Copenhagen Consensus on Climate

ADVICE FOR POLICYMAKERS

Lee Lane & J Eric Bickel, Isabel Galiana & Chris Green, Valentina Bosetti

Introduction by Bjørn Lomborg

www.FixTheClimate.com

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PREFACE

In 2009, the Copenhagen Consensus Center commissioned new research on the economics and feasibility of different responses to global warming, and then used Nobel Laureate economists to evaluate that research and identify the best and worst ways to counter this global challenge.

Much of the world's current focus is on cutting carbon dioxide emissions, but there are many ways to go about fixing the climate. The optimal policy response will combine an array of responses in a way that creates the biggest impact for the available money.

The Copenhagen Consensus Center commissioned top climate economists to write research papers that each examine the benefits and costs of one response to global warming. Eight sets of authors looked at the following topics: climate engineering, carbon mitigation, forestry, black carbon, methane, adaptation, technology-led policy response, technology transfers.

A second set of papers provided a peer review of the analyses and assumptions used, to ensure that a range of expert perspectives was heard.

The research papers, available at www.fixtheclimate.com, are being published in full by Cambridge University Press in 2010.

An Expert Panel of five world-class economists – including three recipients of the Nobel Prize – met in September 2009 to consider the research papers, and form conclusions about which solution to climate change is the most promising. The Expert Panel comprised: Finn E Kydland (Nobel Laureate), Thomas C Schelling (Nobel Laureate), Vernon L Smith (Nobel Laureate), Nancy L Stokey (Frederick Henry Prince Distinguished Service Professor of Economics at the University of Chicago), and Jagdish Bhagwati (University Professor at Columbia University). They were asked to answer the question:

If the global community wants to spend up to, say, $250 billion per year over the next 10 years to diminish the adverse effects of climate changes, and to do the most good for the world, which solutions would yield the greatest net benefits?

After scrutinizing the 21 research papers, the Expert Panel agreed upon a prioritized list showing the most – and least – effective ways of reining in temperature increases. They concluded that the most effective use of resources would be to invest in:

- Researching solar radiation management technology;
- A technology-led policy response to global warming that is designed to develop green technology faster;
- Researching carbon storage technology;

In the Advice for Policymakers publication, the authors go a step further, by outlining the arguments for investment in each of the Expert Panel's top-rated proposals. These papers provide a timely and useful contribution to discussion about the best responses to global warming.

The Copenhagen Consensus Center

The Copenhagen Consensus Center is a global think-tank based in Denmark that publicizes the best ways for governments and philanthropists to spend aid and development money.

The Center commissions and conducts new research and analysis into competing spending priorities. In particular it focuses on the international community's efforts to solve the world's biggest challenges.

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Finding the Smartest Ways to Fix the Climate

Bjørn Lomborg

Global warming will have a myriad of impacts, many of which will be negative in the long run. We have long ago moved on from any mainstream disagreements about the science of global warming. The crucial, relevant conversation today is about the economics of our response.

Without a solid economic footing for our decisions, we will repeat mistakes made in the past. Concern has been great, but humanity has done very little so far that will actually prevent these outcomes. Carbon emissions have kept increasing, despite nearly twenty years of repeated promises of cuts.

Everybody wants to prevent global warming, and the real question is: How can we do that best?

We all have a stake in ensuring that climate change is dealt with. We turned to climate scientists to inform us about the problem of global warming. We need to turn to climate economists to enlighten us about the benefits, costs, and possible outcomes from different responses to this challenge.

Should policy-makers continue with attempts to make a binding, international agreement on carbon reduction? What could be achieved by planting more trees, cutting methane, or reducing black soot emissions? Is it sensible to focus on a technological solution to warming? Or should we just adapt to a warmer world?

Much of the current policy debate remains focused on cutting carbon, but there are many ways to go about repairing the global climate. Our choices will result in different outcomes and different costs. The optimal combination of solutions will create the biggest impact for the least money.

In the preface, the Copenhagen Consensus on Climate project was outlined: top climate economists and expert researchers were commissioned to write papers that closely examined different feasible responses to global warming. These papers looked carefully at the likely costs, benefits and ramifications, and were critiqued by a second set of specialist academics. A group of five world-class economists – including three Nobel laureates – identified the most effective policy options, which are elaborated upon here. It is helpful here to reproduce the Expert Panel's findings (overleaf).

One of the most striking characteristics of the Expert Panel's findings, obviously, is the verdict that short-term carbon emission reductions through carbon taxes are a "poor" response to global warming. It bears pointing out that the expert panel also noted in their findings[1] that cutting carbon through cap-and-trade would be an even poorer solution.

This seems counter-intuitive given the state of policy discussion today. At its heart, much of the debate over climate change deals with just one divisive and vexing question: How big should cuts in carbon emissions be?

This narrow focus makes the debate unconstructive. Everybody wants to prevent global warming, and the real question is: How can we do that best? We should be open to other ways to stop warming – such as cutting carbon emissions in the future instead of now, or focusing on reducing emissions of other greenhouse gases.

Global warming will create significant problems, so carbon reductions offer significant benefits. Cutting carbon emissions, however, requires a reduction in the basic energy use that underpins modern society, so it will also mean significant costs.

1 Available atwww.fixtheclimate.com.

Dr. Bjørn Lomborg is the director of the Copenhagen Consensus Center, is a global opinion leader. He is the author of the *Skeptical Environmentalist* and *Cool It*. He was named one of the 75 most influential people of the 21st Century by *Esquire* magazine, one of the 50 people who could save the planet by the *Guardian*, one of the top 100 public intellectuals by *Foreign Policy*, and one of the world's 100 most influential people by *Time*. He is the former director of Denmark's Environmental Assessment Institute, and an adjunct professor at Copenhagen Business School. The Copenhagen Consensus Center brings together some of the world's top economists, including Nobel laureates, to set priorities for the world. Editor of the book *Global Crises, Global Solutions* first and second edition (Cambridge University Press).

Copenhagen Consensus on Climate Expert Panel Results

RATING		SOLUTION	FROM RESEARCH PAPER
"Very Good"	1	Marine Cloud Whitening Research	Climate Engineering by Bickel and Lane
	2	Technology-led Policy Response	Technology by Green and Galiana
	3	Stratospheric Aerosol Insertion Research	Climate Engineering by Bickel and Lane
	4	Carbon Storage Research	Technology by Bosetti
"Good"	5	Planning for Adaptation	Adaptation by Carraro *et al*
	6	Research into Air Capture	Climate Engineering by Pielke Jr
"Fair"	7	Technology Transfers	Technology Transfers by Yang
	8	Expand and Protect Forests	Forestry by Sohngen
	9	Stoves in Developing Nations	Black Carbon Mitigation by Montgomery *et al*
"Poor"	10	Methane Reduction Portfolio	Methane Mitigation by Kemfert and Schill
	11	Diesel Vehicle Emissions	Black Carbon Mitigation by Montgomery
	12	$20 OECD Carbon Tax	Carbon Mitigation by Yohe and Tol (research from Copenhagen Consensus 2008)
"Very Poor"	13	$0.50 Global CO_2 Tax	Carbon Mitigation by Tol
	14	$3 Global CO_2 Tax	Carbon Mitigation by Tol
	15	$68 Global CO_2 Tax	Carbon Mitigation by Tol

Nearly 20 years after the so-called "Earth Summit" in Rio de Janeiro (which produced the first international agreement to limit emissions of greenhouse gases) and 12 years after the Kyoto summit (whose equally lofty goals have gone almost entirely unmet), it seems clear that no major industrialized power has the political will to impose the draconian carbon taxes or order the massive short-term carbon cuts it would take to markedly lower carbon emissions.

An examination of the economics of this approach helps to explaining this failure – and to account for this policy option's low ranking by the Copenhagen Consensus on Climate's Expert Panel.

For the Copenhagen Consensus on Climate, prominent climate economist Professor Richard Tol, who has been a contributing, lead, principal, and convening author for the Intergovernmental Panel on Climate Change's working groups, analyzed the benefits and costs of cutting carbon now versus cutting it in the future.

His paper[2] for the project found that cutting early would cost $17.8 trillion, whereas cutting less across the entire century would cost just $2 trillion. Nonetheless, the reduction in CO2 concentration – and hence temperature – in 2100 will be ***greater*** from the future reductions. Cutting emissions now is much more expensive, because there are few, expensive alternatives to fossil fuels. Our money simply doesn't buy as much as it will when green energy sources are more cost-efficient.

2 Available for download atwww.fixtheclimate.com and part of a forthcoming book by Cambridge University Press

Tol strikingly showed that grand promises of drastic, immediate carbon cuts – reminiscent of the call for 80% reductions by mid-century that some politicians and lobbyists make – are an incredibly expensive way of doing very little good.

Major industrialized nations – the G8 – have promised to use carbon emission cuts to limit global warming to no more than 2 degrees Celsius above pre-industrial levels.

Tol showed that achieving the target would require a high, global CO_2 tax starting at around $68 per ton. Based on conventional estimates, this ambitious program would avert much of the damage of global warming. However, Tol concludes that a tax at this level could reduce world GDP by a staggering 12.9% in 2100—the equivalent of $40 trillion a year. Despite the fact that we will also avoid damages from climate worth some 2-5% of GDP towards the end of the century, the costs will hit much sooner and much harder, meaning that for each dollar spent on the 'solution', we will avoid only about 2 cents of climate damage.

