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Global Warming

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Introduction

The Bali Conference concluded in mid-December with vague reference to a road map for a post-Kyoto climate policy. I, too, will begin with a "road map": to give the reader/listener some idea of my quite different "perspective" on climate policy. The following points are salient:

1. Greenhouse gas mitigation is first and foremost an energy technology problem.

2. Human-induced climate change is real, and, despite uncertainties, requires serious and priority attention.

3. The initiative that Yohe et al. (2007) find to have the highest benefit-cost B/C ratio (a combination of mitigation, adaptation, and R&D) includes the right set of components. However, the mix, rationale, and methodological approach suggested in this "Perspective" are rather different.

4. Most important of all, stabilizing atmospheric concentrations of carbon dioxide at levels such as 450 or 550 parts per million (ppm) is a much bigger challenge than one would conclude from stabilization analyses that use so-called "no climate policy" emissions scenarios as their reference points or "baselines". The implication: stabilizing concentrations does not seem possible (or at best would be prohibitively costly) without directly addressing the difficult-to-solve energy technology problem.

How do these points mesh with the findings of Yohe et al.? We are at one in maintaining that climate change needs to be taken seriously. We are also at one that there will be rising damages, evident even in Richard Tol's conservative Fund model with its adaptation-oriented approach, and in the work of Gary Yohe. The avoidable damages (benefits) exceed the paper's estimated mitigation costs, but this may reflect the existence of the Copenhagen Consensus (CC) budget constraint.

Because mitigation is budgetarily as well as benefit-cost limited, it is not clear by how much (or even whether) the Yohe et al., modeling results are also dependent on an assumed carbon-free "backstop" energy technology(ies). The MERGE model has sometimes employed such an assumption, in common with many (most?) economy- environment models. My approach is clearly at odds with a "backstop" assumption, whether carbon price-induced, or otherwise, because the backstop assumption essentially assumes away the problem by introducing unspecified carbon-neutral technologies with no assessment of their technological or economic feasibility. However, my views are consistent with a reported statement of one of the co-authors (Richels) who was quoted (by Andrew Revkin of the New York *Times*, May 4, 2007) as saying that "a carbon policy without an R&D policy is bankrupt".

I approach my task as follows. I take as established that climate scientists (IPCC WG I, and to some extent WG II), have made a compelling case that (i) humankind is altering the energy balance of the earth-atmosphere system; (ii) the impact of the altered balance on climate is likely to become substantial (the hope for a low and relatively benign "climate sensitivity" is rapidly fading), and (iii) the physical (glaciers, sea ice, etc) and biological (corals, biodiversity, etc) "tolerance" to substantial climate change may be lower than we might have hoped. (The jury is still out, it would seem, on the extent of the impact on humans, although predictable water stress, sea level rise and potential climate change-induced migrations suggest human impacts may be substantial too.)

But I diverge substantially from the claims of IPCC WG III which conclude that the technologies needed to stabilize emissions are currently available or under development. I do not think the evidence indicates that we currently have the technological means to stabilize greenhouse gas concentrations at a level sufficient to avoid an increasingly unpleasant climate change future. The issue of whether current technologies are adequate to climate stabilization has been a matter of vibrant debate that was almost entirely ignored by the IPCC (Hoffert et al., 1998, 2002; IPCC WG III, 2001; Caldeira et al., 2003; Pacala and Socolow, 2004; Edmonds et al., 2007; Wigley et al., 2007 [unpublished]).

The debate is important! In my view, belief that the requisite technology(ies) is/are available and adequate to the challenge (IPCC, 2001) has badly distorted climate policy for more than a decade. It has not only contributed to virtually single-minded focus on near-term mitigation, but mitigation achieved by mandating emission-reduction targets. The commitments made not only lack credibility (Schelling, 1992), but promote costly, and ultimately futile, attempts to achieve them. In what follows, I demonstrate why the technology challenge to GHG stabilization is huge (implying that there is a very large energy technology gap), and that most assessments seriously understate the magnitude of that challenge.

Assessing the Challenge of GHG Stabilization

In this Section, I assess the technology challenge to GHG stabilization with three rather different and impressionistic vignettes. One is the proposal that emanated from the G-8 meeting in May 2007. The second involves the move of energy intensive industry to the Far East, particularly China. The third involves baselines against which the stabilization challenge is measured.

The G-8 Proposal

In 2007, the host of the G-8 meeting was Germany's Chancellor, Andrea Merkel. Chancellor Merkel wanted the Group of Eight to sign onto an agreement to target a 50% reduction in **global** greenhouse-gas (GHG) emissions from *1990* levels. President Bush initially refused to go along, but under pressure appeared to agree that a 50% reduction from **current** (2007**)** levels could be seriously considered. (The final communiqué emphasized the apparent agreement on the 50%-by-2050 target, but fails to specify a reference year, nor define what is meant by net "GHG emissions" and so fails to address the relative importance of reductions in CO2 versus other GHGs.)

