are in some sense optimal.

As we show in Figure 2, for the sake of simplicity, we assume that SRM R&D will lead to the development of an SRM capability and resolve any uncertainty regarding the level of damages. Reality is certainly more complex and it is possible that SRM R&D may fail. Again, as we stressed earlier, previous CC authors (G&G, Hecht et al. 2011) also assume that R&D will produce results. If the reader prefers to allow for some probability p that SRM R&D will succeed then they can determine the expected net benefits for their preferred scenario by multiplying the estimates we present below by p. Of course, if one believes that SRM R&D will fail (p = 0) then SRM R&D would be worthless; one does not need a model to understand this fact. While we do not attempt to estimate the chance of success, we do not believe it to be so low as to negate the value of R&D. After all, as B09 noted, "basic technical analyses suggest that [SRM] would be within our current technological capabilities." The primary uncertainty is the degree of damage that would attend SRM's use.

The Model

As in BL10, we use the DICE-2007 model Nordhaus (2008) to examine the economics of climate change and the possible use of SRM. As before, we use DICE's endogenously determined discount rate to calculate present values. This has the following benefits. First, it facilitates the comparison of our results to those of Nordhaus (2008). Second, using a different discount rate would be internally inconsistent with the DICE model. While our real discount rate varies, it averages about 4% over our 200-year study period (2005 through 2205). Investigating other discount rates, such as 3% or 5%, would not materially change our results while doubling the number of cases we need to consider, which could obfuscate our results and reasoning.

Table 1 summarizes the performance of the emissions control regimes we consider here. The maximum temperature change by 2205 under a policy of NC is 5.2°C and it is above 2°C for 140 years. Total damages, which are comprised solely of climate damages, are $22.5 trillion (2005$). Under EC, temperature change is above 2°C for 130 years and reaches a maximum of 3.5°C. Total damages in this case total $19.5 trillion, almost 90% of which are in the form of climate damage. Thus, economically efficient emission controls regimes are structured to accept significant climate damages. Climate damages are reduced to $13.4 trillion by limiting temperature change to 2°C, but the increase in abatements costs leads to about $2.7 trillion more damage than under a policy of NC.

Table 1: Performance Characteristics of Considered Emissions Controls Regimes ($ are trillions of '05 $)

Emissions Control Regime	Years to Phase out CO2 Emissions	Max. Temp. Change (by 2205)	Years above 2°C (by 2205)	Climate Damages	Abatement Costs	Total Damages
No Controls (NC)	NA	5.2°C	140	$22.5	$0.0	$22.5
Emissions Controls (EC)	185	3.5°C	130	$17.4	$2.1	$19.5
Limit 2°C (L2C)	80	2.0°C	0	$13.4	$11.8	$25.2

Changes Made to DICE

To estimate the benefits of SRM we modify a few features of DICE. As detailed in BL10, these include changes to DICE's radiative forcing equation, given in Equation (1). $F(t)$ is the increase in radiative forcing at the tropopause at the beginning of period *t* (decades), measured in W m^{-2}. η is the increase in radiative forcing due to a doubling of CO2 concentrations, assumed to be 3.8 W m^{-2}. $M_{AT}(t)$ is the mass of carbon in the atmosphere at the beginning of period *t*. $M_{AT}(1750)$ is the preindustrial concentration of atmospheric CO2, which is defined as the concentration in 1750 (280 ppm). When the ratio of $M_{AT}(1750)$ to $M_{AT}(1750)$ is 2 the log-base-2 term will evaluate to 1. $F_{EX}(t)$ is the external forcing of non-CO2 greenhouse gases and the negative forcing of aerosols. $SRM(t)$ is the negative forcing created by an SRM program. Our treatment of aerosol forcing is consistent with DICE's own modeling of aerosols and the work of other researchers (Andronova 2001, Goes et al. 2011, Bickel and Agrawal 2011). We emphasize, however, as noted by B09 that this global and uniform treatment of SRM is done for reasons of convenience. It may not be feasible or desirable to implement SRM in this way.

$$F(t) = \eta \log_2 \left[\frac{M_{AT}(t)}{M_{AT}(1750)} \right] + F_{EX}(t) - SRM(t) \,. \tag{1}$$

This paper further modifies DICE to account for damages that SRM might cause. To do so, following Goes et al. (2011) and Bickel and Agrawal (2011), we introduce a parameter θ that is the damage caused by SRM, as a percent of GWP, when SRM offsets radiative forcing equal to a doubling of CO2 concentrations ($\eta = 3.8$ W m^{-2}). Specifically, the modified damage function, as a percent of GWP, is given in Equation (2). $T_{AT}(t)$ is the global mean temperature of the atmosphere in period *t*. ψ_1 and ψ_2 are parameters chosen to match the literature regarding climate impacts (Nordhaus 2008). As mentioned above, we investigate four values for θ, equal to 0%, 1%, 2%, and 3%.

$$D(t) = \psi_1 T_{AT}(t) + \psi_2 T_{AT}(t)^2 + \theta \frac{SRM(t)}{\eta}. \quad (2)$$

Net Benefit of SRM Research and Development

This section estimates the net benefit of using SRM to hold temperature changes below 2°C. It begins, however, by highlighting the most significant risk driver: the climate sensitivity.

Climate Sensitivity

The values given in Table 2 are based on a single set of deterministic model assumptions. Of course, key model inputs are in fact uncertain. For example, regarding the climate sensitivity, the IPCC (2007) states:

> "The equilibrium climate sensitivity…is likely to be between 2ºC and 4.5ºC, with a best estimate of 3ºC and it is very unlikely to be less than 1.5ºC. Values substantially higher than 4.5ºC cannot be excluded, but agreement of models with observations is not as good for those values."

The IPCC defines *likely* as greater than a 66% probability and *very unlikely* as less than a 10% probability (IPCC 2005). Bickel (2011) found that a lognormal distribution with a mean of 3.0ºC and standard deviation of 1.5ºC represented this uncertainty well. This, in turn, implies that there is about a 10% chance that the climate sensitivity is below 1.5°C and a 10% chance it is above 5.0°C.