Governments should make long-term commitments to invest in energy-technology research and development

Tol's cost figures are based on projections from all of the major economic models of the Stanford Energy Modeling Forum. Around half of the models actually found it impossible to achieve the target of keeping temperature rises lower than 2 degrees Celsius with carbon cuts; the $40 trillion price-tag comes from those models that could do so.

It is, in fact, an optimistic cost estimate. It assumes that politicians everywhere in the world would, at all times, make the most effective, efficient choices possible to reduce carbon emissions, wasting no money whatsoever. Dump that far-fetched assumption, and the cost could easily be ten or 100 times higher.

To put this in the starkest of terms: drastic carbon cuts would hurt much more than climate change itself. Cutting carbon is extremely expensive, especially in the short-term, because the alternatives to fossil fuels are few and costly. Without feasible alternatives to carbon use, we will just hurt growth. This is made especially stark in the Advice for Policymakers chapter, A Technology-led Climate Policy, by Isabel Galiana and Professor Chris Green. They offer a smarter alternative: governments should make long-term commitments to invest in energy-technology research and development, financed by a slowly rising 'carbon tax' to promote low-carbon technologies over the next century. We need an energy-technology revolution, Galiana and Green persuasively argue, and it has not yet started.

Dr Valentina Bosetti looks more closely at one of the technologies that we must develop, in her chapter, A Focus on Carbon Capture and Storage. And Lee Lane and Dr J Eric Bickel argue in their chapter, Solar Radiation Management and Rethinking the Goals of COP-15, that more spending on climate engineering technology would establish the risks, ramifications and possibilities of this technology that – on paper – could delay much of warming's effects in the short-term, for a very low cost.

For twenty years, we have made very little progress on responding effectively to global warming. We must not be the generation that wastes another decade making grand promises, only to realize in ten or twenty years that once again we have failed to make any real progress.

Together, the papers presented here offer fresh thinking on the smart ways to respond to global warming. As politicians continue their scramble to replace the Kyoto Protocol with another binding agreement on carbon emission reductions, this is very timely.

SOLAR RADIATION MANAGEMENT AND RETHINKING THE GOALS OF COP-15

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Solar Radiation Management and Rethinking the Goals of COP-15

Lee Lane and J. Eric Bickel

Introduction

The authors' recent research paper, "An Analysis of Climate Engineering as a Response to Climate Change" (2009) concluded that one type of climate engineering, called solar radiation management (SRM), offered much larger potential net benefits than did the other, air capture of carbon dioxide.

SRM aims at offsetting the warming caused by the build-up of man-made greenhouse gases (GHGs) in the atmosphere by reducing the amount of solar energy absorbed by the Earth. GHGs in the atmosphere absorb long-wave radiation (thermal infrared or heat) and then radiate it in all directions—including a fraction back to Earth's surface. This creates rising temperatures. SRM does not attack the higher GHG concentrations that produce warming. Rather, it seeks to reflect back into space a small part of the Sun's incoming short-wave radiation.

SRM aims at offsetting the warming caused by the build-up of man-made greenhouse gases (GHGs) in the atmosphere by reducing the amount of solar energy absorbed by the Earth.

In this way, temperatures are lowered even though GHG levels are elevated. At least some of the risks of global warming can thereby be counteracted (Lenton and Vaughan, 2009).

Reflecting into space only one to two percent of the sunlight that strikes the Earth would cool the planet by an amount roughly equal to cancelling out the warming that is likely from doubling the pre-industrial levels of greenhouse gases (Lenton and Vaughan, 2009). Scattering this amount of sunlight appears to be possible.

Several SRM concepts have been proposed. They differ importantly in the extent of their promise and in the range of their possible use. At least two such concepts appear to be promising at a global scale: marine cloud whitening and stratospheric aerosols.

Marine Cloud Whitening

One current proposal envisions producing an extremely fine mist of seawater droplets. These droplets would be lofted upwards and would form a moist sea-salt aerosol. The particles within the aerosol would be less than one micron in diameter. These particles would provide sites for cloud droplets to form within the marine cloud layer. The up-lofted droplets would add to the effects of natural sea salt and other small particles, which are called, collectively, cloud condensation nuclei (Latham *et al.*, 2008). The basic concept was succinctly described by one of its developers:

> "Wind-driven spray vessels will sail back and forth perpendicular to the local prevailing wind and release micron-sized drops of seawater into the turbulent boundary layer beneath marine stratocumulus clouds. The combination of wind and vessel movements will treat a large area of sky. When residues left after drop evaporation reach cloud level they will provide many new cloud condensation nuclei giving more but smaller drops and so will increase the cloud albedo to reflect solar energy back out to space." (Salter *et al.*, 2008)

The long white clouds that form in the trails of exhausts from ship engines illustrate this concept. Sulfates in the ships' fuel provide extra condensation nuclei for clouds. Satellite images provide clear evidence that these emissions brighten the clouds along the ships' wakes.

The plan's developers conceive of a highly innovative integration of several advanced technologies. Thus, the energy needed to make the spray is provided by wind power. One key to the system is the wind-driven rotor system developed in the early 20th century by Anton Flettner. This system allows the ships to be

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powered by wind but to avoid the high handling and maintenance costs of sails. It also promises superior handling. The vessels would be unmanned and be guided by a satellite-based navigations system.

Analyses using the general circulation model of the Hadley Center of the UK Meteorological Office suggest that the marine clouds of the type considered by this approach contribute to cooling. They show that augmenting this effect could, in theory, cool the planet enough to offset the warming caused by doubling atmospheric GHG levels. It appears that a relatively low percentage of the total marine cloud cover would have to be enhanced in order to achieve the desired result. A British effort is developing hardware with which to test the feasibility of this concept (Bower *et al.*, 2006).

Stratospheric Aerosols

Inserting aerosols into the stratosphere is another approach. The record of several volcanic eruptions offers a close and suggestive analogy. The cooling from the large Pinatubo eruption (about .5 degrees Celsius) that occurred in 1991 was especially well-documented (Robock and Mao, 1995). Such eruptions loft particles into the atmosphere. There, the particles scatter back into space some of the sunlight that would otherwise have warmed the surface. As more sunlight is scattered, the planet cools.

Injecting sub-micron sized particles into the stratosphere might mimic the cooling effects of these natural experiments. Compared to volcanic ash, the particles would be much smaller in size. Particle size is important because small particles appear to be the most effective form for climate engineering (Lenton and Vaughan, 2009). Eventually, though, even the smaller particles would descend into the lower atmosphere. Once there, they would precipitate out. "The total mass of such particles would amount to the equivalent of a few percent of today's sulfur emissions from power plants" (Lane *et al.*, 2007). If adverse effects appeared, most of these effects would be expected to dissipate once the particles were removed from the stratosphere.

Sulfur dioxide (SO_2), as a precursor of sulfate aerosols, is a widely discussed candidate for the material to be injected. Other candidates include hydrogen sulfide and soot (Crutzen, 2006). A fairly broad range of materials might be used as stratospheric scatterers (Caldeira and Wood, 2008). It might also be possible to develop engineered particles. Such particles might improve on the reflective properties and residence times now envisioned (Teller *et al.*, 2003).

As a matter of logistics, the challenge seems large, but manageable. The volumes of material needed annually do not appear to be prohibitively large. One estimate is that, with appropriately sized particles, material with a combined volume of about 800,000 m^3 would be sufficient. This volume roughly corresponds to that of a cube of material of only about 90 m on a side (Lane et. al., 2007). The use of engineered particles could, in comparison with the use of sulfate aerosols, potentially reduce the mass of the particles by orders of magnitude (Teller *et al.*, 2003).

Several proposed delivery techniques may be feasible (NAS, 1992). The choice of the delivery system may depend on the intended purpose of the SRM program. In one concept, SRM could be deployed primarily to cool the Arctic. With an Arctic deployment, large cargo planes or aerial tankers would be an adequate delivery system (Caldeira and Wood, pers. comm., 2009). A global system would require particles to be injected at higher altitudes. Fighter aircraft, or planes resembling them, seem like plausible candidates. Another option envisions combining fighter aircraft and aerial tankers, and some thought has been given to balloons (Robock *et al.*, 2009).

It appears that a relatively low percentage of the total marine cloud cover would have to be enhanced in order to achieve the desired result.

Deploying SRM might yield large net benefits

Initial estimates of benefits and direct costs

The estimated benefits of marine cloud whitening greatly exceed the estimated direct costs of deploying this technology. Table 1 illustrates the potential net benefits of marine cloud whitening.[1] If deployment were to begin in 2025, over the next 200 years, the present value of the benefits would exceed that of the direct costs by at least $6.3 trillion (in 2005 dollars). The gap might be as much as $14 trillion.

The estimated benefits of marine cloud whitening greatly exceed the estimated direct costs of deploying this technology.

These numbers reflect the estimated results if SRM were linked to a global system of optimal GHG controls. Less-than-optimal controls, or no controls, would decrease global economic welfare, but actually increase the positive contribution of SRM.

