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A Perspective Paper on Climate Engineering

Including an Analysis of Carbon Capture as a Response to Climate Change

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A Perspective Paper on Climate Engineering, Including an Analysis of Carbon Capture as a Response to Climate Change

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PREFACE

ABSTRACT

This response paper critiques the cost-benefit analysis of climate engineering of Bickel and Lane (2009) in two parts. First, it argues that the analysis of solar radiation management is, at best, arbitrary, and more critically, not grounded in a realistic set of assumptions about how the global earth system actually works. The result is an analysis that is precise but not accurate. Second, it summarizes an analysis of the potential role for air capture technologies to play in the decarbonization of the global economy, finding the costs of air capture to be directly comparable with major global assessments of the costs of conventional mitigation policies. The paper concludes, as does Bickel and Lane (2009) that there is justification for continued research into technologies of solar radiation management, but that this judgment does not follow from a cost-benefit analysis. It further concludes that technologies of air capture are deserving of a much greater role in mitigation policies than they have had in the past.

COPENHAGEN CONSENSUS ON CLIMATE

The Copenhagen Consensus Center has commissioned 21 papers to examine the costs and benefits of different solutions to global warming. The project's goal is 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 most good for the world, which solutions would yield the greatest net benefits?"

The series of papers is divided into Assessment Papers and Perspective Papers. Each Assessment Paper outlines the costs and benefits of one way to respond to global warming. Each Perspective Paper reviews the assumptions and analyses made within an Assessment Paper.

It is hoped that, as a body of work, this research will provide a foundation for an informed debate about the best way to respond to this threat.

CONTENTS

INTRODUCTION

Bickel and Lane (2009, hereafter BL09) focus on "climate engineering"[1] in the context of the central organizing question of the Copenhagen Consensus exercise for climate change:

> 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 most good for the world, which solutions would yield the greatest net benefits? – i.e. what are the costs and benefits of different viable climate interventions…given some reasonable assumptions about sensible policies for the rest of 21st century?

More precisely, BL09 focus primarily on two technologies of climate engineering: stratospheric aerosol injection and marine cloud whitening (which together they call SRM, solar radiation management),[2] both of which serve to alter the radiation balance of the global earth system via changes in albedo. BL09 apply a cost-benefit analysis methodology to evaluate the potential value of implementation of these technologies under the assumption that "A finding that net benefits may be large, suggests that we should devote some current resources to researching and developing this capacity."

This response paper to BL09 proceeds in two parts. Part I offers a critique of the cost-benefit analysis methodology of BL09, arguing that the analysis is, at best, arbitrary, and more critically, not grounded in a realistic set of assumptions about how the global earth system actually works. I argue that the present understandings of the potential effects of climate engineering are not sufficiently well developed to allow for any meaningful cost-benefit analyses. Nonetheless, I agree with BL09 when they conclude that there is value in further research on climate engineering technologies. My judgment, as apparently was the case as well for the conclusions of BL09, is not based on numbers that result from precise-looking cost-benefit analyses, but rather, on the fact that our understandings are so poor. I further argue that developing informed understandings will require adopting a more scientifically realistic perspective on the role of climate engineering in the global earth system than is reflected in the simplifications presented in BL09. I conclude that the quantitative cost-benefit analysis of BL09 is guilty of precision without accuracy.

Part II of the paper summarizes an analysis of the potential role for air capture technologies to play in the decarbonization of the global economy. BL09 consider air capture only briefly, leaving a more detailed analysis to this response paper. I show that the costs of air capture are comparable to the costs of conventional mitigation, as presented by the Intergovernmental Panel on Climate Change (IPCC) in its 2007 assessment report, as well as the widely cited Stern Review Report by the government of the United Kingdom. Based on this conclusion I argue that air capture deserves to receive a similar close scrutiny as other mitigation policies.

The paper concludes by considering more general criteria for evaluating technological fixes such as technologies of climate engineering. I suggest that stratospheric aerosol injection and marine cloud whitening comprehensively fail these broader criteria whereas air capture does not.