Figure 3 details how uncertainty in the climate sensitivity propagates to uncertainty in the maximum temperature change under EC. In the base case, the climate sensitivity is 3.0°C and the maximum temperature change is 3.5°C. If the climate sensitivity was, on the other hand, 1.5°C then the maximum temperature change would be 1.9°C. If the climate sensitivity was 5.0°C, the maximum temperature change would be 5.1°C.

High climate sensitivities are all the more troubling when one notes that scientists warn that the climate may contain "tipping points". Crossing these points may trigger large changes in the Earth system. These may include loss of Arctic sea ice, melting of the Greenland and Antarctic ice sheets, irreversible loss of the Amazon rain forest, and abrupt changes in the Indian and African monsoons (Meehl et al. 2007). Lenton et al. (2008) augment the work of the IPCC and prioritize these tipping points in terms of their likelihood and proximity. They are particularly concerned about the loss of Arctic sea ice and melting of the Greenland Ice Sheet. As Arctic sea ice melts, it exposes the darker ocean waters. The change leads to additional warming, a positive feedback. Scientists have not identified the critical tipping point temperature, but Lenton et al. (2008) conclude that "a summer ice-loss threshold, if not already passed may be very close and a transition could occur well within this century."

Nordhaus (2008) provided uncertainty ranges for a set of uncertainties that he found to be the most critical. In addition to the climate sensitivity, these included the rate of growth in total factor productivity, the rate the economy can be decarbonized, the asymptotic global population, and the rate at which CO2 is retained in the atmosphere. Figure 3 displays the 80% probability interval for each of these uncertainties. It shows that, in terms of potential warming, climate sensitivity is far and away the largest risk driver. Other uncertainties, such as the speed with which the economy can be decarbonized, play a much more modest role. This fact is not surprising. Even if emissions reductions do begin, it will take many, possibly hundreds of years for the climate to stabilize. This simple fact has significant implications for climate policy and research. For example, current global "clean energy R&D," which is focused on decarbonizing the economy (the third bar in Figure 3) totals over $15 billion annually (Chiavari and Tam 2011). Yet, no formal research program supports SRM even though it is the only technology that may be able to address the single largest risk factor, climate sensitivity. Indeed, some environmentalists wish to declare it off-limits (ETC Group 2010). This hardly seems a prudent course of action.

Direct Cost Estimates

SRM includes a range of technologies. These have included options from marine cloud whitening to mirrors in space. For simplicity, this paper focuses on stratospheric aerosol injection (SAI). It bases its direct cost estimates on this approach. Using published studies, including the National Academy of Sciences (1992), BL10 estimated the cost to inject aerosols into the stratosphere would be $40 per kg (2005 $), or $40 billion per Tg. The required mass depends upon the efficiency of aerosol forcing, the residence time of these aerosols, and the aerosol precursor. Following Crutzen (2006), we assume an efficiency of -0.75 W m^{-2} per Tg S (1 Tg = 1 trillion grams = 1 million metric tons).[3] We assume a residence time of 2.5 years based on Rasch et al. (2008). As in BL10, we assume hydrogen sulfide as a precursor. As a point of comparison, based on these assumptions, offsetting 1 W m^{-2} would require the injection about 0.57 Tg H_2S per year, costing \$0.023 trillion. These costs are a very small fraction of the potential benefits (reduced climate damages and avoided abatement costs). Therefore, our direct cost estimates play an inconsequential role in our results.

SRM Net Benefit

Table 2 presents the net benefits of SRM as a function of SRM damages. These benefit estimates include the direct costs to deploy SRM and the indirect climate damages attributable to SRM. The first row repeats the total damages (climate and abatement) under the L2C policy; these damages are not a function of the damage caused by SRM, since no SRM is used in this case.

Total damages under a policy of NC, with SRM used to hold temperature change below 2°C, are shown in the second row. If, for example, SRM causes no damage (θ = 0%) then total damages would be \$14.7 trillion, yielding a net SRM benefit of \$10.5 trillion (\$25.2 - \$14.7). If SRM causes damages equal to 1%, 2%, or 3% of GWP at a forcing of 3.8 W m^{-2}, equivalent to a doubling of CO2 concentrations (2xCO2), then the benefit of SRM is reduced to \$7.5, \$4.4, or \$1.2 trillion, respectively. Thus, under NC, every percentage point increase in SRM damages lowers the net benefit of SRM by about \$3 trillion. SRM would provide no net benefit if SRM damages were about 3.4% of GWP at a 2xCO2 forcing.

Table 2: Net Benefit of SRM under No Controls and Emissions Controls (trillions \$2005)

	SRM Damages (θ)			
	0%	**1%**	**2%**	**3%**
Limit 2°C (L2C)	\$25.2	\$25.2	\$25.2	\$25.2
No Controls + SRM2C	\$14.7	\$17.7	\$20.8	\$24.0
NC SRM2C Benefit	\$10.5	\$7.5	\$4.4	\$1.2
Emissions Controls + SRM2C	\$16.0	\$17.6	\$19.1	\$20.7
EC SRM2C Benefit	\$9.2	\$7.7	\$6.1	\$4.5

[3] At this rate, 1 g of S offsets about 320,000 g (0.32 MT) of CO_2. Or, every Tg of S offsets about 40 ppm of CO_2, which is about 20 years of global emissions.

Under a policy of EC with SRM2C, a damage-free version of SRM would be worth about $9.2 trillion. This value declines by about $1.6 trillion for every percentage point increase in SRM damages and would equal about $4.5 trillion at 3% SRM damage. The net benefit of SRM would be negative in this case if SRM damages were greater than about 5.7% at a forcing of 3.8 W m^{-2} (2xCO2 forcing).

Some observers have claimed that DICE understates marginal damage from climate change. In fact, estimates of marginal damage vary widely (Tol 2008). In any case, SRM both lessens climate damage and permits lower GHG control costs. Should climate change prove either more harmful than DICE assumes, or less, added gains in one class of benefits will offset at least part of the shortfall in the other class. The adjustments are not likely to change the finding of large and robust net benefits.