Table 2 provides the comparable figures for stratospheric aerosol injection. These results are only slightly less striking. With a start date of 2025, in present value terms, SRM by this method would yield a surplus of benefits over direct costs of at least $6 trillion. The upper bound estimate of the gap is $13.3 trillion.

The estimated benefits of marine cloud whitening greatly exceed the estimated direct costs of deploying this technology.

The ranges in the estimated surpluses of benefits over costs result because the underlying analysis looked at combining GHG controls with different levels of SRM. Within the range examined by this paper, each increment of SRM adds to the benefits, but by decreasing amounts.

Methodology

SRM might generate benefits in two ways. First, it could lower damage from climate change. Second, by lessening the harm expected from climate change, SRM might also allow a slower, less costly pace for the introduction of greenhouse gas controls. A benefit-cost analysis of SRM must, therefore, account for both kinds of savings.

1 In all tables, costs and benefits reflect 200-year present values under optimal controls scenarios (as determined by DICE) with a market discount rate of 5.5% (real).

Table 1: Costs and Benefits of Marine Stratiform Cloud Albedo Enhancement (50% Seeding) beginning in 2025 *(in billions of 2005 $)*

Net Benefits	PV of Costs	PV of Benefits
6,299 – 13,994	0.9 – 5.8	6,300 – 14,000

Table 2: Costs and Benefits of Stratospheric Aerosol Injection (Naval Rifles) beginning in 2025 *(in billions of 2005 $)*

Net Benefits	PV of Costs	PV of Benefits
6,070 – 13,320	230 – 680	6,300 – 14,000

The estimated economic benefits of SRM discussed above were, as a first approximation, viewed as independent of the specific technology used. Thus, benefits can be calculated for a generic SRM system. The analysis that is being described here estimated the direct benefit of reducing radiative forcing by 1, 2, or 3 watts per square meter.

This analysis uses the DICE-2007 model (Nordhaus, 2008) to estimate benefits. DICE is a well-established integrated-assessment climate change model. DICE allows an estimate of the impact of SRM on key policy variables such as emissions control rates and carbon taxes. Recent meta-analysis has confirmed that one of DICE's primary outputs, the social cost of carbon, is in the "mainstream" of peer-reviewed estimates (Tol, 2008). DICE, like any model, is of course necessarily an imperfect reflection of reality.

The generic approach used to assess benefits will not work for the task of estimating the direct costs of SRM. Direct costs are those of developing and deploying SRM technology. (Indirect costs are those that might flow from unwanted effects on climate or other valuable human or natural systems.) Costs will differ from one SRM concept to another. This analysis bases its estimates of the direct costs of SRM on previously published studies. For stratospheric aerosol injection, its estimate rests in large measure on updating the estimates of a U.S. National Academy of Sciences study done in 1992. The cloud whitening cost estimates are based on Lenton and Vaughan (2009), Latham *et al*. (2008), and Salter *et al*. (2008). In both cases, the analysis then scales these costs to reflect the three possible levels of deployment.

Further potential benefits of SRM

Some of the benefits of SRM will fall outside the scope of this methodology. For instance, some grounds exist for fearing that current models understate the risks of extremely harmful climate change (Weitzman, 2007). Emission controls, even if they could be implemented effectively, ***i.e***. globally, require more than a century before actually cooling the planet (IPCC, 2007).

SRM, however, would stand a much better chance of preventing the worst should such a nightmare scenario begin to unfold. Once developed, either of the two techniques discussed here could be deployed very rapidly. The low costs of SRM mean that a few nations working together, or even a single advanced state, could act to halt warming, and it could do so quickly (Barrett, 2009).

Merely developing the capacity to deploy SRM, therefore, is like providing society with a climate change parachute that could greatly reduce or possibly eliminate the risk of abrupt change. And like a real parachute, having it may be valuable even if it is not actually deployed. Still, the more one credits the risk of rapid, highly destructive climate change, the greater is the potential value of SRM.

Important uncertainties remain

SRM could, then, offer important help in reducing some of the risks of climate change, but it poses some risks as well.

Concerns about possible indirect costs

Many of the risks that have been attributed to SRM are somewhat poorly defined (Smith, 2009). Some, however, are clear enough, at least in concept. One such risk is the possible lessening of rainfall. The strength of the Indian or African monsoons is a particular worry. Other concerns also exist. For example, until chlorine concentrations return to levels present in the 1980s, sulfate aerosols added to the stratosphere may retard the ozone layer's recovery (Tilmes *et al*., 2008).

Concerns have also arisen over acid precipitation if SO_2 were injected into the stratosphere. In addition, stratospheric aerosol injections would whiten skies, interfere with terrestrial astronomy, and reduce the efficiency of some kinds of solar power (Robock, 2008). Finally, some analysis suggests the possibility of "rebound warming" should SRM be deployed for a long time period and then halted abruptly (Goes *et al*., 2009).

Viewing indirect costs in a larger perspective

Several points about the above concerns warrant attention.

None of the possible ill-effects of SRM has been monetized. Therefore, how they compare with SRM's apparently large potential benefits is unclear. In fact, the scale of the effects of these unintended consequences is highly speculative. With regard to the Indian monsoon, for example the underlying climate science is too uncertain to assess the scale of the changes with confidence (Zickfeld *et al*., 2005). Thus, Rasch *et al*. (2008), on which Robock is an author, observe:

> "Robock *et al*. (2008) have emphasised that the perturbations that remain in the monsoon regions after geoengineering are considerable and expressed concern that these perturbations would influence the lives of billions of people. This would certainly be true. However, it is important to keep in mind that: (i) the perturbations after geoengineering are smaller than those without geoengineering; (ii) the remaining perturbations are less than or equal to 0.5 mm d^{-1} in an area where seasonal precipitation rates reach 6–15 mm d^{-1}; (iii) the signals differ between the NCAR and Rutgers simulations in these regions; and (iv) monsoons are a ***notoriously*** difficult phenomenon to model [Annamalai *et al*., 2007] [emphasis in original].

In a somewhat similar vein, Rasch *et al*. (2008) note that while ozone depletion may in fact take place, the attenuation of ultraviolet-B radiation by the sulfate cloud may offset this effect's impact on human health. Similarly, with regard to acid deposition, a recent study concluded that "…the additional sulfate deposition that would result from geoengineering will not be sufficient to negatively impact most ecosystems, even under the assumption that all deposited sulfate will be in the form of sulfuric acid" (Kravitz *et al*., 2009).

On rebound warming, the significance of the problem is, again, unclear. For the effect to be large, the SRM regime would have to remain in place for at least several decades. During this period, SRM would have to perform so well that adaptation and GHG control efforts would be held to low levels (Bickel and Lane, 2009). Then something would have to convince each and every major nation on Earth abruptly to halt SRM. *Ex ante*, such a course of events may be possible, but it hardly seems a likely pattern of international behavior.

...the relevant choice before us is not between a climate-engineered world and a world without climate change; rather, it is between the former and the world that would prevail without climate engineering.

All of these factors doubtless warrant study. Nonetheless, to take a step back from the details, a few broader factors should also be kept in mind. ***Most importantly, it is worth noting that the relevant choice before us is not between a climate-engineered world and a world without climate change; rather, it is between the former and the world that would prevail without climate engineering.*** SRM may, indeed, do some harm. As a result, society may simply have to choose between accepting these risks on the one hand and running the risk of a planetary emergency on the other.

Finally, governments should probe the side-effects of all options with equal rigor. GHG controls, for instance, may imply serious risks from greater reliance on biofuels or nuclear power. Border tax adjustments may unleash a global trade war (Barrett, 2007). In weighing the relative priority of SRM and GHG control, these factors are no less relevant than SRM's impacts on rainfall or ozone. The point is not that SRM's possible indirect costs should be slighted. They should not be. At the same time, a fair comparison also demands full exploration of the indirect costs of GHG controls.

Approaches to limiting the risks of SRM

R&D as a risk reduction strategy

The relevant question is: how do the potential risks of SRM compare with its possible benefits? Only an R&D program can buy the information needed to make a more knowledgeable comparison, and the potential benefits of SRM appear to be very large compared to the costs of such an R&D effort. Advances in climate science could lower the risks of SRM deployment (Goes *et al*., 2009), and a vigorous, but careful, R&D program could contribute to such advances. Such an effort might both identify faulty concepts and find new means of avoiding risks.

Such an R&D program would begin with modeling and paper studies, move to laboratory testing, and eventually embark on field trials. The latter would start small and increase in scale by increments. As R&D progresses, spending would increase from tens of millions of dollars in early years to the low billions of dollars later. Total spending may fall in the range of $10-15 billion (Bickel and Lane, 2009). The work would stress defensive research ***i.e***. research designed to identify and limit possible risks. A recent report has defined this type of research agenda for stratospheric aerosols (Blackstock *et al*., 2009).

One or more large field tests will almost certainly be required before full deployment would be either possible or desirable. One candidate for such a field test is an experiment in cooling the Arctic region (Caldeira and Wood, 2008). Field tests will have to be conducted over at least a few years because the effects of a prolonged intervention may differ from those of a brief one. Testing would require investigating possible links between SRM and any anomalies that may appear (Caldeira and Wood, pers. comm., 2009).