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[1] BL09 define "climate engineering" as "the intentional modification of Earth's environment to promote habitability," and is largely synonymous with term "geoengineering."

[2] BL09 also include a brief discussion of the direct "air capture" of carbon dioxide from the atmosphere.

PART I: A CRITIQUE OF THE COST-BENEFIT METHODOLOGY OF BL09

BL09 are to be applauded for sticking their necks out on a very complex and difficult subject. Such intellectual leadership is often followed by critical commentary, and this case is no different. A first thing to note of BL09 is that their policy recommendations do not follow from the cost benefit analysis. Their quantitative analysis results in the following dramatic conclusions:

> The direct B/C ratio for stratospheric aerosol injection is on the order of 25 to 1, while the B/C ratio for marine cloud whitening is around 5000 to 1.

One would think that with such overwhelmingly positive benefit-cost ratios the authors would immediately recommend a strategy of climate engineering as a core policy response to climate change. Instead, the authors recommend only investing in further research: "an initial investment of perhaps 0.3% ($750 million) of the global total proposed by the Copenhagen Consensus guidelines might be an appropriate average yearly expenditure for the first decade."

The authors' reluctance to recommend anything more than an initial investment in R&D reflects an appropriate degree of skepticism in their analysis, which they clearly state is preliminary and tentative. The authors are quite explicit about the limitations to their analysis:

> Any assessment of SRM and AC will be limited by the current state of knowledge, the rudimentary nature of the concepts, and the lack of prior R&D efforts. As noted in §1.1, this analysis relies on numbers found in the existing literature and existing climate change models. These inputs to our analysis are admittedly speculative; many questions surround their validity, and many gaps exist in them. This paper has also stressed the potential importance of transaction costs and "political market failures". Finally, many important scientific and engineering uncertainties remain. Some of these pertain to climate change itself, its pace, and its consequences. Still others are more directly relevant to SRM. How will SRM impact regional precipitation patterns and ozone levels? To what extent can SRM be scaled to the levels considered here? What is the best method for aerosol injection? Are there other side effects that could invalidate the use of SRM?

The concerns expressed by the authors do raise a question of whether cost-benefit analysis is an appropriate tool to use on a subject as complex and uncertain as climate engineering. More specifically, is it possible that the presentation of very precise looking cost-benefit ratios may do more to mislead than provide insight on the practical merits of climate engineering?

Below I argue that the technologies of solar radiation management and marine cloud whitening are not sufficiently developed to allow for any sort of meaningful cost-benefit analysis. I go further and argue that the framework used in BL09 represents a misleading simplification of how the earth system actually works, and would be unable in any case to lead to a practically meaningful assessment of the costs or benefits of even well-developed technologies of climate engineering. Nonetheless, I fully agree with the conclusions of BL09 that climate engineering should be the subject of continued research, perhaps proving the point that agreement on potential costs and benefits is irrelevant to deciding to lend support for additional research on the subject.

Major Issue #1: The Inability to Accurately Anticipate Costs or Benefits

It is a simple logical observation to state that to be able to conduct a meaningful cost-benefit analysis requires some degree of accuracy in estimates of both costs and benefits of alternative courses of action. In the cases of stratospheric aerosol injection and marine cloud whitening there are considerable uncertainties in direct costs of deployment, not least because "no fully worked out concept for implementing SRM." As the authors note with respect to indirect costs (i.e., impacts), there are areas of both uncertainty and fundamental ignorance where even uncertainties are not well understood.[3]

But let us assume that direct costs of the technologies (i.e., implementation) are known with some degree of accuracy, such that they pose no obstacle to conducting a meaningful cost-benefit analysis. It is in the areas of fundamental ignorance in estimates of indirect costs and potential benefits that are fatal to efforts to create a meaningful cost-benefit analysis. When a quantitative analysis of any type is operating in areas of ignorance simplifying assumptions must be made in such a way so as to allow the calculations to occur. Such assumptions can be made in any of a number of potentially plausible ways leading to diametrically opposed conclusions. And when the outcome of an analysis rests entirely on the choice of assumptions that cannot be discriminated from one another empirically, they exercise can do more to obscure than reveal.