According to Table 2, SRM has a positive net benefit for all the SRM damage levels depicted in Figure 1. Thus, society would still gain by deploying SRM, even were SRM to cause damages equivalent to 3% of GWP at a 2xCO2 forcing. How can this be?

The damages caused by GHGs and policies to address them come in two forms: climate damages and abatement costs. Figure 4 displays these components for L2C and EC. L2C incurs substantial abatement costs, which results in total damages under this policy exceeding those of EC until about 2125. In fact, damages under L2C exceed those of NC (shown in Figure 5) through 2105. As mentioned earlier, it is also clear than the economically efficient policy of EC would still accept substantial climate damages.

A successful SRM program would offset both of these costs, not just climate damages. The question, of course, is whether or not the damages caused by SRM would exceed the sum of climate damages *and* the abatement costs that SRM is intended to offset. Figure 5 compares total damages under L2C to EC with SRM2C; NC is shown as a point of reference. Total damages, when using SRM2C, begin to diverge from those of EC in 2075. Given DICE's assumptions, 2075 is the first year in which temperatures without SRM would exceed 2°C. Hence, it is the first year in which SRM would be deployed. If SRM causes no damage (θ = 0%) then total damages are substantially reduced and below those of L2C until about 2205. Allowing for SRM damages increases total damages, but they are still below L2C until the next century. In fact, even if SRM caused 3% damages, it would still be less damaging than attempting to limit temperature changes to 2°C based solely on emissions reductions.

Just how damaging SRM might be is a question for an R&D program, but we offer two points of reference. First, unabated climate change is projected by DICE to cause damages of about 1.4% of GWP in 2065, which is the year CO2 concentrations are doubled. Thus, the SRM damage scenarios we investigate here range from an assumption that SRM is about as damaging as climate change itself (1%) to SRM is more than twice as damaging as climate change (3%). Second, as discussed earlier, the IPCC (2007) estimates that aerosols are currently providing negative forcing of 1.2 W m^{-2}, which is equivalent to about 32% of the forcing we expect for a doubling of CO2. Current emissions are primarily into the troposphere (where we live); whereas most SAI concepts envision injection taking place in the stratosphere. However, as a very rough estimate, SRM damages of 1%, 2%, or 3% at a forcing equivalent to a doubling of CO2 concentrations would equate to GWP reductions of 0.32% (32% of 1%), 0.64%, or 0.96%, respectively.

Net Benefit for Differing Climate Sensitivities

The proceeding section's analysis assumed that the climate sensitivity was 3.0°C. Figure 6 presents the net benefits of SRM under NC (top) and EC (bottom) for climate sensitivities of 1.5°C, 3.0°C, and 5.0°C, as a function of the damage caused by SRM. The lines labeled 3.0°C match the net benefits given in Table 2. At a climate sensitivity of 1.5°C the net benefit of SRM changes very little under NC and not at all under EC. This occurs because little (NC) or no (EC) SRM is needed in this case. Under EC, the benefit attributable to SRM, about $8 trillion, is comprised solely of the fact that with an SRM capability society has the option of choosing a less stringent emissions control policy. This pays off in the case of a 1.5°C climate sensitivity because temperatures would not have reached 2.0°C under EC in any event and, thus, the stricter and more costly controls were not needed. A 5.0°C climate sensitivity tends to increase the value of SRM. However, this value decreases more rapidly with the SRM damage level because the intensity of SRM usage is greater. At a damage level of 3% and a 5.0°C climate sensitivity, SRM would cause net damages under NC.

Level of SRM Usage

Figure 7 displays the level of SRM usage required to hold temperature change under 2.0°C for NC (top) and EC (bottom) as a function of the climate sensitivity. SRM usage intensity is measured in W m^{-2} (left-hand axis) and Tg S per year (right-hand axis), assuming a forcing efficiency of -0.75 W m^{-2} per Tg S and a residence time of 2.5 years. As discussed above, under EC and with a climate sensitivity of 1.5°C, SRM is not required. If climate sensitivity is 3.0°C, then SRM is first deployed in 2075 and reaches about 1.4 W m^{-2} (about 0.75 Tg S per year) by 2100. SRM usage in this case does not exceed 2.0 W m^{-2} until 2100. To place this intervention in perspective, consider four comparisons:

(1) 2 W m^{-2} is about 0.6% of the incoming solar radiation of 341 W m^{-2} (Trenberth 2009),

(2) Anthropogenic aerosol emissions currently provide negative forcing of 1.2 W m^{-2} (IPCC 2007),

(3) Anthropogenic emissions of sulfur total 55 Tg per year (Stern 2005),

(4) Mt. Pinatubo injected 10 Tg of sulfur into the stratosphere (Crutzen 2006).

Under NC, with a 3.0°C climate sensitivity, SRM is deployed in 2065 and exceeds about 2.5 W m^{-2} (1.3 Tg S) by the end of the century. If the climate sensitivity was 1.5°C, SRM would not be deployed until the next century and would not exceed 2.0 W m^{-2} until 2165. A sensitivity of 5.0°C would bring increased usage, but the SRM intervention would remain below 3 W m^{-2} until 2095. In all cases, SRM is deployed at earliest in 2045, which would seem to provide ample time for well designed R&D program.

Thus, deploying SRM is not on its face infeasible. Further, as its use is considered here, the intervention would be within that which the climate system currently experiences. The interventions considered in this analysis are a fraction of those that humans are currently making. They are an even smaller fraction of natural events such as volcanic eruptions. Even so, the sustained forcing that SAI would cause would differ fundamentally from the impulse disturbances produced by volcanoes (B09). We conclude that SRM should not be deployed given the current uncertainties, nor could it be. At the same time, SRM cannot be dismissed out of hand, and research should be conducted to learn more about the range of possible impacts.

Comparison to Emissions Reductions

Since the CC12 expert panel is tasked, in part, with ranking differing, but not necessarily exclusive, approaches to climate change, we now compare the net benefit of SRM to *costless* emissions reductions. This comparison is helpful since estimates of both benefits are performed within the same model. Table 3 presents the benefit of a technology (or technologies), to be deployed in 2025, that could costlessly (and linearly) eliminate all CO2 emissions within either 40, 80, or 120 years for differing climate sensitivities. Phasing out CO2 emissions, in this way, over 80 years brings a benefit of between \$3.0 and \$13.5 trillion, depending upon the climate sensitivity. A, very rapid, 40-year phase out is worth between \$4.1 and \$18.2 trillion.