The inference seems clear: a fairly long period is likely to ensue between the launch of an R&D effort and the earliest time at which a system might confidently be deployed. The more likely it is that high-impact climate changes might appear within the next few decades, the greater the need would be for making an early start on R&D. It is quite true that research cannot entirely eliminate risk (Smith, 2009). Yet the risk of deploying a system under emergency conditions and without full testing are clearly higher than those entailed by deploying a more fully tried and better understood system.

Delayed deployment as a risk management strategy

Another option might be to develop SRM but to delay its deployment. The analysis reported here considered an option of delaying deployment until 2055. This approach offers two advantages.
First, delay is likely to make it easier for the nations wishing to deploy SRM to gain international agreement for their plans. Today, some nations may still benefit from additional warming. Some of those nations might strenuously object to near-term efforts to halt warming. Russia, one of the nations that might adopt this view, is a great power. It could probably apply enough pressure to prevent any other nation from deploying SRM. However, as decades pass, climate change is increasingly likely to threaten even Russia with net costs. As this happens, Russian and other objections to SRM are also likely to fade.

Second, the ozone depletion problem will also diminish with time. The stock of ozone depleting chemicals in the atmosphere is shrinking. Before mid-century, levels will return to those that prevailed pre-1980. At that point, the impact of stratospheric aerosols on UV radiation also looses significance (Wigley, 2006).

The more likely it is that high-impact climate changes might appear within the next few decades, the greater the need would be for making an early start on R&D.

Delayed deployment, of course, would also lower the difference between SRM's total benefits and its direct costs. Even so, large net benefits remain. This result obtains for both SRM concepts. Thus, as illustrated in Table 3, if marine cloud whitening were deployed in 2055, estimated benefits would exceed direct costs by at least $3.9 trillion and perhaps as much as $9.5 trillion.

If stratospheric aerosols were deployed in 2055, as illustrated in Table 4, the gap between total benefits and total costs would range between $3.8 trillion and $9.3 trillion.

Again, if SRM is linked to no controls or to suboptimal controls, like placing a 2 degree Celsius ceiling on warming, the value of SRM would be still greater.

Table 3: Costs and Benefits of Marine Stratiform Cloud Albedo Enhancement (50% Seeding) beginning in 2055 *(in billions of 2005 $)*

Net Benefits	PV of Costs	PV of Benefits
3,899 – 9,498	0.3 – 1.8	3,900 – 9,500

Table 4: Costs and Benefits of Stratospheric Aerosol Injection (Naval Rifles) beginning in 2055 *(in billions of 2005 $)*

Net Benefits	PV of Costs	PV of Benefits
3,830 – 9,290	70 – 210	3,900 – 9,500

Conclusion: Implications for COP-15 and beyond

As nations consider the choices posed by COP-15 and the years that will follow, they need to begin to consider the option of exploring SRM. Several implications seem clear:

First, SRM offers a very large upside potential for reducing the total damages from future climate change and the costs of controlling that damage. If, as seems certain, GHG controls are limited to just a few nations, or are based on non-optimal targets, or are poorly structured, the potential value of SRM rises still higher. At this point no other option on the horizon appears to offer such large rewards for such modest costs.

Second, should a climate emergency occur, one or more nations would be very likely to resort to SRM. In that case, the knowledge gained by a careful and incremental research program would be invaluable. Since such a program will take time to complete, it should begin soon. The greater is the risk that climate change might cause severe harm before, or shortly after, mid-century, the stronger is the rationale for making an early start in exploring SRM.

The search for new knowledge about all the means of countering climate change should be central to a larger effort.

Third, to counter the compound and changing nature of the threat, the world needs a compound response that will evolve over time. At any one time, multiple actions will be needed. The key questions are: how much of each action is called for now, and how should this mix of actions change as the problem develops, new knowledge emerges, and society evolves? Adaptation and GHG controls are important parts of a total response. ***The search for new knowledge about all the means of countering climate change should be central to a larger effort***. R&D on SRM deserves to be a high priority part of this quest for new knowledge. The current COP-15 talks remain focused on a relatively narrow range of responses. Climate change demands a new and more dynamic strategic vision.

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A TECHNOLOGY-LED CLIMATE POLICY

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A Technology-led Climate Policy

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Introduction

Humankind is changing the earth's energy balance. It is doing so by releasing large amounts of greenhouse gas (GHGs) emissions into the atmosphere. While it is still a matter of scientific debate how much the build-up will alter our climate, there is little doubt that at least some change will occur, with potentially serious ecological, social and economic implications. Over the past year, Yvo de Boer, the UNFCCC's executive secretary, has reiterated the "need for ambitious emission reduction targets for industrialized countries", and it is likely that COP15 discussions will revolve around these. At COP15 and beyond, delegates will be asked to evaluate potential means, costs and impacts of mitigation and adaptation. They will be asked to determine the best way of steering economic growth towards a green, low-emission future.

A comprehensive policy that includes energy goals, and incentives to meet these goals with clean technologies, is imperative.

Mitigation and by extension climate stabilization may be achieved through two channels:

- Targeting per period emissions and thus economic development; or
- Targeting cumulative long-term emissions through the development of low-carbon technologies.

Typically, policies that target technologies have been proposed as complements to emission targeting systems such as cap-and-trade or taxation policies. This policy advice paper draws from and adds to the authors' "An Analysis of a Technology-led Policy Response to Global Warming" (2009) which inverts the relative importance of these two complementary policy tools.

We suggest that in order to create a politically feasible and economically sensible policy, one that does not impose extreme restrictions on growth in either developing or developed countries, requires focusing on reducing the carbon intensity of economic activity through the development and diffusion of low-carbon technologies.

A comprehensive policy that includes energy goals, and incentives to meet these goals with clean technologies, is imperative.

Given the large proportion of emissions that will be coming from the developing countries it is crucial that an effective climate policy addresses the world's energy technology and economic growth needs.

At the same time, a technology-led proposal addresses another concern: that emissions be reduced sufficiently to avoid great harm. Using the fact that it is not the flow of greenhouse gases, but rather the stock of these gases in the atmosphere that matters for climate change, we show that investing heavily now in research development and diffusion is not only cost-effective but environmentally effective as well.

Structure of a Technology-led Policy

A technology-led climate policy would focus on the technological drivers of GHG emissions rather than on the emissions themselves. The centerpiece of a technology-led policy is commitments to invest in:

- Basic research and development of new low-carbon energy technologies, followed by testing and demonstration;
- Researching and developing technologies that would "enable" the increased scalability of current technologies, such as utility-scale storage for intermittent solar and wind energy;

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- Researching and developing ways to reduce the costs of current technologies; and
- Energy-related infrastructure, such as smart grids.

Our technology-led policy also includes a variant on carbon pricing. We would use a carbon price in two ancillary but important ways. First, the purpose would be to raise revenues to finance the energy R&D. A $5.00 charge levied on each tonne of CO2 emitted could raise $150 billion annually, $30 billion in the US alone. Second, over time, the carbon charge (or fee) would be allowed to gradually rise, doubling, say, every 10 years. In this way the carbon price would send a "forward price signal" to deploy new, scalable, cost effective, low-carbon technologies as they reach "the shelf" (when they are ready). As new technologies become available, the carbon price would make their implementation increasingly profitable to CO2 emitting industries and entities.

The proposal amounts to a strategy that reduces emissions by investing in energy R&D and providing incentives for deployment of the fruits of successful innovation.

Why a Technology-led Policy?

There are five major reasons for adopting a technology-led climate policy.

Magnitude of the energy technology challenge

This challenge is certainly much greater than is widely appreciated. Here is an illustration:

Suppose by 2100 we wish to reduce global emissions by 75% from current levels. Suppose further, that over the course of the 21st century, the "trend" rate of global GDP growth in the absence of climate policy were 2.3%. To achieve the emission reduction target and not lose more than 11% of the cumulative output that would otherwise flow from a 2.3% per annum growth in global economic activity, would require that by 2100: global energy intensity is reduced by two thirds from the level in 2000, and carbon emission-free energy in 2100 is two and a half times greater than the level of **total** energy consumed globally in 2000. (In 2000, global energy consumption was~420EJ/yr, 85% of which was supplied by fossil fuels. Of the carbon-free energy produced, 95% was nuclear and hydroelectric.)

State of current low-carbon energy technologies

A stock-take of current low-carbon energy technologies shows that these are not nearly ready or up-to-the-task and new ones will be needed as well:

Sites for **hydroelectric power** are limited.

Conventional **nuclear** energy faces important resource and waste storage limits. A bigger contribution from nuclear awaits the development of "breeder reactors" that create and reuse their own fuels, greatly reducing resource supply and waste storage constraints, but posing proliferation and security (terrorist) problems. A potentially important means of avoiding these problems is the "integral fast reactor" (IFR), the further development of which awaits re-activation after a fifteen year hiatus.

The proposal amounts to a strategy that reduces emissions by investing in energy R&D and providing incentives for deployment of the fruits of successful innovation.

Carbon Capture and Storage is important because of the large number of coal-fired plants currently operating. However, ramping up CCS will be slow for technological and geological investigation reasons. Currently, capturing carbon dioxide emissions from existing coal-fired plants is very energy-intensive. For new plants, there is still work to be done on gasification technology. Geologically, much investigation is required to assure that depositories won't leak. Capturing CO2 from the air is possible, but requires technological breakthroughs.