Consider Goes et al. 2009, which just as in BL09, uses a modified version of the DICE integrated assessment model as the basis for calculating the potential indirect costs and benefits of SRM. Goes et al. 2009 conclude the following:

> … aerosol geoengineering hinges on counterbalancing the forcing effects of greenhouse gas emissions (which decay over centuries) with the forcing effects of aerosol emissions (which decay within years). Aerosol geoengineering can hence lead to abrupt climate change if the aerosol forcing is not sustained. The possibility of an intermittent aerosol geoengineering forcing as well as negative impacts of the aerosol forcing itself may cause economic damages that far exceed the benefits. Aerosol geoengineering may hence pose more than just "minimal climate risks," contrary to the claim of Wigley (2006). Second, substituting aerosol geoengineering for CO2 abatement fails an economic cost-benefit test in our model for arguably reasonable assumptions.

Thus, using the same (or a very similar) integrated assessment model and simply varying assumptions about "deep uncertainties" leads to results that are completely contradictory with those presented in BL09. This outcome is not because BL09 is wrong and Goes et al. 2009 is right, or vice versa. This outcome results because there is presently no way to discern which set of assumptions is more appropriate to use in the analysis, hence the presence of "deep uncertainty" which I have here called "ignorance."

The conclusion that should be reached from the comparison of the two studies is that while it is certainly possible that techniques of SRM can lead to very large benefits in relation to costs, it is also possible that SRM could lead to very large costs with respect to benefits.

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[3] I do not here address the issue of political transaction costs, which are raised in BL09. I do agree with BL09 that such costs are "speculative" at this point, adding another layer of ignorance to the issue. They write, "No one can yet know how the process will distort the various options."

There is simply no way at this point to empirically adjudicate between these starkly different conclusions. It is this fundamental ignorance that leads to the conclusion that "more research is needed."

Underscoring the very large uncertainties present on climate engineering, Goes et al. (2009) cite a 1992 NRC report, finding its conclusions to still be current:

> More than a decade ago, a Unites States National Academies of Science committee assessing geoengineering strategies concluded that "Engineering countermeasures need to be evaluated but should not be implemented without broad understanding of the direct effects and the potential side effects, the ethical issues, and the risks" (COSEPUP[NRC], 1992). Today, we are still lacking this broad understanding.

The conclusions presented by BL09 finding benefit cost ratios of 25 to 1 and 5,000 to 1 should thus be taken with a very large dose of salt, as they reflect choices made in the analysis that, had they been made differently but also plausibly, could have resulted in very different (even opposite) conclusions. Hence, in this case the cost-benefit analysis leads to precision without accuracy, and risks doing more to obscure uncertainties than to clarify them.

As a consequence, there are no policy recommendations in BV09 that result directly from the cost-benefit analysis. The recommendation to fund research is a matter of qualitative judgment, and the size of investment into SRM proposed by BL09 of $750 million over 10 years is arbitrary. I agree with BL09 that some investment in research on climate engineering makes sense, however, I disagree that a cost-benefit analysis tells us anything meaningful about how much should be invested in research or what the potential payoffs might be. Because climate engineering research has considerable value to advancing fundamental understandings of the global earth system, there are other justifications for its support beyond the potential development of climate engineering technologies.

Major Issue #2: Reliance on a Demonstrably Incorrect Conceptual Model of How Climate Engineering Influences the Global Earth System

Beyond the ability to accurately assess the costs and benefits of climate engineering, there is a more fundamental issue with the approach taken by BL09, and that is the reliance on a conceptual model of the global earth system that is scientifically flawed. The broader complexities are discussed by Goes et al. 2009:

> The analysis, so far, assumes that geoengineering causes environmental damages only through the effects on global mean temperatures (i.e., the value of $\theta$was set to zero). As discussed above, the aerosol geoenginering forcing is projected to change Earth system properties such as precipitation- and surface temperature-patters, El Niño, and polar ozone concentrations, to name [sic] (Robock, 2008; Lunt et al., 2008). A review of the current literature on the impacts of stratospheric aerosol on natural and human systems suggests that aerosol injections into the atmosphere might cause potentially sizable damages (Lunt et al., 2008; Robock, 2008; Robock et al., 2008; Trenberth and Dai, 2007).[4]

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[4] I note that none of the citations in this passage from Goes et al. 2009 is also cited in BL09.