As is usual when discussions of targets come up, the emphasis is on what *ought* to be done, not on what *can* be done. The G-8 is apparently no exception. Had the appropriate arithmetic been done, it would have been clear that a reduction of **global** CO2 emissions from an estimated 8 GtC in 2007 (emissions were 6 GtC in 1990) to 4 GtC in 2050 is for all intents and purposes out of the question. Why? Because it is tantamount to requiring a transformation of energy systems and economies sufficiently great that, on average, the world as a whole would have, in 2050, a lower carbon intensity (CO2 emissions divided by GDP—i.e., C/GDP) than Switzerland had in 2005.

Let us put the implications of the G-8 proposal in perspective. Switzerland's economy, with its emphasis on high-valued, low energy-using industries such as watches, banking and finance, could not be more unrepresentative of the world's economy, especially the most rapidly growing and populous part. Not surprisingly, except for two or three exceedingly poor countries, including Chad and Cambodia, Switzerland has the lowest carbon intensity of output in the world.

Energy Intensity in Asia

The heightened concern over climate change is occurring at the same time as the *rate of increase* in global emissions of CO2 is increasing, and momentous changes are taking place in the developing world. These developments are, of course, connected and probably account for much of the tripling in the annual rate of change in emissions from 1.1%/yr in the 1990s to 3.1 %/yr in 2001-2006. At the heart of these emission growth rate changes is the development success story coming out of Asia. That story is increasingly associated with a huge shift in the location and relative importance of energy-intensive industries, which rely heavily on power generated from combusting coal.

The best example is China, which now accounts for 48% of the **world's production** of cement, 49% of the world's production of flat glass, 35% of its steel, and 28% of its aluminum (Rosen and Houser, 2007). The list could go on, but it suffices to say that these are among the world's most energy intensive industries with energy to output ratios ("energy intensities") about 10 times higher than those of most other manufacturing industries. The important point is that as development proceeds, rural populations move to cities, but to an increasing extent no longer to shanties and slums but to high rise buildings on broad streets that consume very energy intensive materials. This is a process that is likely to continue for decades, not only in China, but all over populous southeast and south Asia, and eventually in Africa, until well beyond the middle of the century.

As a result, we have only begun to see the surge in global energy use that the transformational development process now implies. And with that development process and energy surge will come a GHG emissions surge that will only terminate with a transformation of the world's energy systems. Not only will that transformation be a slow process, but the required energy technologies, for the most part, are not yet both ready and *scalable*. And when they are ready and scalable, it will likely require a huge technology transfer to the developing world before there will be a substantial payoff in CO2 emissions reductions.

Reference Scenarios and Baselines

In assessing what it will take to stabilize atmospheric GHG concentrations (in cost and technology terms), models usually employ no-climate-policy emission scenarios as references or baselines. However using such emission scenarios as baselines for assessing climate stabilization can mislead when it comes to assessing the amount of technological change needed (and, by extension, economic cost incurred) to stabilize climate (Pielke et al, 2008). The problem is that built into most emission scenarios are *assumptions* of very large amounts of emission reductions that will occur automatically due to technology change. In general, these "built-in" technologies require no attention to energy policies beyond following a business-as-usual pathway and by definition do not include any future climate policies. Such assumed emission reductions appear to have been ignored by analysts (exceptions include Battelle, 2001, Edmonds and Smith, 2006 and IPCC WGIII, Ch3, 2007) most of whom implicitly assess the magnitude of the energy technology challenge by looking at what it takes to move from an emission scenario (with its "built-in" emission reductions) to an emissions path consistent with some stabilization level.

An illustration of these issues can be found by looking at the widely-cited Pacala and Socolow (*Science*, 2004) paper. Pacala and Socolow (P&S) introduced the "wedge" concept and estimated it would take 7 technology wedges, each equivalent to a cumulative emissions reduction of 25GtC, to hold global emissions constant for the next 50 years. In conducting their analysis, however, P&S used a baseline CO2 emissions path (scenario) that already had 11 built in "wedges. (Socolow (2006) subsequently acknowledged the existence of these built-in wedges, referring to them as "virtual" wedges. Thus the true number of P&S "wedges" needed to maintain emission level constancy is 18 (7+11). About 60% of the challenge over the next 50 years, to say nothing of the challenge thereafter, had simply been assumed away.

To get around the problem of making assumptions about technology and a possible understatement of the future challenge, one can use a "frozen technology" baseline (Edmonds and Smith, 2006, Pielke et al, 2008). A "frozen technology" baseline assumes the future emissions will be the result of production using today's energy technology (hence the technology is "frozen"). Such a baseline then allows complete transparency in assumptions about future innovation and the processes that will lead to such innovation. Two such "frozen baselines" are illustrated (the dashed lines) in Figure 1 below (reproduced from IPCC AR4 WG III, Ch3, p.220, 2007) one each for the IPCCs B2 and A1B scenarios. The built-in (assumed) emission reduction attributable to spontaneous technology change is, in each case, the large and increasing *gray* area. The gray areas dwarf the "stabilization scenario" component, that is, the area that lies between the emission scenario pathway (the B2 and A1B lines in the figures below) and the 550 ppm stabilization pathway (B2 550 and A1B 550 in the figure). While Chapter 3 of IPCC WG III (Fisher and Nakicenovic, et al. 2007) recognizes the potential importance of built-in technologies, other chapters appear not to have incorporated the implications for that report's technology assessment or stabilization cost estimates.