Biomass in its "first generation" forms (such as corn ethanol and soybean-based bio-diesel) is unlikely to produce large amounts of (net) carbon neutral energy. The realization that "first generation" biofuels greatly increase water use but may do little, on a "life-cycle" basis, to reduce emissions (or energy use) has led in two directions: a focus on biomass as a solid energy source for generation of electricity rather than as a liquid biofuel for use in vehicles; R&D into the possibility of "second generation" biofuels produced from cellulosic by-products of primary feedstocks, switchgrass, and algae, or from other biological matter.

Solar and wind currently supply only a fraction (less than 1 %) of the world's energy. The reasons go beyond their higher costs (attributable to low conversion efficiencies and material costs) to include current technological limitations centering on their intermittency and variability (requiring large scale storage as well as "smarter" grids), and diluteness (requiring a high degree of land-use). As a result, a substantial contribution from solar and wind awaits a number of "enabling" technology breakthroughs.

...much work needs to be done to make existing low-carbon technologies sufficiently scalable and competitive

In those few areas (such as Iceland) where hot springs are abundant, **geothermal power** is an excellent source of power. A much greater contribution requires technology breakthroughs including ones in materials science that would allow much deeper drilling to tap the greater heat found deeper in the earth. Geothermal energy for space-conditioning new residential and commercial buildings has a substantial long-term potential.

Ocean Wave Energy faces technological hurdles. The amount of energy in the oceans' waves is large, but it is very dilute, and only a fraction is economically viable.

Hydrogen is a "carrier" of energy (like electricity), not a primary source of energy. For hydrogen to play a much larger role in the energy picture, technological breakthroughs will be needed in production, storage and fuel cell technologies.

It is clear that much work needs to be done to make existing low-carbon technologies sufficiently scalable and competitive to substantially displace carbon-emitting ones. Entirely new technologies such as nuclear fusion will be needed to complete the job.

Limits to which the market can induce technological change

Among the hurdles that face the market are the inability to appropriate the benefits of basic R&D; the uncertainty of success of such R&D; and the relatively distant (measured in decades rather than years) payoffs from successful innovation.

As well, there is evidence that the induced technology mechanism is weak. Moreover, consumers are not likely to be willing to pay the high carbon prices that might make the induced technology mechanism operative.

In addition, Montgomery and Smith (2007) have made a persuasive argument that because current governments cannot tie the hands of future governments to cover anything more than the cost of production of technologies that turn out to be successful, current investors in R&D have no reason to believe that their up-front and risky investments in R&D will be compensated in the future.

'Chicken and egg' problem between carbon pricing and technology

High carbon prices might be acceptable, but only if there were good low-carbon energy technology substitutes available. But new technologies take time to develop. Investments in these will not yield payoffs for at least several years, if not a few decades. Implementing high carbon prices now on the promise of technology payoffs in the future is a hard political sell.

Furthermore, because carbon pricing would fall most heavily on energy-intensive industries, there are important consequences. In a number of developed countries energy-intensive industries are important parts of the production and employment base. In many newly developing countries very energy intensive materials play an important role in the development process. This is particularly true of highly populous developing countries experiencing rapidly rising incomes and urbanization. As high rise buildings replace shanties, the demand for cement, structural steel, flat glass, copper and/or aluminum, all of which are highly energy-intensive, rises rapidly.

These materials are also needed to construct the streets, bridges, over and under passes, subways, railroads and airports that provide intra-urban and inter-urban mobility, and water and sewage systems. In these circumstances, there will be resistance to any means that place a high "tax" on energy use, unless good alternative energy sources are readily available at a reasonable price. Here we clearly have a chicken and egg problem, with the carbon price "chicken" up against the technology "egg".

To use a different analogy: leading with carbon pricing is like a boxer 'leading with his chin'. If it isn't a knockout, the fight is likely to be stopped before proceeding very far.. Wisdom suggests that technology development should lead with carbon pricing playing the ancillary, albeit important, roles set out in the technology-led proposal.

'Brute Force' approach is infeasible

Policies that impose emission reduction targets without paying attention to the capabilities of existing technologies (we term these "brute force" policies) would be prohibitively costly, and would almost surely fail to gain political support.

Attempting to achieve deep emission reductions by "brute force" would exact very high economic costs –and even then the policy may fail to achieve its objectives. For example, unless we quickly undergo an energy technology revolution, it will be very difficult, probably impossible, to meet the widely discussed G-8 goal of slashing global carbon emissions 50% by 2050—a goal that would require developed country emission reductions of up to 80%.

Attempting to meet such goals by "brute force" could result in economic costs ten times or more higher than the 1-3% of GDP estimates that have been widely reported.

How would a Technology-led Policy Work?

A technology-led policy focuses on the technological drivers of carbon emissions rather than on the emissions themselves. Deep reductions in emissions require an understanding of the factors that cause them: population growth; increased economic well-being reflected in rising income per capita; and the nature of the technologies that provide the energy on which our civilization and well-being depend.

There are two major elements to a technology led climate policy. The first is that it focuses on reducing the carbon intensity of economic activity via improvements in energy efficiency (reductions in energy intensity), and the carbon content of energy via development of low-carbon energy supplies. A technology-led climate policy would measure the return to R&D in terms of its contribution to reducing the carbon intensity of economic activity.

Secondly, by reducing the costs of low-carbon energy technologies, and committing to a slowly rising price of carbon, a technology-led policy provides incentives to transition away from carbon emitting technologies to low-carbon ones.

Both private and public investment in R&D will be needed. Although we focus on the latter, we assume there will be growing private interest (as there already is) in investments in carbon-free energy R&D, especially at the applied development and commercialization stages. Here, however, we focus on the means of assuring that the publicly funded portion of R&D is well-spent, and that the incentives generated are compatible with the goal of increasingly rapid change in energy technologies.

In order to maximize the benefits from a technology-led climate policy, it is necessary to make sure the monies are used constructively rather than wastefully. It is much easier to talk about a technology policy than it is to carry out an effective one. It is simpler to spend on R&D than to assure that the monies are well spent. Greater accountability can be achieved by introducing competition in the form of prizes and/or by competing international consortia.

Further, to reduce perverse incentives, each country should put the monies in a dedicated trust fund, administered at arm's length from the executive and legislative bodies by an independent Board of Directors, with decisions on allocation of funds taken by panels of technology experts.

Quantifying the Benefits of a Technology-led Policy

Does it pass a benefit-cost test?

Three different benefit-cost analyses (BCA) were undertaken.

First, we estimated the benefits and costs of a technology-led policy against a no-policy ("business as usual") scenario. Using various technology success rates, we found that the technology-led policy passes a Benefit-Cost test—usually by substantial margins, with most benefit-cost ratios (BCRs) in the range of 2 to 10, depending on the success rate of R&D, and discount rate chosen.

The higher BCRs applied when we looked beyond 2100 to include the large benefits of avoided climate change in the 22nd century as a result of technology success in the 21st century.

A technology-led policy is a more environmentally effective and a lower cost alternative to either doing nothing or pursuing emission reduction targets as we have in the past.

Second, we pitted the technology-led policy against an emission reduction target approach. When we used targets similar to those currently being discussed for 2020 and 2050 in the run-up to the UN meetings in Copenhagen in December 2009, we found even higher benefit cost ratios in favor of a technology-led policy.

This has an important implication: it suggests that in benefit-cost terms the current emission-reduction commitment approach to climate policy without drastic technological improvement is actually worse than doing nothing. But, of course, we do need to do something! A technology-led policy is a more environmentally effective and a lower cost alternative to either doing nothing or pursuing emission reduction targets as we have in the past.

Significantly, we also tested how the technology-led policy fared when it must meet a global carbon budget, one that limits, with some significant probability, the likelihood that overall temperature increase is no more than 2°C. We found that for two of the three profiles reflecting the rate of technology success, cumulative emissions could, with 50% probability, limit global average temperature increase to 2°C. No other policy of which we are aware could plausibly achieve the same goals, and do so at relatively modest cost.

Additional advantages

A technology-led climate policy is **potentially attractive to developing countries**, especially the larger ones such as China, India, Brazil, Indonesia, Korea, and Mexico. Instead of specific emission-reduction commitments, which many of these countries have adamantly opposed, the policy focuses on technology development and deployment. Many of these countries are already pressing ahead with the development and manufacture of low-carbon technologies.

A technology-led policy **emphasizes the role of human creativity and innovation** rather than sacrifice and limits to growth. It would attract generations of young people with scientific and creative capabilities to a world-wide low-carbon energy technology race. A technology-led policy is up-beat because it rewards success rather than punishing failure.

An energy technology race could yield benefits in terms of **spillovers to non-energy technologies** and uses, just as the technologies developed to fight World War II benefited many civilian industries for decades afterwards—and still are.

A technology-led policy is consistent with the Bali Action Plan guidelines in that it is **measurable, reportable and verifiable**, and supports technological innovation and transfers.

A technology-led approach has nice **complementarities with other climate-related policies**, including: adaptation, solar radiation management, air capture and afforestation.