Specifically, BL09 approach the evaluation of costs and benefits of SRM through the framework of "radiative forcing."[5] The IPCC (2007a) notes that the concept is very useful but that "it provides a limited measure of climate change as it does not attempt to represent the overall climate response." The IPCC (2007b) also cautions against simply summing various radiative forcing terms.[6] NRC (2005, 158) offered an even more explicit warning:

> For most policy applications, the relationship between radiative forcing and temperature is assumed to be linear, suggesting that radiative forcing from individual positive and negative forcing agents could be summed to determine a net forcing. This assumption is generally reasonable for homogenously distributed greenhouse gases, but it does not hold for all forcings. Thus, the assumed linearity of radiative forcing has been simultaneously useful and misleading for the policy community. It is important to determine the degree to which global mean TOA [top of the atmosphere] forcings are additive and whether one can expect, for example, canceling effects on climate change from changes in greenhouse gases on the one hand and changes in reflective aerosols on the other.

BL09 modifies the DICE model by using a simple additive term to represent the climatic effect of SRM, which may or may not be scientifically justifiable. Not only are their uncertainties and ignorance about the costs and benefits of climate engineering, but there are fundamental areas of uncertainty and ignorance in how to even conceptualize those effects.

NRC(2005) presented a more complex view of radiative forcing than found in either the IPCC (or BL09) and its relationship to non-radiative forcings, indirect radiative forcings and their feedbacks, as shown in the figure below. The relationship of a forcing agent, such as the injection of stratospheric aerosols or marine cloud whitening, and eventual climate impacts at global as well as regional scales manifests itself in a degree of inter-relationships and feedbacks that cannot resolved simply by adding or subtracting direct radiative forcings. Perhaps future research will show that all other relationships beyond the additive effect on direct radiative forcing can be ignored, however, current research suggests that this is not the case (see the wide range of sources cited in NRC, 2005).

To summarize Part I, the ability to conduct a cost-benefit analysis of climate engineering is hindered by both uncertainties and fundamental ignorance of both costs and benefits. It is quite possible to vary assumptions in plausible ways and to arrive at diametrically opposed results. Further, the analysis in BL09 simplifies physical relationships in a manner suitable for inclusion in a simple integrated assessment model, but in the process fail to reflect that the global earth system may actually respond to forcing agents and changes in climate system components through direct and indirect radiative forcing, non radiative forcings and feedbacks among these. Consequently, I conclude that a quantitative cost-benefit analysis of the climate engineering technologies of SRM is premature at best.

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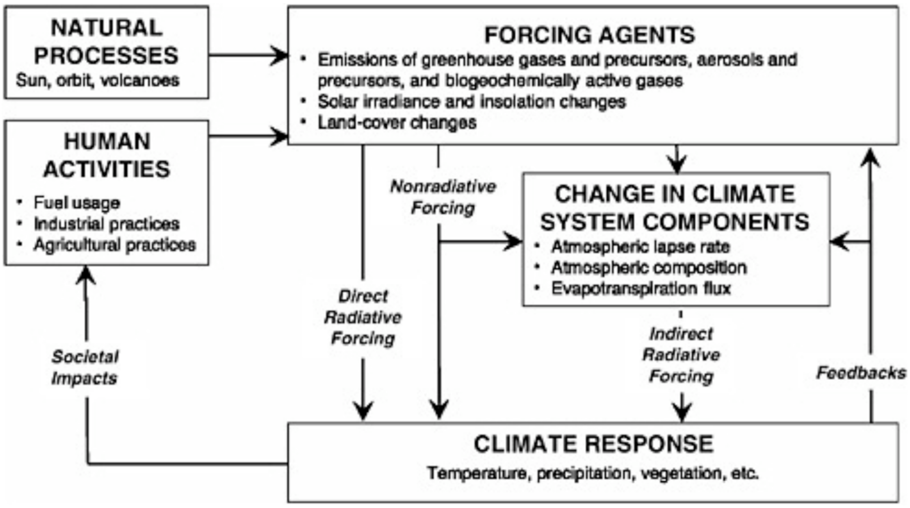
[5] The IPCC defines "radiative forcing" as "'the change in net (down minus up) irradiance (solar plus long-wave; in W m^{-2}) at the tropopause after allowing for stratospheric temperatures to readjust to radiative equilibrium, but with surface and tropospheric temperatures and state held fixed at the unperturbed values." See IPCC (2007a, p. 133).