Figure 1

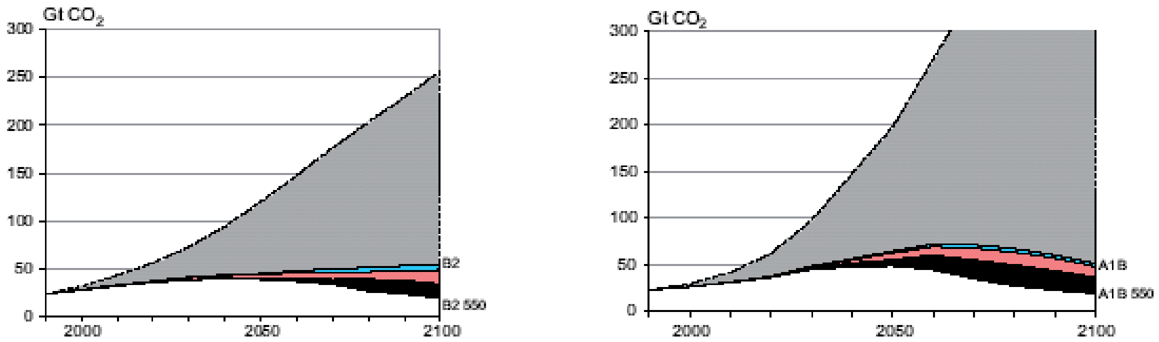

Figure 3.33: *Impact of technology on global carbon emissions in reference and climate mitigation scenarios.*

Note: Global carbon emissions (GtC) in four scenarios developed within the IPCC SRES and TAR (A2, B2 top and bottom of left panel; A1FI and A1B top and bottom of right panel). The grey-shaded area indicates the difference in emissions between the original no-climate policy reference scenario compared with a hypothetical scenario assuming frozen 1990 energy efficiency and technology, illustrating the impact of technological change incorporated already into the reference scenario. Colour-shaded areas show the impact of various additional technology options deployed in imposing a 550 ppmv CO_2 stabilization constraint on the respective reference scenario, including energy conservation (blue), substitution of high-carbon by low- or zero-carbon technologies (orange), as well as carbon capture and sequestration (black). Of particular interest are the two A1 scenarios shown on the right-hand side of the panel that share identical (low) population and (high) economic growth assumptions, thus making differences in technology assumptions more directly comparable.

Source: Adapted from Nakicenovic et al. (2000), IPCC (2001a), Riahi and Roehrl (2001), and Edmonds (2004).

Baseline Analysis

The vignettes in the preceding Section, although different in character, have a common denominator: enormous advances in energy technology will be needed to stabilize the atmospheric concentration of carbon dioxide. If much of these advances occur spontaneously, then the challenge of stabilization might be relatively simple and low-cost, as suggested by the IPCC. However if these advances require significant effort, then the challenge to stabilization could in fact be much, much larger than presented by the IPCC. The first two vignettes presented above allow us to view the energy technology challenge without the burden (yes "burden", not benefit") of GHG emission scenarios. The third implies that most emission scenarios—and this is certainly the case of most of the IPCC SRES scenarios—provide misleading baselines for assessing the energy technology challenge presented by climate stabilization, and, by extension, for estimating stabilization cost.

From an analytical standpoint it is the third vignette that needs pursuing. As indicated above, an alternative baseline for assessing the energy technology challenge to stabilization is the "frozen technology" baseline, as pictured in Figure 1. The advantage of a "frozen technology" baseline is that it allows the analyst to explicitly account for **all** technological changes required for stabilization.

Failure to so account may lead to **double counting** of technologies: once in the "gap" between the "frozen" technology baseline and the emissions scenario and again in the gap between the emission scenario and the stabilization path.

I am not aware of any stabilization analyses that indicate which specific changes in technology are built into the reference or emission scenario baselines and which are policy-induced. (Perhaps my knowledge of the relevant literature is too limited.) It is likely that stabilization models of the "top-down" variety, ones that invariably begin from a no-climate-policy (SRES-type) emissions scenario baseline, are particularly vulnerable to this problem. Although "bottom-up" models might, in principle, avoid this problem, the fact that most generate lower stabilization cost estimates than top-down models is an indication that these, too, are not accounting for all required technological changes in their stabilization assessments.

That these methodological concerns are important is indicated by as yet unpublished work by Wigley et al., (2007). These authors used the Pacala-Socolow concept of a "wedge" (i.e., a cumulative CO2 emission reduction of 25 GtC) to calculate the number of wedges built into the IPCC SRES scenarios, as compared to the number required to move from the SRES scenario to a range of stabilization scenarios, viz. the WRE (Wigley et al., 1996) stabilization paths. If we confine ourselves to the A1B and B2 scenarios, and to stabilization at 450 and 550 ppm, the Table below provides interesting findings.