One of the outcomes of the Copenhagen Consensus on Climate project is that it produced a wealth of studies on the efficacy of different approaches to reducing the harm, and mitigating the causes of climate change. Viewed as a package, rather than individually, the various approaches provide a cost-effective arsenal for avoiding "dangerous climate change".

Both climate and technology change are imbued with uncertainty. As a result, the timing and extent of climate change, and the pace of technology development, not only make some adaptation inevitable, but greater adaptation may be required if, as is widely anticipated, emissions overshoot the targeted stabilization level.

Mitigation is not limited to energy-related CO_2 emissions. Some examples of "alternative mitigation" are: forest carbon sequestration, black carbon, methane mitigation, and CO_2 capture from the air. Afforestation would reduce the approximately twenty percent of carbon dioxide emissions that occur as a result of changes in land use. Methane is a powerful greenhouse gas that is in good part associated with animal husbandry, agriculture (especially rice cultivation) and landfills. Black carbon is particularly associated with inefficient use of diesel fuel. If cost-effective means of reducing these sources of carbon are available or can be found, then "alternative mitigation" is clearly both desirable and within the scope of technology-led R&D.

There may also be need for a palliative in the event of rapid climate change. The role of the "palliative" would be to limit climate change while the technological means are developed to substantially reduce emissions. One category of "palliatives" is solar radiation management (SRM). Proposals to research and develop the means of increasing the reflectivity of clouds or injecting aerosols or other reflective particles into the stratosphere to reflect away incoming solar radiation might prove useful if climate rapidly changes.

Do we have Enough Time for a Technology-led Climate Policy?

An argument that we are likely to hear is that the science of climate change indicates the world cannot wait for a technology-led policy to bear fruit. A slower pace of emission reduction than science "demands", so the argument goes, could mean huge damages, the possible transgression of "tipping points", and the "catastrophic" changes that doing so might entail.

But what is the choice? No approach to climate stabilization will work without an energy technology revolution. Further, our technology-led proposal is also driven by a sense that there are few more important things to human survival than energy. We obviously need air to breathe and water, but after these, virtually all of the requirements of life (including clean air and clean water) will depend in a highly populated world on abundant energy.

And coping with rapid climate changes that might occur if we really reach and surpass a "tipping point" will, as with survival from natural "catastrophes", such as an asteroid hitting the earth or the eruption of a "super-volcano", require all the concentrated energy at our disposal. Until alternative technologies become scalable and reliable, that means we will remain largely reliant on fossil fuels and nuclear energy. If we wish to limit our impact on climate, then we need a technology revolution that provides effective means of sequestering emissions and reliable low-carbon energies capable of displacing the technologies that currently supply 85% of global energy requirements and most of our carbon emissions.

No approach to climate stabilization will work without an energy technology revolution.

Our analysis indicates that making investments now can set off an energy technology revolution that makes possible very rapid future rates of de-carbonization a few decades hence.

We showed that these reductions could, with 50% probability, limit cumulative emissions to levels consistent with a global average temperature increase of 2°C, or less. While nothing is certain, we know of no other ***feasible*** policy that could do nearly as well.

Accountability and Measures of Performance

Does a technology-led policy have anything to offer those who believe that without emission-reduction targets there is no accountability or measure of performance? This is an important issue. We make the following points:

A technology-led climate policy is **inconsistent with date-specific emission reduction mandates**. We cannot accurately predict when technological breakthroughs will occur and when scalable, cost-effective technologies will be ready.

Technology success is likely to breed success; emission-reduction target failure is likely to breed similar failures.

But a technology-led policy **does involve commitments**: one is to provide long-term funding (preferably with a small, $5.00/t CO2 carbon charge), of research and development, testing and demonstration of new and/or more cost-effective energy technologies; another is to provide inducements (preferably by allowing the carbon charge to gradually rise) to deploy the technologies when ready.

The technology-led policy also provides **a measure of performance: the rate of decline in the carbon intensity** of economic activity (rate of "de-carbonization), with performance evaluated in terms of a rising, and eventually accelerating, rate of de-carbonization.

Another **measure of success is the number of countries actively and willingly engaged**. Here there is already evidence of success. A growing number of countries (including, China, Japan, India, Korea, Brazil, the US, and others) are becoming actively engaged in developing low-carbon energy technologies. Here we find enthusiasm in contrast to the reluctance that surrounds demanding emission reduction commitments. Why? Technology success is likely to breed success; emission-reduction target failure is likely to breed similar failures.

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A FOCUS ON CARBON CAPTURE AND STORAGE

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A Focus on Carbon Capture and Storage

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Introduction

The purpose of this paper[1] is to discuss the costs and benefits associated with researching and developing the technology of Carbon Capture and Storage[2] (CCS).

...there appears to be room to improve the design of a climate policy by including some mechanisms to promote spillovers of knowledge

Much has been written on how to reduce anthropogenic emissions with the aid of a portfolio of existing technologies. However, the stabilization of temperature to a safe level requires that over time net emissions fall to very low levels or even to zero. There is only one way that this can be achieved in a manner that is acceptable to the majority of the world's citizens: through some kind of technological revolution.

Extensive research and development (R&D) investments will be required to bring about such a breakthrough. This will be specifically important for countries interested in maintaining both a leading position in climate negotiations and a first-mover advantage in earning the rents on innovation.

Indeed, technological breakthroughs (and maybe, more importantly, the large-scale commercialization of these new technologies) will play an essential role in the competitiveness issue that has lately gained great relevance in the policy debate. On top of this, technological transfers to developing countries could be pivotal to solve the logjam affecting international negotiations. Innovation and technology treaties have been analyzed in the context of climate coalition formation, suggesting that they could improve the robustness of international agreements to control climate change (Barrett, 2003; Hoel and de Zew, 2009; Burniaux *et al*, 2009).

While it is commonly agreed that we need extensive R&D efforts to reduce emissions in an efficient manner, there is less consensus on whether relying on R&D policies alone might be sufficient to achieve the required reduction in emissions.

The argument that innovation and technology policies might be enough to solve the climate change problem has a strong appeal to policymakers. Some climate-related scientific and technology agreements have emerged, including the Carbon Sequestration Leadership Forum, the Asia Pacific Partnership on Clean Development and Climate, and the International Partnership for a Hydrogen Economy. Proposals of international technology agreements that would encompass domestic and international policies to foster R&D and knowledge-sharing, have been put forward (Newell, 2008).

However, many scholars have argued that R&D policies alone will not be sufficient to achieve stringent targets and/or to minimize mitigation costs, because such an approach would provide no direct incentives for the adoption of new technologies and, by focusing on the long-term, would miss near-term opportunities for cost-effective emissions reductions (Philibert, 2003; Sandén and Azar, 2005; Fisher and Newell, 2007; Bosetti *et al*, 2009a).

1 This policy brief was written while the author was visiting fellow at the Princeton Environmental Institute (PEI), in the framework of cooperation between CMCC and PEI. The hospitality and excellent working conditions offered there are gratefully acknowledged. The author gratefully acknowledges useful comments from Massimo Tavoni and Shoibal Chakravarty. All usual disclaimers apply.

2 When describing CO_2 in geological formations and oceans, the term "CO_2 storage" is used. It is now commonly accepted that the term CO_2 sequestration refers only to the terrestrial storage of CO_2.

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An Analysis of the Large, but Limited, Power of Innovation Policies

Recent empirical and numerical studies show that R&D investments, though essential to improve the efficiency of a climate policy, are typically induced by some carbon price signal. An extensive study based on patent counts, (Dechezleprêtre *et al*, 2008) shows how the Kyoto Protocol induced innovation in carbon-free technologies in Annex I countries that ratified the Kyoto Protocol that was not mirrored in Australia and in the USA.

In general, it is well-documented (see for example the review in Vollebergh, 2007) that environmental policy has a clear impact on invention, innovation and diffusion of technologies.

However, (Dechezleprêtre *et al*, 2008) also find that there is no evidence that the Kyoto Protocol ***increased*** the transfers or international spillovers of knowledge. That technology transfers are a crucial point in negotiations is no big news, as manifested by the creation of an Expert Group on Technology Transfer within the UNFCCC framework.[3] Hence, there appears to be room to improve the design of a climate policy by including some mechanisms to promote spillovers of knowledge (although this might be tricky as free knowledge spillovers lower the rents on innovation and thus might discourage innovators).

Many analysts have concluded that the current scale of energy R&D is inadequate for the climate challenge and propose more or less arbitrary increases to the level of effort. Both the United States and the European Commission envision large expansions of government energy R&D funding[4]. Nemet and Kammen (2007) claim that a five- to tenfold increase in American energy R&D spending is both warranted and feasible. Using a rule of thumb, Stern, 2007 recommends doubling all government energy R&D budgets.