[6] See the caption to figure SPM.2 in the 2007 Summary for Policymakers of Working Group I where it states, "The net anthropogenic radiative forcing and its range are also shown [in the figure]. These require summing asymmetric uncertainty estimates from the component terms, and cannot be obtained by simple addition."

Figure 1. Radiative forcing in context, NRC, 2005.



See, http://www.nap.edu/openbook.php?record_id=11175&page=13

PART II: THE COSTS OF AIR CAPTURE

As part of my response to BL09 I was asked to provide an overview of the costs and benefits of 'air capture" technologies. Air capture refers to a range of methods and technologies for the direct removal of carbon dioxide from the ambient air, ranging from photosynthesis to chemical extraction, and has received increasing attention in recent years.[7] After removal, in order to draw down atmospheric concentrations of carbon dioxide the gas needs to be either sequestered or otherwise used.

Air capture is particularly amenable to a cost-benefit analysis because it directly addresses a part of the climate change issue that has been most intensively studied, the increasing accumulation of carbon dioxide in the atmosphere. There have been various studies of the economic benefits of limiting the accumulation of greenhouse gases, which will not be recited here. Thus, in order to compare air capture as a possible contributor to stabilizing concentrations of greenhouse gases, it need only be compared in terms of costs to other approaches to stabilizing concentrations.[8] The fundamental question to be asked is how does the cost of air capture compare to other approaches to stabilizing concentrations of carbon dioxide in the atmosphere?

Estimates vary for the cost of capturing carbon dioxide directly from the atmosphere. Keith et al. (2006) suggest that using existing technology the costs could be as much as $500 per ton of carbon, and perhaps eventually under $200/ton. In 2007 Keith suggested that the cost of air capture could drop below $360 per ton (Graham-Rowe, 2007). Columbia University's

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[7] This section of the paper draws on Pielke (2009) which provides a more comprehensive review of air capture and its economics. The focus in Pielke (2009) is on techniques of chemical extraction, however the economic analysis is a function of cost rather than specific technology and thus could be equally applied to biological or geological means of air capture.

[8] Of course, all studies of the benefits of mitigation policies could be wrong, however that will affect judgments of mitigation policies in general, and not an analysis of air capture specifically.

Klaus Lackner has suggested that the costs today are less than $360 per ton of carbon, and may eventually fall beneath approximately $100 per ton. IPCC (2007a) discusses air capture only in passing:

> Studies claim costs less than 75 US$/t$CO_2$ [$275/tC] and energy requirements of a minimum of 30% using a recovery cycle with $Ca(OH)_2$ as a sorbent. However, no experimental data on the complete process are yet available to demonstrate the concept, its energy use and engineering costs.[9]

In the simple exercises below I use three values for the costs of air capture: (a) $500 per ton of carbon, (b) $360 per ton, and (c) $100 per ton, as described in Pielke (2009). The IPCC (2007a) estimate falls near the middle of this range.

The Costs of Stabilization via Air Capture

At 2.13 GtC equivalent to1 ppm carbon, this means that the current (idealized) costs of air capture are about $1 trillion per reduced ppm of atmospheric carbon dioxide at a cost of air capture equal to $500/tC. $1 trillion represented about 2.5% of global GDP in 2007. At $500/tC complete mitigation of net 2008 human emissions would cost about $4 trillion, or about 10% of global GDP. At $100/ton the 2007 cost would be about 2.0% of global GDP.