Table 1

Emission Reduction Wedges Required to Follow Different WRE CO2 Concentration Stabilization Paths out to 2055

Scenario	Built-in Wedges	450 ppm*	550 ppm*
A1B	24.8 (86%)	13.4	6.5
B2	14.7 (80%)	6.5	0.7

* "wedges" needed to move from the SRES emission scenario to the specified WRE stabilization path

Table 1 demonstrates that even by mid-century technology "wedges" built-into the SRES scenarios dominate the number of technology "wedges" required to move from a frozen technology baseline to a stabilization path. (As Figure 1 indicates, the domination is much greater by 2100.) What is the nature of the built-in technology? In Table 1, the numbers in parentheses indicate the percentage of built-in "wedges" attributable to energy intensity decline—that is, to a combination of energy efficiency improvements and sectoral shifts in output from energy-intensive industries to less energy intensity industries.

Several points can be made about energy intensity decline in SRES emissions scenarios.

1. In general, the SRES scenarios build in high rates of energy intensity decline. Of the 40 scenarios (from 4 basic families, A1, A2, B1, and B2) 32 had 110 year (1990-2100) built-in energy intensity declines greater than the 1.0%/yr rate used in the BAU IS92a scenario. It is likely that, on balance, the energy intensity declines in the SRES scenarios are unrealistic.

2. Baksi and Green (2007), have devised a method, using mathematically exact formulas for computing aggregate energy intensity decline from changes over time in the efficiency of different energy-using sectors and their relative contributions to GDP and energy use. They find that *even after applying stabilization policies*, it would be very difficult to substantially exceed a 1.0%/yr *global*, *average,* rate of energy intensity decline over 1990-2100. Yet eighty percent of the *pre-policy* SRES scenarios build in 110 year global average annual rates of energy intensity decline that exceed 1.0%/yr.

3. The formulas generated by Baksi and Green (2007) can be used to demonstrate that only about 20% (bounds of 10-30%) of the **global** energy intensity decline can possibly come from sectoral shifts. The rest must come from energy efficiency improvement, which means technology change. While at the individual country level sectoral shifts can contribute considerably more than 20% of energy intensity decline, at the global level there is a lot of cancelling out as energy-intensive industries move from one part of the world to another.

4. Baksi and Green (2007) also demonstrate that achieving very high rates of energy intensity decline (ones that would greatly reduce the amount of carbon-free energy required for stabilization) require improvements in energy efficiency that are almost surely physically impossible. For example, Baksi and Green show (*supra,* Table 4, p. 8) that a 2.0% rate of decline (the B1 marker scenario has a 2.13% average annual rate of decline, 1990-2100), requires sectoral energy efficiency improvements ranging from 450 to 1100 %.

5. Yohe et al., (2007), use the SRES A2 emission scenario in their CC “challenge” paper. It is not clear why the authors chose A2, because that scenario is the most pessimistic of all the SRES scenarios. A2 has by far the highest 2100 population (15.3 billion), lowest global GDP growth rate (2.1% average over 1990-2100), and *lowest technology change* (e.g, only a 0.57% rate of energy intensity decline, 1990-2100) among the family of SRES scenarios. Fortuitously, the last characteristic implies that A2 has the least amount of built-in technology change. Wigley et al., (2007) calculate that out to 2055, only 4.7 wedges are built into the A2 scenario, while 12.7 and 6.9 additional wedges are required to stabilize at 450 and 550 ppm, respectively. (These numbers contrast starkly with the results for the A1B and B2 scenarios given in Table 1.)

Implications for Climate Policy

If, as seems likely, the SRES emissions scenarios have made CO2 stabilization appear much easier than it will be (Green and Lightfoot, 2002, Pielke, et al, 2008), then there are important implications for climate policy. First and foremost, a target-based climate policy that is focused on *ends* should give way to a technology-based policy that focuses on the *means* required to achieve stabilization. Further, there are implications for the relationship between a carbon-price policy and a technology policy. Instead of the carbon-price policy carrying the main load in the early stages, carbon prices should be viewed as playing two supportive roles: (a) as a means of raising revenues to finance the publicly financed component of the energy technology race without which stabilization is unachievable; and (b) as a way of sending a forward price signal that will be increasingly powerful as the carbon price slowly rises and as new technologies appear "on the shelf". The latter suggests that a carbon tax should start low and slowly, but automatically, rise over time.

In thinking about climate policy, an important distinction should be made between technologies that are "on the shelf" and therefore are deployable now (if it were economically advantageous to do so), and those that either (a) require further development before deployment is possible; or (b) are still at the basic R&D stage; or (c) have not yet been thought of. (Sanden and Azar, 2005). Carbon prices are likely to be effective in inducing deployment of technologies that are "on the shelf", but may well be ineffective inducements to invest, long-term, in technologies that still require basic R&D. The value of such technologies is therefore quite *uncertain*. Even if R&D proves them to be viable, they must be decades away from deployment. (Montgomery and Smith, 2005).