Similarly, by using an Integrated Assessment model with a fairly detailed description of endogenous technical change in the energy sector, Bosetti *et al*., 2009b, find that energy R&D is crucial if we aim at creating a significant dent in carbon emissions. Investments in Public Energy R&D would need to return to at least the peak of the 1980s as a relative share of GDP. Expenditures should thus increase from today's 0.02 percent to 0.08 percent of world GDP, or equivalently from 8 to 40 Billion USD. These extra investments should take place in the next 20 years, given the long lags that separate research from market adoption. They look at different types of energy R&D, and find that public energy R&D should be targeted at innovative technologies that can contribute to the decarbonization of energy indispensable for significant emissions cuts. Especially the non-electric sector (transport above all) needs breakthrough technologies that are not available today. The power sector needs innovation as well, but to a smaller extent. Only if the use of existing carbon free technologies such as nuclear power, renewables or CCS is limited by socio-political constraints, is the development of alternative technologies necessary to prevent policy costs from increasing by up to 40 percent. Nonetheless, R&D may also contribute to improving the efficiency and safety of existing technologies.

...technological transfers to developing countries could be pivotal to solve the logjam affecting international negotiations.

In order to understand the potential benefit of R&D in breakthrough technologies one can estimate the additional cost of a climate policy assuming that no R&D program aimed at bringing down the cost of breakthrough technologies in both the electric and

3 See for example the Advance report on recommendations on future financing options for enhancing the development, deployment, diffusion and transfer of technologies under the Convention. Note by the Chair of the Expert Group on Technology Transfer. SUBSIDIARY BODY FOR IMPLEMENTATION. Thirtieth session. Bonn, 1–10 June 2009

4 "National Commission on Energy Policy. 2004. Ending the Energy Stalemate: A Bipartisan Strategy to Meet America's Energy Challenges. Washington, D.C.: National Commission on Energy Policy." and "European Commission. 2009. Communication from the Commission to the Council, the European Parliament, the European Economic and Social Committee and the Committee of the Regions: Towards a Comprehensive Climate Change Agreement in Copenhagen. Brussels: European Commission. p.10, section 3.3."

Table 1. Increase in climate policy costs without an R&D program aimed at breakthrough in low carbon technologies, for two climate policy targets.			
		Discount Rate	
		3%	6%
Increase in Climate Policy costs associated with the lack of a breakthrough R&D program. (Discounted Trillions 2005 USD)	550 ppm	$ 24	$ 3
	450 ppm	$ 63	$ 9.5

non electric sectors is undertaken. As a result, the costs of these new technologies would remain as high as they are today in the coming years. Breakthrough technologies would become competitive 20 years later, without an R&D program, thus diffusion and learning-by-doing mechanisms would be delayed as well. Table 1 reports figures relative to the increase in policy costs, for two different policies: a mild climate target (550 CO2 ppm) and a more stringent one (450 CO2 ppm). In both cases, and independently of the discount rate, there is a sizeable increase in policy costs due to the lack of the induced breakthrough.

...a R&D policy complementing carbon pricing could lead to visible efficiency gains, reducing policy costs by up to 10-15 percent.

One should not forget that technological change is an uncertain phenomenon. In its most thriving form, ground-breaking innovation is so unpredictable that any attempt to model the uncertain processes that govern it is close to impossible. Despite the complexities, research dealing with long-term processes (such as climate change) largely benefits from incorporating the uncertainty of technological advance. Adu-Bonnah and Baker (2008), Bosetti and Tavoni (2009), and Blanford (2009), among others, model R&D as an uncertain phenomenon. Two of the main findings of this literature are: that the optimal level of energy R&D investments should be higher in order to cope with climate change if we acknowledge the uncertainty characterizing the innovation process; a portfolio of technologies should be considered in order to hedge the risks of R&D program failures.

Additional evidence corroborating the call for R&D policies comes from the analysis of the uneven international distribution of R&D efforts and the recognition that social returns on R&D are higher than private ones. National and international R&D funds, aiming to foster technology diffusion and to overcome the various innovation market failures, such as the underinvestment in R&D in the private sector, could be extremely beneficial. As investigated in Bosetti *et al*. (2009a) a R&D policy complementing carbon pricing could lead to visible efficiency gains, reducing policy costs by up to 10-15 percent.

However essential, R&D programs will not be sufficient. As underlined in Bosetti *et al* (2009a), under fairly optimistic assumptions about the funding available for, and the returns to R&D, innovation policies alone cannot stabilize global concentration and temperature; a strong carbon price signal is indispensable. A very robust finding across a wide range of simulations is that the largest achievable reduction in cumulative emissions with respect to the baseline case is in the order of 13 to 16 percent. To put this in perspective, the reduction required to be consistent with a mild stabilization target (550 ppm CO2) would be in the order of halving cumulative emissions.

There are three basic conclusions from an analysis of energy R&D and R&D policies as a solution to climate change:

- R&D investments will be an essential part of any climate policy. This will hold true independent of how stringent the climate target is going to be; however, technological breakthroughs will be increasingly important the further we travel along a decarbonization path.
- R&D policies alone will not be sufficient, unless the goal is limited to diversify energy provision rather than significantly reduce emissions;
- When added to a carbon policy, a R&D policy (as for example an international fund for breakthrough technologies R&D) might lead to efficiency gains and could be used to improve the regional distribution of costs.

Cost-Benefit assessment of R&D in Carbon storage

Among the many technologies available in the climate mitigation portfolio, CO_2 capture and storage (CCS) is considered central because it allows the continued use of fossil fuels while reducing the CO_2 emissions produced. CCS may play an important role, especially in countries that heavily rely on coal for the generation of electricity, such as China and India. Low-carbon electricity could also have an additional value if the decarbonisation of the transport sector follows an electrification path. CCS can also play a significant role in the event that a very stringent climate policy, such as a 2°C stabilization target, is enacted. Bio-energy coupled with CCS is the only way to obtain negative emissions that might become unavoidable in the very long run.

On the other hand, unlike other technologies which present benefits unrelated to climate change (such as increasing energy security, decreasing local pollution or producing electricity at lower cost), CCS is not meaningful outside the context of a climate policy, as it otherwise represents a decrease in plant efficiency and an increase in capital and operating expenses. In addition, CCS technologies present a whole set of non-technical difficulties related to the long-term security of geologic storage and social acceptance.

CO_2 is already being captured in the oil and gas and chemical industries. Indeed, several plants capture CO_2 from power station flue gases for use in the food industry.[5] However, only a fraction of the CO_2 in the flue gas stream is captured: to reduce emissions from a typical power plant by 75 percent, the equipment would need to be 10-times larger. If capture is used to minimize CO_2 emissions from a power plant, it would add at least 1.5 US cents/kWh to the cost of electricity generation. In addition, the generating efficiency would be reduced by 10 to 15 percentage points based on current technology. The wide-spread application of this technology is expected to result in developments leading to a considerable improvement in its performance. The cost of avoiding CO_2 emissions is 40-60 US$/ton of CO_2[6] (depending on the type of plant and where the CO_2 is stored), which is comparable to other means of achieving large reductions in emissions.

5 For more references on the technical description of CO_2 capture and storage and detailed information on current R&D programs the reader is referred to IEA Greenhouse Gas R&D Programme site: http://www.co2captureandstorage.info/

Having captured the CO_2, it would need to be stored securely for hundreds or even thousands of years, in order to prevent it from reaching the atmosphere. Major reservoirs, suitable for storage, have been identified under the earth's surface and in the oceans. Work is still in progress to develop many of these options.

CO2 capture and storage (CCS) is considered central because it allows the continued use of fossil fuels while reducing the CO2 emissions produced.

As underlined in the IEA report on CO_2 Capture and Storage (2008), the next 10 years will be critical for CCS development. By 2020, the implementation of at least 20 full-scale CCS projects in a variety of power and industrial sector settings, including coal-fired power plant retrofits, will considerably reduce the uncertainties related to the cost and reliability of CCS technologies. Given that the financial resources required to support these demonstration projects cannot be obtained from the market alone, one of the most crucial factors for the development of CCS technologies is the need for government finance to support these decisive early demonstration projects. Also, some additional effort by governments in designing adequate legal and regulatory frameworks is needed, as storage of CO_2 raises issues such as liability for CO_2 leakage and property rights. A similarly important endeavor will be needed to carry out a careful campaign to inform and raise public opinion awareness, as large-scale CCS might encounter strong public resistance. We refer the reader to the IEA report on CO_2 Capture and Storage (2008), for a detailed description of R&D actually undertaken in OECD and fast-growing countries. Research projects currently in place range from the analysis of public acceptance, to the availability of sites and the risks associated with CO_2 storage, to the optimal structure of the transport network.

6 It should be noted that the actual figure is uncertain and some sources talk about 100 US$/ton of CO_2.

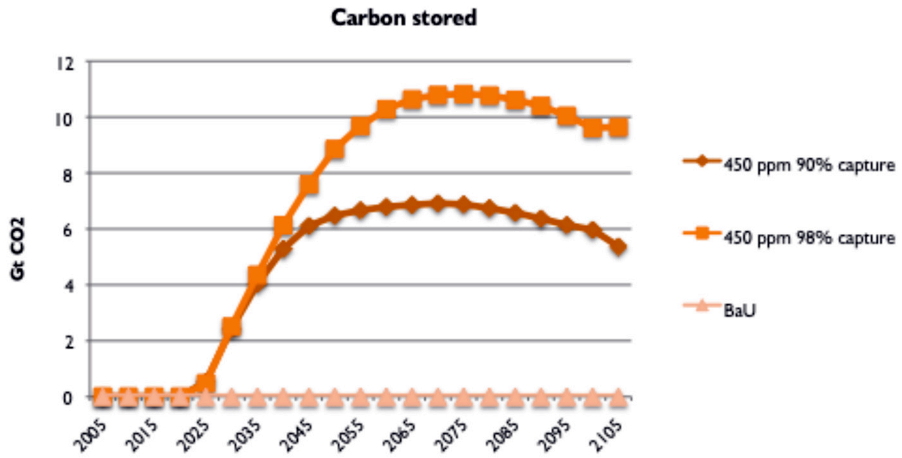

Figure 1

Keeping in mind that the demonstration part is the top priority in preparing the avenue to large scale deployment of CCS technologies, research investments, though secondary in this early stage, might play an important role later on. One important future breakthrough would for example concern the increase of the capture rate at a reasonable cost and with reasonable losses in the plant efficiency.