If the goal of air capture is to limit cumulative carbon dioxide emissions during the remainder of the 21st century to less than 240 GtC (as suggested by the IPCC as being consistent with a 450 ppm target), then there are many different temporal paths over which air capture might be implemented. That is, it is the cumulative emissions over the 21st century that matter, not the specific emissions trajectory. The further into the future one assumes deployment the lower the present value will be as a function of the discount rate chosen. The analysis below does not discount.

The analysis errs on the side of understating costs as there are no assumptions made about the economies of scale associated with a widespread deployment and likely reductions in costs of the technology (McKinsey, 2008). The calculation of cost involves simply multiplying the expected capture cost per ton of carbon by the integral of the difference between projected emissions and emissions under air capture. The analysis here assumes that cumulative, business-as-usual (i.e., no air capture), net carbon dioxide emissions will be approximately 880 gigatonnes of carbon from 2008 to 2100, which is somewhat higher than the mid-range projection of the IPCC (see Pielke, 2009 for details). Higher or lower values, which are certainly plausible, will result in corresponding changes in the cost estimates of air capture.

Under these assumptions, Tables 1a and b shows the cumulative costs of air capture over the periods 2008-2050 and 2008-2100 for different stabilization levels and different costs per ton of carbon. Table 1a assumes an annual global GDP growth rate of 2.9% following IPCC (2000), and Table 1b assumes after Stern (2007) an annual global GDP growth rate of 2.5%. Stern (2007) uses a global GDP of $35 trillion in 2005. No effort has been made here to

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[9] Working Group III, Chapter 4, p. 286. The IPCC provides no reference or justification for its cost estimate. The IPCC's dismissal of air capture in this manner is surprising, because much of the IPCC's analysis of the prospects for and costs of greenhouse gas mitigation depend upon policies and technologies whose implementation has not been proven successful in practice.

account for the time value of money or different approaches to calculating economic growth across countries, which have been discussed elsewhere in great depth in the context of climate change, and all dollars are expressed in constant-year terms.

Table 1a. Cost of air capture as a percentage of global GDP, assuming 2.9% global GDP growth to 2100 after IPCC (2000).

	\$500/GtC	\$360/GtC	\$100/GtC
450 ppm Cost to 2050	2.7%	1.9%	0.5%
550 ppm Cost to 2050	0.0%	0.0%	0.0%
450 ppm Cost to 2100	2.1%	1.5%	0.4%
550 ppm Cost to 2100	1.5%	1.1%	0.3%

Table 1b. Cost of air capture as a percentage of global GDP, assuming 2.5% global GDP growth to 2100 after Stern (2007).

	\$500/GtC	\$360/GtC	\$100/GtC
450 ppm Cost to 2050	3.0%	2.2%	0.6%
550 ppm Cost to 2050	0.0%	0.0%	0.0%
450 ppm Cost to 2100	2.7%	2.0%	0.5%
550 ppm Cost to 2100	2.0%	1.4%	0.4%

All of the values presented in Tables 1a and b for the costs of stabilization at 450 ppm via air capture fall within the range of those presented in the Stern Review (2007) which suggested that stabilization at 450 ppm carbon dioxide would cost about 1% of global GDP to 2100 (with a range of plus/minus 3%).[10] Stern (2007) explained how one might think about this value:

> . . . if mitigation costs 1% of world GDP by 2100, relative to the hypothetical 'no climate change' baseline, this is equivalent to the growth rate of annual GDP over the period dropping from 2.5% to 2.49%. GDP in 2100 would still be approximately

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[10] Stern (2007) equated a 450 ppm carbon dioxide level with a 550 ppm carbon dioxide equivalent concentration, which includes other gases.

940% higher than today, as opposed to 950% higher if there were no climate-change to tackle.

If air capture technology could be implemented at $100/ton, then the cost to stabilize emissions over the 21st century would be less than the Stern median estimate. For stabilization at 550 ppm or about twice pre-industrial, air capture costs nothing prior to 2050.