As Montgomery and Smith have demonstrated, private funding of long-term R&D may run into a "dynamic" (time) inconsistency. Generally, current governments cannot tie the hands of future governments to cover the potentially large (as well as uncertain) *up-front* R&D investment costs for technologies that may or may not prove successful and deployable decades hence. The Montgomery and Smith and Sanden and Azar papers therefore imply that "induced technical change" may be less important than one might gather from IPCC WG III Ch. 11 (Barker, et al. 2007). Further, to these considerations we may add a "political" time inconsistency between a 4 to 5 year election cycle and the decades-long time scale for the development of deployable and scalable carbon-neutral energy technologies. These factors suggest that in choosing a carbon tax (and any artificially-generated carbon price is and will be viewed as a "tax") one should focus on "political acceptability" rather than guess what effect such a tax might have on many decades worth of innovation. (In other words, we cannot simply rely solely on price mechanisms to stimulate the required technology R&D—or stimulate even a significant fraction of this R&D).

An equally important question is the extent to which the technologies required for stabilization are already "on the shelf", or almost so. In 2001, in its Summary for Policy Makers (SPM) IPCC WG III argued that "most model results indicate that known technological options could achieve a broad range of atmospheric CO2 stabilization levels, such as 550ppmv, 450ppmv, or below over the next 100 years, but implementation would require associated socio-economic and institutional changes" (IPCC, 2001:8). The IPCC defined "known technological options" as "technologies that exist in operation or pilot plant stage today…" (*supra* p.8n). In 2007, with only slightly more caution, the IPCC AR4 states in the SPM of its Synthesis Report (SYR) that "There is *high agreement* and *much evidence* that all stabilization levels assessed can be achieved by deployment of a portfolio of technologies that are currently available or expected to be commercialized in coming decades…" (IPCC SYR 2007: 22).

. As indicated above, the earlier IPCC (2001) technology claims were contested by Hoffert, et al (2002). One possible reason for the difference between the IPCC and Hoffert et al., is that the former may be judging technological adequacy from the standpoint of moving from a SRES emissions scenario, with a considerable amount of technological change already built into it, to a stabilization path, rather than from a frozen technology baseline In contrast, the methodology developed by Hoffert et al. (1998) avoids this trap because it calculates carbon-neutral energy requirements from the equivalent of a "frozen" technology baseline, *given the explicitly accounted for average annual rate of decline in energy intensity*.

There is another reason for the difference between IPCC WG III and the Hoffert et al (1998, 2002) technology assessments. This one revolves around the *scalability* of current carbon-neutral technologies. The scalability issue, emphasized by Hoffert et al. (2002), is more complex than may first appear. Some technologies are not yet scalable because they are still at the R&D stage. In some cases, apparently "on the shelf" technologies are also not yet scalable. These technologies include some that are often touted as the future means of powering the planet in a carbon-neutral manner (e.g. solar and wind energy, carbon capture and storage (CCS), and reprocessed, closed cycle, nuclear). An important limitation of these technologies is that they lack one or more "enabling" technologies required for scalability.

An example of an "enabling" technology is storage for intermittent and variable solar and wind power. These potentially large, but dilute energy sources are not only land-intensive (Lightfoot and Green, 2002), but of limited use without storage. Electric utilities generally will not be able to meet any more than about 10% of non-peak electricity demand from directly supplied, intermittent or variable sources. While pumped hydro, hydrogen, and compressed air energy storage can provide some storage potential, we are still very far from a good, reliable, and scalable means of storage for electricity generation and supply.

Similarly, CCS faces scalability issues on the storage side. While studies suggest that there is potentially plenty of storage capacity for CO2 emissions captured and geologically sequestered in the foreseeable future (Herzog, 2001, IPCC, 2005), as a practical matter each geological storage site needs to be checked for leakage potential. This will require a potentially time-consuming effort by a large number of geologists. Detailed examinations cannot be ignored: carbon dioxide leakage would not only limit the effectiveness of CCS, but create a public hazard because CO2 in concentrated form is an asphixiant that disperses only slowly if a leak occurs. It is true that there are a number of small scale examples of CCS, but there is nothing even remotely approaching the scale required for CCS to contribute significantly to reducing future net CO2 emissions. Finally, "conventional", once-through, nuclear fission is not only limited by uranium 235 supplies, but faces limitations with respect to storage of the large amounts of radioactive waste that would be generated even if nuclear simply maintained its current 17% share of global electricity generation.