The values presented in Table 3 are on the same order as the benefit of SRM, even accounting for the fact that SRM is not costless and may incur damages (compare Table 3 to Table 2 and Figure 6). This occurs because even if CO2 emissions begin in 2025, the CO2 that is currently in the atmosphere, and that which would be emitted during the phase out period, will take many, possibly hundreds, of years to fade away. This, in conjunction with thermal capacity of the oceans, means that some warming will be unavoidable—unavoidable that is, if one does not consider the use of SRM. This highlights another important policy issue regarding the use of SRM: SRM's performance characteristics differ completely from those of emissions reductions. While SRM may not be well suited to substitute for emissions reductions in the long-term, emissions reductions are certainly not suited to provide benefits in the near term. This suggests a blended strategy, where SRM might be used to lessen warming and risk in the near term, while emission reductions address long-terms risks.

Table 3: Net Benefit of Costless and Complete CO2 Emissions Reductions Beginning in 2025 (trillions $2005)

Years to Phase Out All CO2 Emissions	Climate Sensitivity		
	1.5°C	3.0°C	5.0°C
40	4.1	10.5	18.2
80	3.0	7.7	13.5
120	2.3	5.8	10.1

Benefit-Cost of SRM Research and Development

With the results of the previous section, we now estimate the BCR of SRM R&D. Following GG10 we define the BCR of R&D as

$$BCR = \frac{\text{Present Value of Net Benefits}}{\text{Present Value of R\&D Expenditures}}. \tag{3}$$

Assuming that an SRM R&D program would require a total investment of $5 billion, as we detailed earlier in the paper, and using the net benefit assessments contained in Table 2, we obtain the BCR estimates shown in Table 4.

Table 4: BCR of SRM R&D under No Controls and Emissions Controls

	SRM Damages (θ)			
	0%	1%	2%	3%
NC SRM2C	2,107	1,497	877	247
EC SRM2C	1,844	1,530	1,214	895

Thus, we believe the BCR for SRM R&D is large: possibly between about 250 and 2000 to 1. The actual BCR depends up the damages caused by SRM. As a rough estimate, though, we might say the BCR is

1000 to 1. Again these results assume that an SRM R&D program would be successful. But the very large net benefits that would flow from the success of SRM argue that R&D is a very good investment. *Even a 10% chance of success will result in an expected-BCR of 100 to 1*.

Discussion and Conclusion

As discussed at the outset, the results of this paper are hardly surprising and the logic is easy to follow: If one believes that climate change will result in significant damages either due the warming itself or the costs imposed by abatement measures, then the ability to reduce warming at low cost will accrue substantial benefits. Given that the technology to achieve these benefits does not exist and might itself cause damage, research is likely to pay large dividends.

Funding Request

The CC12 expert panel is tasked with allocating a total budget of $75 billion over the next four years. The analysis presented above details the BCR of a complete, ten-year, R&D program. We do not attempt to compute the BCR of only the first four years of research. Had we done so, the values are likely to be even greater than those shown in Table 4 for at least two reasons. First, the required investment for Phase 1 of the R&D program consists almost entirely of laboratory experimentation and computational modeling. It is, therefore, only about 12% of the total ten-year requirement. Second, much is likely to be learned during the initial research.

Caldeira and Keith's (2010) R&D cost estimates imply spending of about $60 million over the first four years. We multiplied these estimates by 10 and thus request $600 million ($0.6 billion) or 0.8% of the CC12 budget be allocated to climate engineering R&D. In all likelihood, this funding request is too large, but again, we have tried to err on the side of underestimating the benefit-cost ratios.

Climate Engineering R&D: Why Not?

We believe the analysis outlined in this paper makes a compelling case for climate engineering research. Why then do some oppose it? What is the case against *research*? There are five primary arguments:

1. The climate system's complexity is beyond our capacity to understand and therefore any intervention is fraught with risk and should be considered unsafe (Cicerone 2006).
2. SRM will be perceived as a substitute for greenhouse gas controls and therefore society will lose its will to implement emission controls (Cicerone 2006).

3. Some environmentalists object to SRM on the grounds that it treats the symptoms of manmade climate change rather than removing its root cause (Tetlock and Oppenheimer 2008).
4. The development of technology to reduce warming may trigger international tensions and even conflict as countries vie for the right to choose the optimal climate (Victor 2009).
5. SRM is inherently unjust because (1) its benefits and costs will not be uniform, (2) it places future generations at risk, and (3) it is inexpensive and thus could be implemented unilaterally (Svoboda 2011).
6. Once started, it might be stopped prematurely resulting in rapid warming and increased damages (Goes et al. 2010).

Addressing all of these concerns in detail is beyond the scope of our current effort, however, we would like to offer a few thoughts, which we number in accordance to the list above.

1. **Complexity.** First, complexity of the climate system also makes it difficult to know the degree to which increased GHG concentrations will cause harmful climate change. Second, ignorance is in any case an argument for research rather than one against it.
2. **Substitution.** One might argue that SRM should only be deployed as a "last resort." While this is easy to say and might allow one to discuss SRM without controversy, we do not subscribe to this view. First, how do we define "last resort"? Even if the answer were clear, will we be able to tell that such a moment has arisen? Second, it may indeed be economic to replace some degree of emissions reductions with SRM. Substitution might well lessen both climate damages and the costs of GHG controls. We are not aware of any proof that such a blended strategy is clearly suboptimal. What valid reason, then, could warrant suppressing a welfare enhancing policy option, and, absent an order of Platonic guardians, on what authority would it be done?
3. **Treating symptoms.** This approach is often the most cost-effective available response: "Typically in an attempt to find a solution to a problem people look to its causes, or yet more fatuously, to its *root* causes. However, there need be no logical connection between the cause of a problem and appropriate or even feasible solutions" (Collier 2010). Thus, in economics, medicine, and politics treating symptoms often lowers total costs. The high

institutional hurdles to curbing greenhouse gas emissions suggest that, with climate change too, treating symptoms may offer great benefits.