Similarly, the ranges of costs for air capture are comparable to those presented in IPCC (2007a) which estimated the costs of mitigation for 2050 at a level of 535-590 ppm carbon dioxide equivalent (comparable to Stern's 450 ppm carbon dioxide) to fall within the IPCC range of -1% to 5.5% of global GDP in 2050. The IPCC median value of 1.3% is less than the cost of air capture at $360 cost per ton of carbon, but almost three times the cost at $100 per ton.

Making global cost estimates for any complex set of interrelated systems far into the future is a dubious enterprise. However, the analysis here shows that using very similar assumptions to the IPCC (2007c and d) and Stern (2007), air capture compares favourably with the cost estimates for mitigation provided in those reports. The main reason for this perhaps surprising result, given that air capture has a relatively high cost per ton of carbon, is the long period for which no costs are incurred until the stabilization target is reached. Further, a factor not considered here is that the economy would likely grow at a higher rate than with early, aggressive mitigation, meaning that the costs of air capture would be a smaller fraction of future GDP than comparable costs per ton of C requiring large costs early in the century. The cost of air capture under the assumptions examined here is also less that the projected costs of unmitigated climate change over the 21st century, which Stern (2007) estimated to be from 5-20% of GDP annually and IPCC (2007e) estimate to be 5% of global GDP by 2050.

There are several additional factors, beyond those already discussed, which serve to overstate the cost estimates of air capture found in Tables 1a and b. Carbon dioxide emissions from power plants, representing perhaps as much as half total emissions over the 21st century could be captured at the source for what many believe is a cost considerably less than direct air capture.[11] The technical, environmental, and societal aspects of carbon sequestration are identical for capture of carbon dioxide from both power plants and ambient air. To the extent that improvements in efficiency and overall emissions intensity occur, these developments would further reduce total emissions and thus the need to rely on air capture.[12] The assumptions here assume simplistically a fixed average cost of air capture over time, whereas experience with technological innovation suggests declining marginal costs over time (e.g., McKinsey, 2008).

Consideration of these factors could reduce the values presented in Tables 1a and b by a significant amount perhaps by as much as half. Uncertainties in rates of increasing emissions, economic growth, and concentrations mean that the values presented here could be more or less than under different assumptions. Because the analysis relies on the mid-range values of the IPCC for these various factors, it is unlikely that a more comprehensive treatment of

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[11] For a review of the costs of carbon capture and storage (CCS), see IPCC (2005).

[12] In addition, if the allowable "carbon allocation" is understated (overstated) by the simple methodology here, then there would be less (more) need for air capture and corresponding less (more) costs.

uncertainties would lead to qualitatively different conclusions if one begins with assumptions underpinning and implications following from the IPCC.

To summarize, a simple approach to costing air capture as a strategy of achieving carbon dioxide stabilization targets using 2007 technology results about the same costs as the costs estimates for stabilization at 450 ppm or 550 ppm carbon dioxide presented by IPCC (2007c) and Stern (2007). If the costs of air capture decrease to $100 per ton of carbon, then over the 21st century air capture would in fact cost much less than the costs estimates for stabilization presented by IPCC (2007c) and Stern (2007). This surprising result suggests, at a minimum, that air capture should receive the same detailed analysis as other approaches to mitigation. To date it has not.

CONCLUSION: CLIMATE ENGINEERING AS A TECHNOLOGICAL FIX

BL09 raise important questions about how to evaluate the role of a technological fix in efforts to stabilize concentrations of greenhouse gases (primarily carbon dioxide) in the atmosphere. In this response I have argued that cost-benefit analyses of SRM are limited in the insights they can bring to bear on highly complex systems that are incompletely understood. Writing in Nature, Sarewitz and Nelson (2008) offer three broader criteria by which to distinguish "problems amenable to technological fixes from those that are not." Here in conclusion I briefly apply these criteria to the technology of climate engineering, concluding that indirect approaches to climate engineering such as SRM fall well short of all three of the criteria that Sarewitz/Nelson present as guidelines for when to employ a technological fix. By contrast the technology of air capture offers much greater promise.