Storage is not the only "enabling" technology that is required to make a number of carbon-neutral energy technologies viable. Other examples include retrofit technologies for the large and rising number of coal-fired plants, especially those in China, India and the US, or as an alternative CO2 capture from the air (Lackner, 2003; Pielke, 2007). While nuclear electric generation is an obvious low carbon-emitting alternative to coal, large-scale expansion will greatly increase the incentive to reprocess nuclear “waste”. However, doing so will require some means of “spiking” the resulting plutonium to make it too hot to handle by terrorists, and a means of preventing nuclear proliferation. While the latter clearly involves political ingenuity, it also involves science and engineering developments, as is indicated by the apparent technological as well as political hurdles ahead for the US-promoted Global Nuclear Energy Partnership (GNEP). (Tollefson, 2008)

Once the scalability problem is understood, it is easier to see why there is still a large technology gap between usable carbon-neutral energy with current technologies and the amount required for climate stabilization. Green et al. (2007), build on Hoffert et al. (1998), in attempt to measure the “advanced energy technology gap” (AETG), the gap between the carbon-neutral energy required for stabilization and the carbon-neutral energy that could be supplied from “conventional” sources. “Conventional” carbon-neutral energy technologies include: hydroelectricity (subject to site limitations); once-through nuclear fission (subject to uranium 235 supplies as well as security, political and waste storage limitations); solar and wind without storage; and some geothermal, tidal and wave (ocean) energies. The authors found that “conventional” carbon-neutral energy sources might supply 10-13 TW by 2100. Assuming 13 TW from these “conventional” sources, we still need 15-25 TW of power from advanced technologies (the AETG) to reach the 28-38 TW of carbon emission-free energy required by 2100 to stabilize at 550 ppm, assuming a 2.4 % rate of growth of GDP (1990-2100). These findings support the Hoffert et al. (1998, 2002) claim that major breakthroughs in new as well as existing energy technologies and sources will be required for stabilization at 550ppm, and even more so for stabilization at 450 ppm.

Implications for Copenhagen Consensus Benefit-Cost Assessment

Yohe et al. demonstrate that moderate mitigation combined with energy R&D passes, by a substantial margin, a B/C test, even when a “descriptive” rather than “prescriptively” low discount rate is employed. However, despite some explicitness on the technology side, their modeling does not, I think, capture just how difficult (and, in the absence of numerous technological breakthroughs, potentially costly) stabilizing CO2 concentrations at any acceptable level will be.

Yohe, et al, express some frustration at the CC budgetary constraint. I share their frustration. A climate policy limited *globally* to $75 billion/yr over four years seems to take climate change less seriously than it deserves. Climate change has a long time scale dimension, global reach, and such huge complexity that it arguably should be set apart from other policies considered by CC. There is, it seems, an “apple and oranges” problem with the CC budget constraint—the issues considered do not seem directly comparable and so are difficult to consider under a single common set of budget constraints.

Nevertheless, given the technology-based approach that is central to any feasible attempt to stabilize climate, the CC budget constraint may not be too binding in the near future. If $60 billion a year could be devoted *globally* to developing new energy technologies, especially those of an “enabling” sort, it would not take long before energy R&D would become a major global preoccupation. Substantial, scalable, “on-the shelf” technologies could be expected to follow. (*As this constitutes a separate proposal for the panel to consider, a benefit-cost analysis is found in the Appendix*.)

If the $60 billion were raised by a carbon tax, then even a tax with a 25% cost of public funds would stay within the CC budget constraint ($60 + .25(60) = $75 billion). A tax of $4 per ton CO2 on just 50% of the approximately 30 GtCO2/yr (~8GtC/yr) currently emitted would raise 60$ billion/yr. But frankly, if it were politically feasible, I cannot see why we cannot do better by starting with a more robust $8-10/tonne CO2, and then allow the tax to rise gradually over time. To keep within CC ground rules the extra revenues could be used to reduce other taxes that have even higher marginal costs of public funds.

Inconvenient Truths

Former Vice-President Gore was right that climate change is an “inconvenient truth”. But, there is more than one “inconvenient truth”. A second “inconvenient truth” is that climate change is a difficult-to-solve energy technology problem. That, of course, has been the main theme of this “Perspective”. Unfortunately, recognition (especially in climate policy circles) of the second “inconvenient truth” remains long overdue.

As a result, there is growing interest in “geo-engineering” climate (Crutzen, 2006, Wigley, 2006, Matthews and Caldeira, 2007) to reduce the impact of rising concentrations of GHGs on climate, if not the emissions themselves. There are several rationales for the interest in geo-engineering. These include: (i) its apparently relatively low monetary cost; (ii) the belief that policy-makers will not act quickly enough to reduce emissions sharply, and (iii) a perceived need to buy time for requisite new and scalable technologies to reach fruition. However, as Wigley (2006) stresses, geoengineering cannot be used as a replacement for mitigation. Mitigation directed towards GHG (particularly CO2) concentration stabilization is essential; in order to keep ocean acidity from rising to levels that might seriously damage the ocean biosphere, so geoengineering can only be seen as a way to gain time to develop and deploy appropriate mitigation technologies, or as an emergency response should climate changes occur more rapidly than currently projected.

While research on the possible use and practical feasibility of geoengineering is important (including possible adverse consequences), priority should be given to mitigation research and the energy technology challenges that GHG stabilization presents. As indicated above, the annual cost of such a race could for the time being stay within very conservatively defined budget constraints. Such a research effort could make use of the huge amount of brainpower in an increasingly educated *global* population of 6.5+ billion. It would also give the next and following generations a large challenge (one that motivates because it is based on *creativity* rather than on *sacrifice*). Moreover, a science-driven energy technology program could have numerous beneficial *spillovers* into other uses.

