1.1 Climate Engineering

Alternative Perspective

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Introduction

This Perspective paper critiques the cost-benefit analysis (CBA) of climate engineering (CE) in chapter 1 by J. Eric Bickel and Lee Lane (Bickel and Lane 2009, hereafter, BL09) in two parts. First, it argues that the analysis of solar radiation management (SRM) 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 (AC) technologies to play in the de-carbonization of the global economy, finding the costs of AC to be directly comparable with major global assessments of the costs of conventional mitigation policies. The Perspective paper concludes, as does BL09, that there is justification for continued research into technologies of SRM, but that this judgment does not follow from a CBA. It further concludes that technologies of AC are deserving of a much greater role in mitigation policies than they have had in the past.

BL09 focuses on "climate engineering"[1] in the context of the Copenhagen Consensus exercise for climate change, where authors were tasked by the Copenhagen Consensus Center with addressing 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?

More precisely, BL09 focus primarily on two technologies of CE: stratospheric aerosol injection and marine cloud whitening (which together they call SRM),[2] both of which serve to alter the radiation balance of the global earth system via changes in albedo. BL09 apply a CBA 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 Perspective paper proceeds in two parts. The first part offers a critique of BL09's CBA methodology, 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 understandiugs of the potential effects of CE are not sufficiently well developed to allow for any meaningful CBAs. Nonetheless, I agree with BL09 when they conclude that there is value in further research on CE 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 CBAs, 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 CE in the global earth system than is reflected in the simplifications presented in BL09. I conclude that the quantitative CBA of BL09 is guilty of precision without accuracy.

The second part of the Perspective paper summarizes an analysis of the potential role for AC technologies to play in the de-carbonization of the global economy. BL09 consider AC only briefly, leaving a more detailed analysis to this paper. I

[1] BL09 define "climate engineering" (CE) as "the intentional modification of Earth's environment to promote habitability," and is largely synonymous with term "geoengineering" (GE).
[2] BL09 also include a brief discussion of the direct "air capture" (AC) of CO_2 from the atmosphere.

show that the costs of AC 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 *The Stern Review* report by the government of the UK (Stern 2007). Based on this conclusion I argue that AC deserves to receive a similar close scrutiny as other mitigation policies.

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

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 BCR for stratospheric aerosol injection is on the order of 25 to 1, while the BCR for marine cloud whitening is around 5,000 to 1.

One would think that with such overwhelmingly positive BCRs the authors would immediately recommend a strategy of CE 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 . . . , 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 chapter 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 CBA is an appropriate tool to use on a subject as complex and uncertain as CE. More specifically, is it possible that the presentation of very precise-looking BCRs may do more to mislead than provide insight on the practical merits of CE?

Below I argue that the technologies of SRM and marine cloud whitening are not sufficiently developed to allow for any sort of meaningful CBA. 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 CE. Nonetheless, I fully agree with the conclusions of BL09 that CE 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 CBA 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 there is "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 CBA. 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 CBA. 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, the exercise can do more to obscure than reveal.

Consider Goes *et al.* (2009) which, as in BL09, uses a modified version of the DICE integrated assessment model (IAM) as the basis for calculating the potential indirect costs and benefits of SRM. Goes *et al.* (2009: 14) 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 CO_2 abatement fails an economic cost-benefit test in our model for arguably reasonable assumptions.

Thus, using the same (or a very similar) IAM 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. 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 CE, Goes *et al.* (2009: 14) cite a 1992 NRC report, finding its conclusions to still be current:

> More than a decade ago, a United 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 BCRs 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 CBA leads to precision without accuracy, and risks doing more to obscure uncertainties than to clarify them.

[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."

As a consequence, there are no policy recommendations in BL09 that result directly from the CBA. 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 ten years is arbitrary. I agree with BL09 that some investment in research on CE makes sense, however, I disagree that a CBA tells us anything meaningful about how much should be invested in research or what the potential payoffs might be. Because CE 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 CE 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 CE, 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: 11):

> The analysis, so far, assumes that geoengineering causes environmental damages only through the effects on global mean temperatures (i.e., the value of θ was set to zero). As discussed above, the aerosol geoenginering forcing is projected to change Earth system properties such as precipitation – and surface temperature – patterns, El Niño, and polar ozone concentrations, to name [several] (Lunt *et al.* 2008; Robock 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]

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 there uncertainties and ignorance about the costs and benefits of CE, 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,

[4] I note that many of the citations in this passage from Goes *et al.* (2009) are also cited in BL09.