4. **Conflict.** Except for purely domestic adaptation, all steps to cope with climate change are likely to trigger some level of conflict. Efforts to control emissions certainly have. Growing global interdependence strengthens all states' incentives to cooperate. Yet the interests of major states often conflict. States build regimes such as the World Trade Organization to lower the transaction costs of cooperating on the issues on which they have some interests in common and some in conflict. (Keohane 1984). Experienced diplomats expect that were SRM to be deployed, the major powers would form a regime to govern its use (Benedick 2011).
5. **Justice.** No action we may take in response to climate change will result in uniform costs/benefits and remove all risk from future generations. In fact, a failure to research and deploy SRM would be unjust by this standard because it moves future generations towards a climate tipping point.
6. **Termination.** For two reasons, once SRM starts, there will be strong incentives to continue it. First, termination would be unlikely just because it would be costly. Second, as interest groups organize around existing programs, government policy often develops strong path dependence. Further, the validity of the case against a start and stop use of SRM depends heavily on what will happen if SRM is not used. Bickel and Agrawal (2011) detail many cases where SRM would produce net benefits even if it was aborted.

In sum, SRM is a family of technologies that could offer immense benefits. The proposed research program is inexpensive, a small fraction of current climate science R&D spending and the CC12 budget. The time has come to begin researching this approach to dealing with climate change.

References

Andronova, N.G. and M.E. Schlesinger, 2001. Objective estimation of the probability density function for climate sensitivity. *Journal of Geophysical Research* **106**(D19) 22605 – 22611

Arrow, Kenneth J., 1962. "Economic Welfare and the Allocation of Resources for Invention" in *The Rate and Direction of Inventive Activity: Economic and Social Factors*, Richard Nelson (ed). Princeton, Princeton University Press

Baker, Erin, Haewon Chon, and Jeffrey Keisler, 2009: Advancing solar R&D: Combining economic analysis with expert elicitations to inform climate policy, *Energy Economics* **31**, S37-S49

Baker, E. and Y. Peng, 2010: The value of better information on technology R&D programs in response to climate change, Working Paper.

Barrett, Scott. 2003: *Environment & Statecraft: The Strategy of Environmental Treaty-Making.* New York: Oxford University Press

Benedick, Richard Elliot (2011): "Considerations on Governance for Climate Remediation Technologies: Lessons from the 'Ozone Hole'," *Stanford Journal of Law, Science & Policy* 4, 6-9

Bickel, J.E. 2008. The Relationship between Perfect and Imperfect Information in a Risk-Sensitive Two-Action Problem. *Decision Analysis* **5**(3) 116-128

Bickel, J.E., 2011: The Economics of Using Climate Engineering to Manage the Risk of Tipping Points in the Climate System. Under review at *Socio-Economic Planning Sciences*

Bickel, J.E., and S. Agarwal, 2011: Reexamining the economics of aerosol geoengineering. Under review at *Climatic Change*

Bickel, J.E. and L. Lane, 2010: Climate Engineering, in *Smart Solutions to Climate Change: Comparing Costs of Benefits*, Bjørn Lomborg (ed.), Cambridge University Press, Cambridge, 9-51

Blackstock, J.J., D.S. Battisti, K. Caldeira, D.M. Eardley, J.I. Katz, D.W. Keith, A. A. N. Patrinos, D. P. Schrag, R.H. Socolow and S.E. Koonin, 2009: *Climate Engineering Responses to Climate Emergencies*, archived online at: http://arxiv.org/pdf/0907.5140

Broder, J.M., and M.L. Wald, 2009: Big Science Role is Seen in Global Warming Cure, *New York Times*, 12 February, A24

Caldeira, K. and D. E. Keith, 2010: The Need for Climate Engineering Research, *Issues Sci Tech* **27**(1), 57-62

Chiavari, J. and C. Tam, 2011: *Good Practice Policy Framework for Energy Technology Research, Development and Demonstration (RD&D)*, International Energy Agency

Cicerone, R.J., 2006: Geoengineering: Encouraging research and overseeing implementation, *Climatic Change*, 77(3-4), 221-226

Collier, P. 2010: *The Plundered Planet: Why We Must – and How We Can – Manage Nature for Global Prosperity*. New York: Oxford University Press

Crutzen, P.J., 2006: Albedo Enhancement by Stratospheric Sulfur Injections: A Contribution to Resolve a Policy Dilemma?, *Climatic Change*, **77**(3-4), 211-220

ETC Group, 2010: Geopiracy: The Case Against Geoengineering.

Edgerton, D., 2007: *The Shock of the Old: Technology and Global History since 1900*, Oxford University Press, New York, NY

Galiana, I. and C. Green, 2010: Technology-Led Climate Policy, in *Smart Solutions to Climate Change: Comparing Costs of Benefits*, Bjørn Lomborg (ed.), Cambridge University Press, Cambridge, 292-339

Goes, M., N. Tuana and K. Keller. 2011. The economics (or lack thereof) of aerosol geoengineering. *Climatic Change*

Hecht, R., D.T. Jamison, J. Augenstein, G. Patridge, and K. Thorien, 2011: *Vaccine Research and Development*. Copenhagen Consensus Center & Rush Foundation

Higgins, Paul A. T., 2011: Climate change in the FY 2011 budget, *AAAS Report XXXV: Research and Development FY 2011*, Washington, DC

IPCC, 2005: Guidance notes for lead authors of the IPCC fourth assessment report on addressing uncertainties.