Still, there is legitimate concern that an R&D spending spree could be more wasteful than helpful. What is lacking is the design of an incentive-compatible (energy) technology program. Possibly, competing international consortias could do the trick (Green, 1994)—but this idea requires more thought. While a few economists have given attention to the R&D process as it may apply to climate change (e.g, Blanford and Clarke, 2003), it seems to me that more attention to (practical) mechanism design and incentive compatibility would be highly desirable.

There is a final matter. Failure to take a real shot at tackling the “second inconvenient truth” increases the likelihood that we will face still another “inconvenient truth”. If for lack of technological focus we must turn to geoengineering, we may have to make lose-lose choices between possibly deleterious side effects of geoengineering and unavoidable GHG-induced climate change. That is a “third inconvenient truth”, and probably not the last that climate change will throw at us.

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APPENDIX

Benefit Cost Analysis

The Copenhagen Consensus calls for applying benefit-cost analysis whenever a specific program or policy is proposed. This Perspective has accented the role of technology in “stabilizing climate”, suggesting that the magnitude of the technology challenge is so large that stabilization is essentially unachievable without a successful energy technology program/race. As a result, some attention to quantifying the relative benefits and costs of a technology-based climate policy is required.

Applying benefit-cost analysis to a technology-based program aimed at stabilizing climate is a dubious proposition. The results, timing, scalability of successful technologies, and success in reducing their costs on the one hand, and the climate change damages avoided (benefits) on the other are so uncertain as to make benefit cost assessment little more than impressionistic. Of more concern is that benefit cost ratios could easily distract from the message of the paper: that not only is the technology challenge to stabilization huge, but analyses that use emission scenarios as baselines systematically understate the magnitude of the challenge.

To partially avoid these pitfalls, I employ a very simple analytical framework, which could easily be adjusted by the (disbelieving) reader. I have placed the benefit-cost exercise in an Appendix so that it may be less obtrusive to the main argument of the Perspective.

Assumptions

The framework that I employ assumes that:

(1) for a **50 year** period (say 2010-2060), the accent is on an *incentive compatible* technology race financed by a carbon tax that yields at least $60 billion constant dollars annually. Thus over a 50 year period global R&D spending on new energy technologies would total $3 trillion. As indicated in the text, I assume a marginal cost of public funds of 25% bringing the annual cost to $75 billion.

(2) over the course of the 50 year period enough successful, deployable and scalable technologies will appear that together they are capable of displacing carbon emitting ones at a pace that makes possible the eventual stabilization of climate, even if for a time carbon concentrations “overshoot” the long term level at which they must (to avoid mounting damages) be stabilized. (The methods of, and costs associated with, inducing deployment of successful technologies once they reach the “shelf” are ignored in the benefit-cost analysis. The best that I can say, here, is that (i) a rising carbon tax can offset other taxes, and (ii) technology standards, although they may carry allocative inefficiency baggage, generally have low administrative costs.)

(3) serious climate change-related damages only begin 50 years hence (in 2060). “Avoidable damages” (d) are assumed to constitute 3% of gross world product (GWP) from 2060 on.

Instead of using an infinite horizon, I focus on the avoidable damages from 2060 to 2200. The avoidable damages constitute the "benefits" in the analysis.

(4) The growth rate of GWP from 2010 to 2060 is 2.5%, rising from approximately $32 trillion US (2000) dollars in 2000 (an estimated $41 trillion in 2010) to $140 trillion US (2000) dollars in 2060.

(5) The average annual growth rate (g) of GWP from 2060 to 2200 is 1.8%.

(6) All values are discounted back to the present at a rate (r) of 4%, an assumption that will be modified in the sensitivity analysis.

Benefit-Cost Ratios

These assumptions are used to provide a "central" benefit-cost ratio (BCR). The present (2010) discounted (at 4%) value of the annual $75 billion costs ($60 billion R&D plus $15 billion marginal cost of public funds to finance them—see text) over the 50 year period (2010-2060) is $1.676 trillion. The present value of the benefits (avoidable climate damages, 2060-2200, if the R&D program succeeds in developing sufficient to stabilize, scalable, deployable and competitive new non-or low-carbon emitting energy technologies), is $26.5 trillion. These yield a ratio of benefits to costs of **15.8:1** (that is a BCR of **15.8**). Clearly the "central" BCR value passes a benefit-cost test by a wide margin. Before qualifying what some may view as a dubious finding, I provide some sensitivity analysis, to give a tentative range of estimates of the BCR.

Suppose instead the discount rate (r) is raised from 4 to 5%, and the avoidable (climate) damages are reduced from 3 to 2% of GWP (2060-2200). The discounted (back to 2010) value of the 50 years of R&D expenditure plus marginal cost of public funds falls to $1.438 trillion (from $1.676 trillion in the *central* case). The benefits (the 2010 present value of avoidable damages) decline even more dramatically to $7.9 trillion (from $26.5 trillion in the *central* case). The BCR is now much lower at **5.5**, but still well above the margin.