[5] The IPCC defines "radiative forcing" as "'the change in net (down minus up) irradiance (solar plus longwave, 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: 133).

[6] See the caption to figure SPM.2 in the 2007 *Summary for Policy Makers 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."

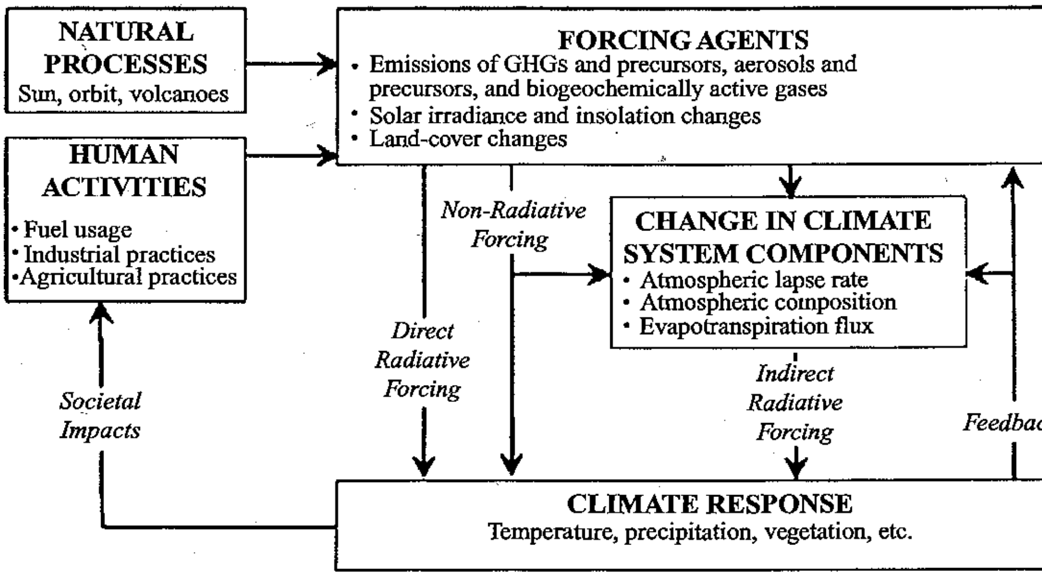

Figure 1.1.1 *Radiative forcing in context (NRC 2005), see www.nap.edu/openbook.php?record_id=11175&page=13*

as shown in figure 1.1.1. 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 interrelationships and feedbacks that cannot be 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, the ability to conduct a CBA of CE 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 fails 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 CBA of the CE technologies of SRM is premature at best.

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" (AC) refers to a range of methods and technologies for the direct removal of CO_2 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 CO_2 the gas needs to be either sequestered or otherwise used.

AC is particularly amenable to a CBA because it directly addresses a part of the climate change issue that has been most intensively studied, the increasing accumulation of CO_2 in the atmosphere. There have been various studies of the economic benefits of limiting the accumulation of greenhouse gases (GHGs), which will not be recited here. Thus, in order to compare AC as a possible contributor to

[7] This section of the Perspective paper draws on Pielke (2009), which provides a more comprehensive review of AC 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 AC.

stabilizing concentrations of GHGs, 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 AC compare to other approaches to stabilizing concentrations of CO_2 in the atmosphere?

Estimates vary for the cost of capturing CO_2 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 AC could drop below $360 per ton (Graham-Rowe 2007). Columbia University's 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 AC 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 AC: (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 AC are about $1 trillion per reduced ppm of atmospheric CO_2 at a cost of AC 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 AC is to limit cumulative CO_2 emissions during the remainder of the twenty-first 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 AC might be implemented. That is, it is the cumulative emissions over the twenty-first century that matter, not the specific emissions trajectory. The further into the future one assumes deployment the lower the present value (PV) will be as a function of the discount rate chosen. The analysis below does not discount.

Table 1.1.1a Cost of AC as a percentage of global GDP, assuming 2.9% global GDP growth to 2100 (after IPCC 2000)

	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]

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 & Co. 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 AC. The analysis here assumes that cumulative, business-as-usual (BAU) (i.e. no AC), net CO_2 emissions will be approximately 880 GtC 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 AC.

Under these assumptions, tables 1.1.1a and 1.1.1b show the cumulative costs of AC over the periods 2008–50 and 2008–2100 for different

[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 AC specifically.