IPCC, 2007: Summary for policymakers. In: S. Solomon, D. Qin, M. Manning, Z. Chen, M. Marquis et al. (eds.), *Climate change 2007: The physical science basis*. Contribution of Working Group 1 to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA

Jacoby, H.D., M.H. Babiker, S. Paltsev, and J.M. Reilly, 2008: Sharing the Burden of GHG Reductions, *MIT Joint Program on the Science and Policy of Global Change*, Report No. 167

Keohane, R.O. 1984: *After Hegemony: Cooperation and Discord in the World Political Economy*. Princeton: Princeton University Press

Lane, L., K. Caldeira, R. Chatfield, and S. Langhoff, 2007: Workshop Report on Managing Solar Radiation, NASA Ames Research Center, Carnegie Institute of Washington Department of Global Ecology, NASA/CP-2007-214558

Lane, L., and D. Montgomery, 2008: Political Institutions and Greenhouse Gas Controls, *AEI Center for Regulatory and Market Studies*, Related Publication 08-09

Latham, J., P.J. Rasch, C.C. Chen, L. Kettles, A. Gadian, A. Gettleman, H. Morrison, K. Bower, and T. Choularton, 2008: Global Temperature Stabilization via Controlled Albedo Enhancement of Low-Level Maritime Clouds, *Philosophical Transactions of The Royal Society A*, **366**(1882), 3969-3987

Lenton, T.M., H. Held, E. Kriegler et al,. 2008: Tipping elements in the Earth's climate system. *PNAS* 105(6) 1786-1793

Lenton, T.M., and N.E. Vaughan, 2009: The radiative forcing potential of different climategeoengineering options, *Atmospheric Chemistry and Physics Discussions 9*, 2559-2608

Meehl, G.A., T.F. Stocker, W.D. Collins et al., 2007: Global climate projections. In: S. Solomon, D. Qin, M. Manning, Z. Chen, M. Marquis et al. (eds.), *Climate Change 2007: The physical science basis. Contribution of Working Group 1 to the Fourth Assessment Report on the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, United Kingdom and New York, NY

National Academy of Sciences (NAS), 1992: Policy Implications of Greenhouse Warming: Mitigation, Adaptation, and the Science Base, National Academy Press, Washington, DC.

Nelson, Richard R. and Sidney G. Winter 1977: "In Search of Useful Theory of Innovation," *Research Policy*, 6(1): 36-76

Nordhaus, W.D., 2002: Modeling induced innovation in climate change policy, in A. Grubler, N. Nakicenovic, and W.D. Nordhaus (eds.), *Modeling Induced Innovation in Climate change* Policy, Resources for the Future Press, Washington, DC

Nordhaus, W. D., 2008: *A Question of Balance: Weighing the Options on Global Warming Policies*, Yale University Press, New Haven, CT

Pielke, Jr., R.A., T.M.L. Wigley and C. Green, 2008: Dangerous Assumptions, *Nature* **452**(7187), 531-532

Pielke, Jr., R.A., 2009: An idealized assessment of the economics of air capture of carbon dioxide in mitigation policy, *Environmental Science & Policy*, **12**(3), 216-225

Popp, D., 2004: ENTICE: endogenous technological change in the DICE model of global warming, *Journal of Environmental Economics and Management* 48(1), 742-68

Rasch, P.J., P.J. Crutzen, and D.B. Coleman, 2008: Exploring the geoengineering of climate using stratospheric sulfate aerosols: The role of particle size, *Geophysical Research Letters*, **35**(2), L02809.

Richels, R.G., A.S. Manne, and T.M.L. Wigley, 2004: Moving Beyond Concentrations: The Challenge of Limiting Temperature Change, *AEI-Brookings Joint Center for Regulatory Studies*, Working Paper 04-11.

Royal Society, The, 2009: Geoengineering the climate: Science, governance and uncertainty, London.

Salter, S., G. Sortino, and J. Latham, 2008: Sea-going hardware for the cloud albedo method of reversing global warming, *Philosophical Transactions of The Royal Society A*, **366**(1882), 3989-4006

Smith, A.E., 2010: Climate Engineering, Alternative Perspective, in *Smart Solutions to Climate Change: Comparing Costs of Benefits*, Bjørn Lomborg (ed.), Cambridge University Press, Cambridge, 62-73

Stern, D.I. 2005: Global sulfur emissions from 1850 to 2000. *Chemosphere* **58**, 163-175

Stocker, T.F., 2003: Changes in the Global Carbon Cycle and Ocean Circulation on the Millennial Time Scale, *Global Climate: Current Research and Uncertainties in the Climate System*, X. Rodó and F.A. Comín (eds), Springer-Verlag, Berlin, Germany

Stokey, N.L., V.L. Smith, T.C. Schelling, F.E. Kydland, and J.N. Bhagwati, 2010: Expert Panel Ranking, in *Smart Solutions to Climate Change: Comparing Costs of Benefits*, Bjørn Lomborg (ed.), Cambridge University Press, Cambridge, 381-393

Svoboda, T., K. Keller, M. Goes. 2011: Sulfate aerosol geoengineering: The question of justice. *Public Affairs Quarterly*

Tetlock, P.E. and M. Oppenheimer (2008). "The Boundaries of the Thinkable," *Daedalus* **137**(2): 59–70.

Tol, R.S.J., 2008: The Social Cost of Carbon: Trends, Outliers and Catastrophes. *Economics: The Open-Access, Open-Assessment E-Journal*, 2(25), 1-22.

Trenberth, K.E., J.T. Fasullo, and J.T. Kiehl, 2009: Earth's Global Energy Budget, *Bulletin of the American Meteorological Society*, **90**(2), 311-323

United Nations Framework Convention on Climate Change (UNFCC), 2008: UNFCCC: Rising industrialized countries' emissions underscore urgent need for political action on climate change at Poznan meeting, Press release, 17 November

United States Department of Energy (US DOE), 2002: Response Options to Limit Rapid or Severe Climate Change: Assessment of Research Needs, *National Climate Change Technology Initiative*

Victor, D.G., M.G. Morgan, J. Apt, J. Steinbruner, and K. Ricke, 2009: "The Geoengineering Option: A Last Resort Against Global Warming?" *Foreign Affairs*, 88(2): 64-76