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Baker *et al* (2009) focus on understanding how current investment in R&D has the potential to lower CCS costs 40 to 50 years in the future. They perform an expert elicitation, to identify areas where there is potential for significant progress or even breakthroughs and then assess probability of success and failure of R&D programs in these different areas. Crucial areas of investigation are: Pre-combustion carbon capture, alternative combustion, and Post-combustion removal. They find that both post combustion and chemical looping (alternative combustion) targeted R&D programs are characterized by serious disagreement over the probability of success. They also underline that the rationale of a large R&D investment in CCS technologies strongly depends on the likelihood of implementing CCS technologies at large scale. Indeed, "if the likelihood of implementing CCS is not high, then it reduces the attractiveness of a broad R&D investment in this technology (and increases the importance of pursuing other lines of research)". The National Academy of Science study on Prospective Evaluation of Applied Energy Research and Development[7] made a first attempt to assess this likelihood, but they recognize it is a very complicated question as it involves technical issues about the viability and long-term security of geologic storage, plus a range of non-technical issues and social preferences.

Given the large sources of uncertainties we have discussed so far, both concerning the actual implementation of large CCS technologies and the probability of success of R&D programs, some heroic assumptions have to be made in order to evaluate benefits and costs of R&D in CCS technologies as a solution to climate change.

Let us assume that R&D investments can contribute to improving the capture rate of CCS technologies. Then, it is possible to provide a rough estimate of the benefits associated with such an improvement in terms of decreased policy costs and compare these with the potential costs of the R&D program itself.[8] The numerical experiment is performed using the WITCH model, an Integrated Assessment model first described in Bosetti *et al* (2006), as it

7 National Research Council. Prospective Evaluation of Applied Energy Research and Development at DOE (Phase Two). The National Academies Press, Washington D.C., 2007. http://www.nap.edu/catalog/11806.html.

8 In the short run, efforts should concentrate on the D part of the R&D pair; however, computing the benefits and costs of direct investments in CCS would be losing sight of the primary objective of this analysis, hence we look here at longer term benefits of research part.

Table 2 Technological parameters for traditional coal and IGCC-CCS power plants.

	Investment costs World average USD_{2005}/KW	O&M World average USD_{2005}/KW	Fuel Efficiency %	Load factor %	Plant Lifetime years	Depreciation %
Coal	1530	47	45%	85%	40	5.6%
IGCC-CCS	3170	47	40%	85%	40	5.6%

explicitly represents the optimal portfolio in energy technologies in the face of different climate policies. In WITCH, CCS can be applied to integrated coal gasification combined cycle power plant (IGCC-CCS). IGCC-CCS competes with traditional coal, so that it replaces it for a sufficiently high carbon price signal. CCS transport and storage supply cost curves are region specific and they have been calibrated following Hendriks *et al*. (2004). Costs increase exponentially with the capacity accumulated with this technology. The CO2 capture rate is set at 90 percent and no after-storage leakage is considered. Other technological parameters such as efficiency, load factor, investment and O&M costs are described in Table 2.

The investigation focuses on two policy scenarios where the objective is to stabilize CO2 concentration at 450 ppm and 550 ppm levels by the end of the century, respectively. For each of the two policy scenarios two cases are considered: i) the basic case, where capture rate is 90 percent; ii) the R&D enhanced case where, as a result of a R&D program, the capture rate is 98 percent without any increase in electricity costs or efficiency loss. As an example, Figure 1 shows the potential beneficial effect of a higher capture rate, ceteris paribus, on stored carbon for the 450 ppm policy case. During the second half of the century the climate target implies an increasing carbon price. The vented carbon that is not captured represents a cost for IGCC plus CCS plants; hence, being able to reduce such a pricy by-product could decisively increase the potential of CCS technologies.

The decrease in policy costs that would be associated with such a technological leap is computed and then used as a measure of the benefit of a dedicated R&D program. Table 3 reports benefits, as decreased policy costs, for two discount rates and for two policies. By considering the two policy scenarios we mimic two cases, one where damages from climate change are higher (the 450 ppm stabilization case) and a second where climate change damages are lower (the 550 ppm stabilization case).

In order to provide an estimate of the R&D program costs, it is assumed that the expenditure on the R&D program on CCS is 10 percent of the overall energy R&D bill (which is endogenously calculated by the model) and that its duration spans between 2010 and 2045. Table 4 summarizes the benefit cost ratios.

The basic message that can be derived by this very preliminary analysis is that if we place some value on the reduction of the climate change threat, then investing in an R&D program in CCS technologies passes the cost benefit tests.

Table 3. Cost Benefit Analysis of a R&D in CCS Technologies Program

		Discount Rate	
		3%	6%
Benefit as avoided policy costs (Discounted USD Trillions)	550 ppm	$ 0.48	$ 0.09
	450 ppm	$ 0.92	$ 0.20
Cost of R&D Program (Discounted USD Trillions)		$ 0.03	$ 0.02

Table 4: Benefit /Cost ratios for a R&D in CCS Technologies Program

Discount rate	Low (3%)		High (6%)	
Climate change damage	Low (550 PPM)	High (450 PPM)	Low (550 PPM)	High (450 PPM)
BCR	16	30.7	4.5	10

Many simplifications are required to perform this analysis; hence results should be taken with due caution. In particular, it should be kept in mind that cost estimates are very rough as we assumed the probability of failure of the R&D program as equal to zero. However, the gap between benefits and costs is wide. To improve on this analysis one should bear in mind the following caveats:

- Estimates do not take into account the additional benefits that result from these measures, such as the growth in markets, job creation, etc. On the other hand the extensive use of coal has many external costs, for example those associated with mining that we have not accounted for here.
- Institutional, legal and social barriers can become a major issue in the large scale deployment of CCS technologies. As we have seen, independently of the technological dimension, a large deployment of CCS might not take place.
- The analysis performed is deterministic. Baker *et al* (2009), discuss extensively of the uncertainties surrounding the effectiveness of such R&D programs. In order to diversify such risk, the portfolio of CCS R&D investments should cover different promising technologies, at least in the early stages.
- Deployment and demonstration projects are the key to bring about some reduction in costs; these are not considered in the present analysis.
- International spillovers of knowledge might speed up the breakthrough in capture technologies, thus lowering the actual costs of the R&D program, but they are not considered in the present analysis.

Conclusions

In July 2009, G8 countries reiterated their commitment to take rapid and effective global action to combat climate change. The representatives of the largest developed economies have recognized the need to set a 2°C limit to the increase in global average temperature above preindustrial levels. They have also agreed on aiming to reduce developed countries' emissions by 80 percent by 2050, and proposed a global objective of minus 50 percent by 2050.

Meeting these targets is going to require a monumental change of the energy system and of the whole economy, a change that only a series of technology revolutions can make possible. The question rests on whether technology-push or market-pull instruments will do the trick. Both instruments will be required and a hybrid policy will probably prove to be the most effective both in economic and environmental terms.

Induced and directly financed R&D investments should be diversified (over many technologies, such as solar, CCS, nuclear etc., and alternatives for each technology broad category as well, such as photovoltaic, solar thermal, etc.) as only a portfolio of investments can hedge against risks associated with the success of R&D programs. Innovation is highly uncertainty and its dynamic poorly understood, and large efforts should thus be made to improve our understanding of how to measure and how to foster innovation.

Transport is the sector where carbon free alternative technologies are the least competitive; therefore a large part of the R&D portfolio should be dedicated to existing promising technologies in order to cut the costs and start commercializing some of these technologies.

CCS technologies could bring very large benefits as they are very flexible and can be applied in different abatement contexts. If electrification of the transport sector becomes one of the major responses to the quest for the decarbonisation of transport, then CCS could play an even larger role. Finally, if CCS technologies are coupled with biomass to produce both fuels and electricity, then CCS could have a crucial role in providing negative emissions as well. Assuming that all non-technical barriers to the large scale diffusion of CCS technologies can be overcome, then investing in R&D in CCS technologies (as one of the different options in a larger portfolio) would pass the cost-benefit test.

The demonstration phase is now the top priority in preparing the avenue to large scale deployment of CCS technologies; research investments finalized at improving the capture rate and capturing costs of

CO_2, though secondary in this early stage, might play an important role later on.

Finally, one should keep in mind that stringent stabilization targets will require large shifts of investments in the energy sector and in the economy as a whole, figures which are an order of magnitude larger than R&D investments, a small, although important, portion of the overall picture.

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