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

Table 1.1.1b Cost of AC as a percentage of global GDP, assuming 2.5% global GDP growth to 2100 after Stern (2007)

	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]

stabilization levels and different costs per ton of carbon. Table 1.1.1a assumes an annual global GDP growth rate of 2.9% following IPCC (2000), and table 1.1.1b 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 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.

All of the values presented in tables 1.1.1a and 1.1.1b for the costs of stabilization at 450 ppm via AC fall within the range of those presented in Stern (2007), which suggested that stabilization at 450 ppm CO_2 would cost about 1% of global GDP to 2100 (with a range of plus/minus 3%).[10] Stern (2007: 249) 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 940% higher than today, as opposed to 950% higher if there were no climate-change to tackle.

If AC technology could be implemented at $100/ton, then the cost to stabilize emissions over the twenty-first century would be less than the Stern median estimate. For stabilization at 550 ppm or about twice preindustrial, AC costs nothing prior to 2050.

Similarly, the ranges of costs for AC are comparable to those presented in IPCC (2007a) which estimated the costs of mitigation for 2050 at a level of 535–590 ppm CO_2 equivalent (comparable to Stern's 450 ppm CO_2) 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 AC 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, 2007d) and Stern (2007), AC compares favorably with the cost estimates for mitigation provided in those reports. The main reason for this perhaps surprising result, given that AC 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 AC would be a smaller fraction of future GDP than comparable costs per ton of carbon requiring large costs early in the century. The cost of AC under the assumptions examined here is also less that the projected costs of unmitigated climate change over the twenty-first 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 AC found in tables 1.1.1a and 1.1.1b. Carbon dioxide emissions from power plants, representing perhaps as much as half total emissions over the twenty-first century could be captured at the source for what many believe is a cost considerably less than direct AC.[11] The technical, environmental, and societal aspects of carbon sequestration are identical for capture of CO_2 from both power plants and ambient air. To the extent that improvements in efficiency and overall emissions intensity occur, these developments would further

[10] Stern (2007) equated a 450 ppm CO_2 level with a 550 ppm CO_2 equivalent concentration, which includes other gases.
[11] For a review of the costs of carbon capture and storage (CCS), see IPCC (2005).

reduce total emissions and thus the need to rely on AC.[12] The assumptions here assume simplistically a fixed average cost of AC over time, whereas experience with technological innovation suggests declining marginal costs over time (e.g. McKinsey & Co. 2008).

Consideration of these factors could reduce the values presented in tables 1.1.1a and 1.1.1b 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 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 AC as a strategy of achieving CO_2 stabilization targets using 2007 technology results about the same costs as the costs estimates for stabilization at 450 ppm or 550 ppm CO_2 presented by IPCC (2007c) and Stern (2007). If the costs of AC decrease to $100 per ton of carbon, then over the twenty-first century AC 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 AC 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 GHGs (primarily CO_2) in the atmosphere. In this response I have argued that CBAs 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 CE, concluding that indirect approaches to CE 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 AC offers much greater promise.

Sarewitz–Nelson Criterion 1: The Technology must largely Embody the Cause–Effect Relationship Connecting Problem to Solution

As argued in the first part of this Perspective paper, SRM does not directly address the cause–effect relationship between emissions and increasing atmospheric concentrations of CO_2 (and other GHGs). It addresses the effects, and only in an 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 levée (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 levées, where cause and effect are unambiguous, SRM has unknown consequences. In contrast, AC prevents a human perturbation through the release of CO_2 into the atmosphere, and thus directly addresses the accumulation of CO_2 in the atmosphere. Thus, AC 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 the first part of this Perspective paper, the effects of CE 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

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

modification provides a cautionary set of lessons in this regard (cf. Travis 2009). In contrast, the technology of AC does not require developing a better understanding of the global Earth system – simply knowing that the accumulation of CO_2 poses risks worth responding to is a sufficient basis for considering deployment. In other words, if the accumulation of CO_2 in the atmosphere is judged to be a problem, then its removal logically follows as a solution.

Sarewitz–Nelson Criterion 3: R&D is most likely to Contribute Decisively to Solving a Social Problem When it Focuses on Improving a Standardized Technical Core that already Exists

CE 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 AC 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 offered by Sarewitz and Nelson, suggesting that it offers little prospect to serve as a successful contribution to efforts to deal with increasing concentrations of CO_2. 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 CBA as well as with respect to broader criteria of a technological fix, technologies of AC are deserving of a much greater role in mitigation policies than they have had in the past.

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