Figures

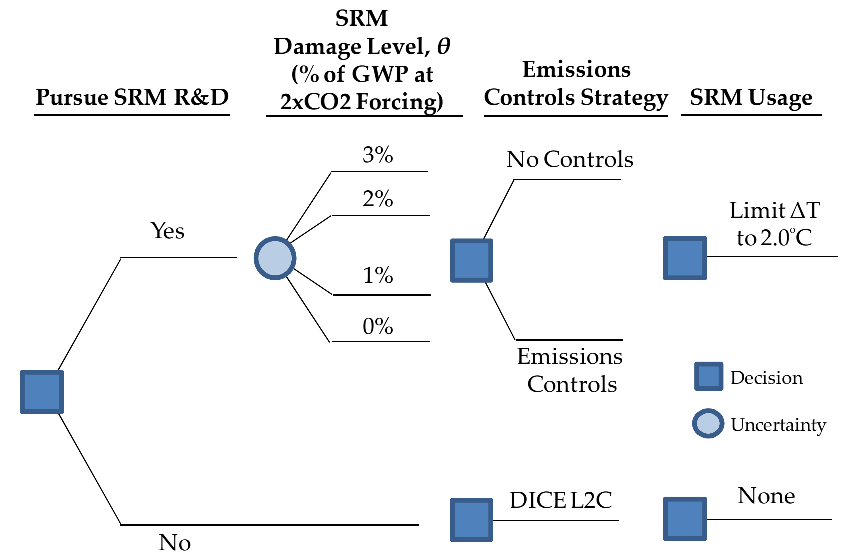


Figure 1: Framing of SRM R&D decision.

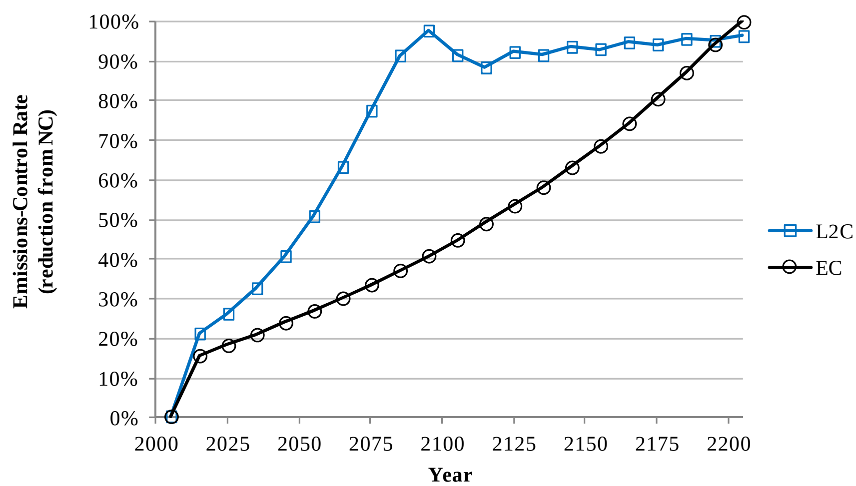


Figure 2: Emissions-control rates under EC and L2C.

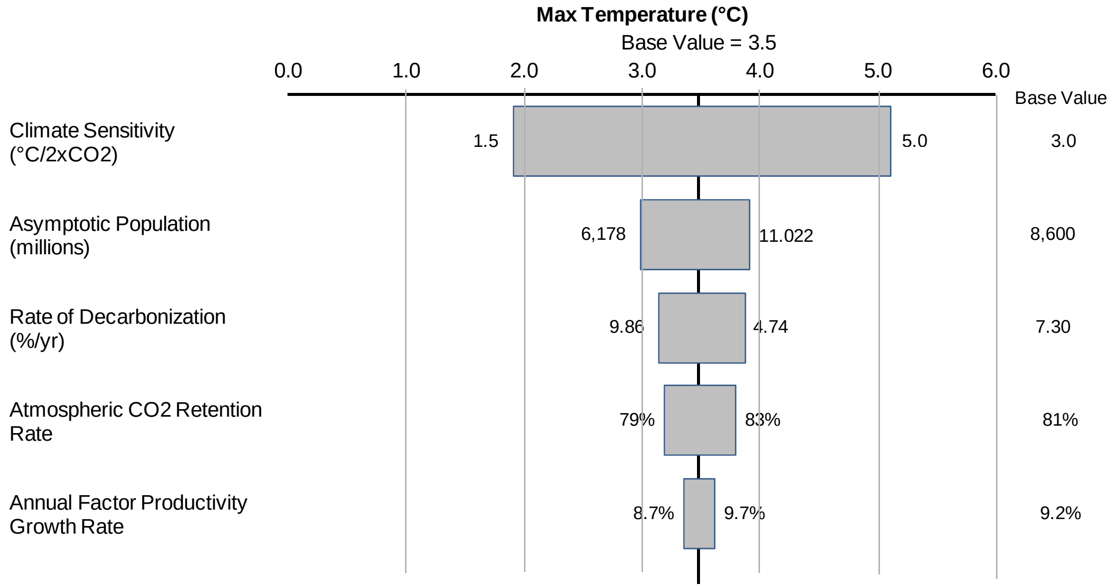
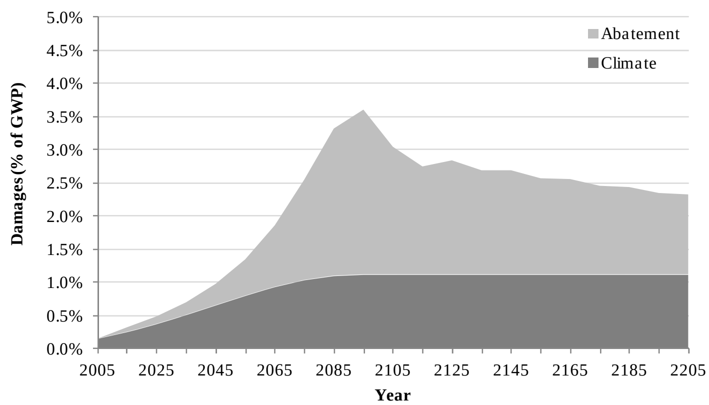


Figure 3: Sensitivity of maximum temperature change under EC to key model inputs.