Sarewitz/Nelson Criterion #1: The technology must largely embody the cause-effect relationship connecting problem to solution.

As argued in Part I of this paper, SRM does not directly address the cause-effect relationship between emissions and increasing atmospheric concentrations of carbon dioxide (and other greenhouse gases). It addresses the effects, and only in indirect, poorly understood fashion. It is thus appropriate to consider SRM as a form of adaptation to human-caused climate change. In this instance, rather than building a levee (i.e., changing localized topography) to physically ward off rising seas, the goal of SRM is to alter the earth system in other ways to compensate for the effects of changes in climate. Unlike levees, where cause and effect are unambiguous, SRM has unknown consequences. In contrast, air capture prevents a human perturbation through the release of carbon dioxide into the atmosphere, and thus directly addresses the accumulation of carbon dioxide in the atmosphere. Thus, air capture is a form of mitigation.

Sarewitz/Nelson Criterion #2: The effects of the technological fix must be assessable using relatively unambiguous or uncontroversial criteria.

As argued in Part I of this paper, the effects of climate engineering on climate impacts of concern - including phenomena such as extreme events, global precipitation patterns, sea ice extent, biodiversity loss, food supply, and so on - would be difficult if not impossible to assess on timescales of relevance to decision makers. Research on weather modification provides a cautionary set of lessons in this regard (cf. Travis, 2009). In contrast, the technology of air capture does not require developing a better understanding of the global earth system – simply knowing that the accumulation of carbon dioxide poses risks worth responding to is a sufficient basis for considering deployment. In other words, if the accumulation of carbon dioxide in the atmosphere is judged to be a problem, then its removal logically follows as a solution.

Sarewitz/Nelson Criterion #3: Research and development is most likely to contribute decisively to solving a social problem when it focuses on improving a standardized technical core that already exists.

Climate engineering via SRM on a planetary scale has never been attempted, and to do so would in effect be a decision to implement the technology, as we have only one earth. Thus, its effects cannot be known, only speculated upon and researched with sophisticated scientific tools. Even so, it could easily have unpredicted or undesirable effects. By contrast air capture builds upon existing (and expensive) technologies that can be deployed, evaluated, refined and improved upon with no risk to the climate system.

In short, SRM fails comprehensively with respect to the three criteria for technological fixes offers by Sarewitz and Nelson, suggesting that it offers little prospect to serve as a successful contribution to efforts to deal with increasing concentrations of carbon dioxide. As they write, “one of the key elements of a successful technological fix is that it helps to solve the problem while allowing people to maintain the diversity of values and interests that impede other paths to effective action.” Because it fails with respect to the three criteria, SRM is likely to make the politics of climate change even more complex and contested, resulting in little prospect for success. But even if SRM offers few prospects for successfully addressing the climate issue, as concluded in BL09, continued research on SRM nonetheless make sense both to keep options open and also to contribute to a further understanding of the human role in the climate system. In contrast, for reasons of a preliminary cost-benefit analysis as well as with respect to broader criteria of a technological fix, technologies of air capture are deserving of a much greater role in mitigation policies than they have had in the past.

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The science is clear. Human-caused global warming is a problem that we must confront.

But which response to global warming will be best for the planet? The Copenhagen Consensus Center believes that it is vital to hold a global discussion on this topic.

The world turned to scientists to tell us about the problem of global warming. Now, we need to ensure that we have a solid scientific foundation when we choose global warming's solution. That is why the Copenhagen Consensus Center has commissioned research papers from specialist climate economists, outlining the costs and benefits of each way to respond to global warming.

It is the Copenhagen Consensus Center's view that the best solution to global warming will be the one that achieves the most 'good' for the lowest cost. To identify this solution and to further advance debate, the Copenhagen Consensus Center has assembled an Expert Panel of five world-class economists – including three recipients of the Nobel Prize –to deliberate on which solution to climate change would be most effective.

It is the Copenhagen Consensus Center's hope that this research will help provide a foundation for an informed debate about the best way to respond to this threat.

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