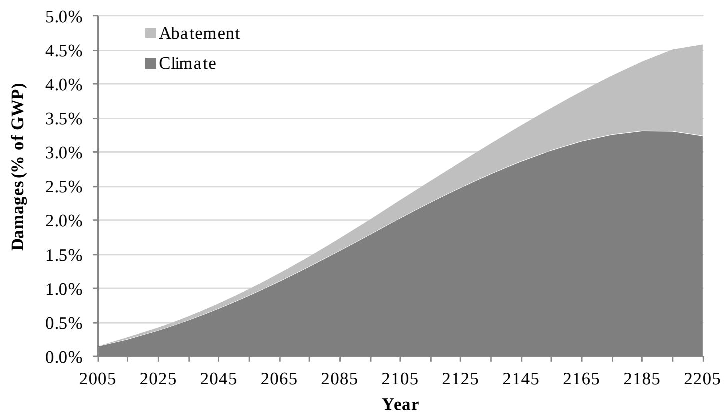


Figure 4: Components of total damages for L2C (top) and EC (bottom).

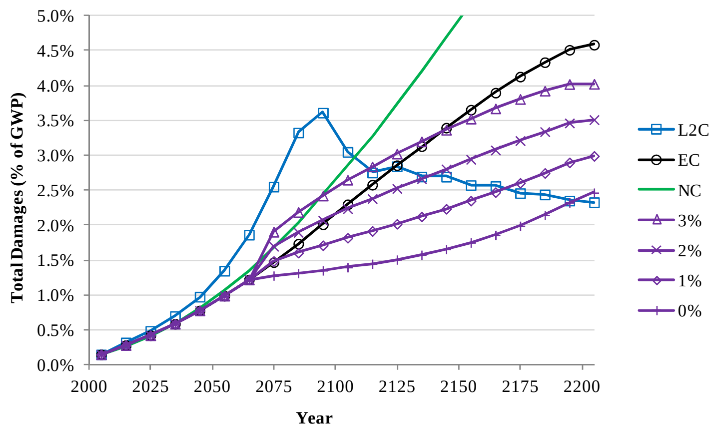
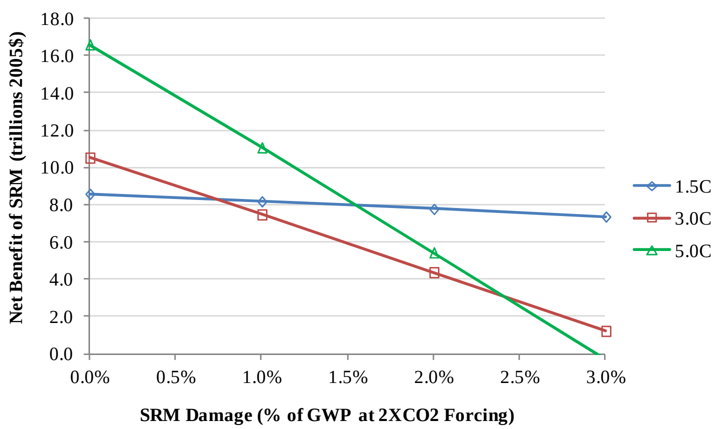


Figure 5: Comparison of total damages under L2C to EC with SRM2C, as a function of the damages caused by SRM (0%, 1%, 2%, or 3%).



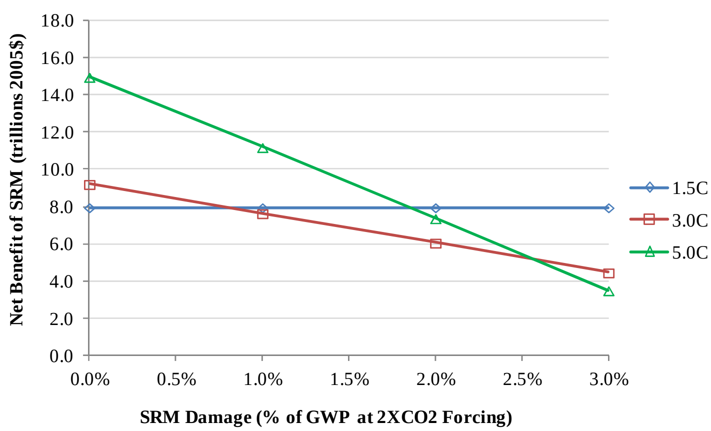


Figure 6: Sensitivity of SRM net benefits to the climate sensitivity under NC (top) and EC (bottom).

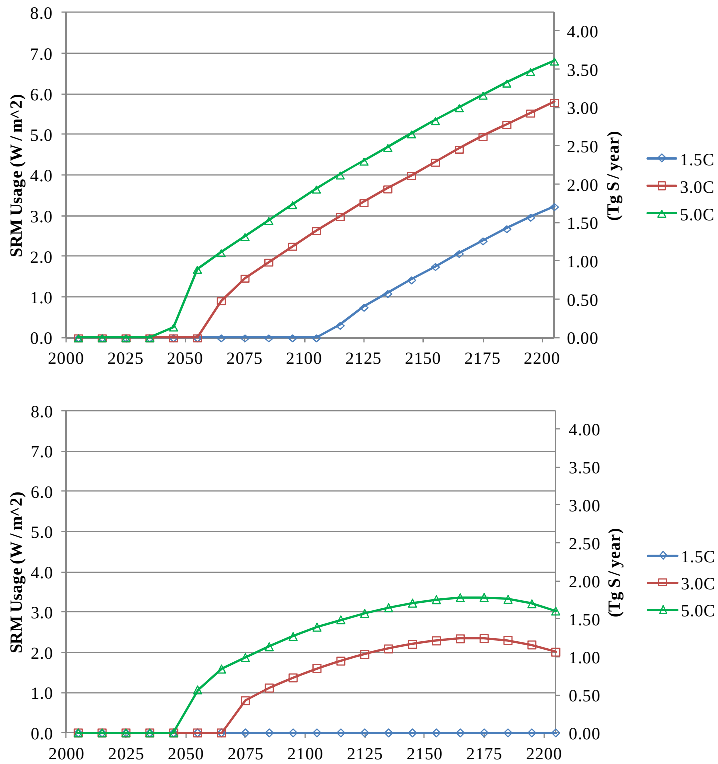


Figure 7: Level of SRM usage under NC (top) and EC (bottom) for differing climate sensitivities.