Alternatively, suppose that the discount rate (r) is reduced to 3%, but the post-2060 GWP growth rate is reduced to 1.5%. The (avoidable) climate damage rate is assumed to be 3%, as in the central case. The resultant present value of the costs (R&D outlays plus marginal cost of public funds) rises to $1.937 trillion (compared to the central value of $1.676 trillion.) The benefits (present value of avoidable climate damages) rise dramatically to $57.3 trillion (compared to the central case of $26.5 trillion). The BCR now is **28.8.** Although the plausibility of the upper bound may be doubtful, it may not be unreasonable given the assumptions of a successful technology race and the possibility of (unaccounted for) positive "spillovers" of new technologies into non-energy fields.

Benefit-Cost Exercise: Summary

	Lower Bound	Central Case	Upper Bound
r	5.0%	4.0%	3.0%
g	1.8%	1.8%	1.5%
d	2.0%	3.0%	3.0%
BCR	**5.5**	**15.8**	**28.5**

Qualifications

While it might seem natural to qualify with respect to the model framework and parameters, I shall not pursue that course. (The reader is more than welcome to make modifications and see whether any of these upset the general conclusion that a successful energy technology race would pass a benefit-cost test with flying colors.) Instead, I will comment on the assumption that I believe most requires qualification: that the R&D program/race proves sufficiently successful over the 50 year period to provide the means of displacing carbon emitting energy technologies/sources to an extent sufficient to avoid major climate damages.

The reason why the “success” assumption requires attention is that even if the world’s scientific/engineering talent (human capital) and its scientific facilities are up to the task (as I think they are or can be) and the funding is adequate, success is not assured. The “program” could still generate more waste than results. Everything from (a) “picking winners”, to (b) “lock-in” to new technologies that could turn out to be inferior to those that come along later, to (c) bureaucratic overload, to (d) turf disputes, to (e) the influences of lobbyists and ideology could derail a technology race

For this reason, in the concluding section of the text, I suggested that economists (and other social scientists too) might do well to give attention to the design of an incentive compatible energy technology race. Here one might think in terms of *practical* “mechanism design”, which could potentially have very large (social) payoffs.

There is another issue that throws some light on the benefit-cost exercise. I will term it the “what is the alternative?” (to a successful energy technology program/race). Suppose that in order to avoid large GHG-related damages, emissions must be cut more or less in half (from current levels) **by 2100**. (Some scientists think they need to be cut in half by 2050.) To do so would require emissions falling at something like an 0.8% *average annual* rate between now and 2100, although likely *rising* for the first few decades and then declining at a much faster (than 0.8%) rate later in the century. The reduction can be achieved either by (i) cutting the rate of growth of GWP, or (ii) technology change that allows for a dramatic decline in the carbon intensity of GWP (C/GWP).

For carbon emissions to **decline** at an *average annual* rate of 0.8% (2010-2100) while GWP grows at an *average annual* rate of 2.2% (the rate implied by the values in the "central" case), requires the carbon intensity of GWP to decline at an *average annual* rate of 3.0%. That is: -0.8%=2.2%-3.0%). What has been our actual experience? In the last few decades of the 20th century, the average annual rate of *decline* in the carbon intensity of GWP was about 1.4%. In the first five years of the 21st century C/GWP has declined at about a 0.7% rate, when GWP measured in purchasing power parity (PPP) terms, and has actually *increased* when GWP is calculated using market exchange rates.

While the recent drop in the rate of decline in C/GWP may be temporary, there is certainly no reason to expect it on its own to rise dramatically above the rate that prevailed in the latter part of the 20th century. But even if we could achieve a century-long 2.0% rate of decline in the carbon intensity of energy with "conventional" low-carbon emitting technologies, we would still fall far short of the "required" 3.0% rate of decline in C/GWP (required if emissions are to be cut in half by 2100 while maintaining a 2.2% "trend" rate of growth in GWP).

Something has to give! Either we engineer a successful energy technology race, dramatically reduce the growth of GWP *force majeur*, or we simply end up with accumulating climate damages. Consider a *force majeur* growth rate cut. Suppose a cut in the growth rate of GWP from 2.2% to 1.8% is required for the period 2010-2100. That would cost (an undiscounted) $85 trillion in 2100 alone and an undiscounted $2225 trillion *cumulative* over the 90 year interval. And that would not do the trick if we cannot push the rate of *decline* in C/GWP up to something close to 2.6%. Finally, compare the $2225 trillion with the 3.75 trillion R&D spent on a successful energy technology race. Where the latter produced very sizeable "benefits" (BCR>1), the former would surely produce a BCR<1, and almost certainly one that is <<1.

Adaptation as an Option

What about adaptation? Is it an option? Realistically, no matter what we do some adaptation will be necessary? But is "stand alone" adaptation an option? Yohe at al, include adaptation in their package of proposals. However, stand alone adaptation (with its three-digit BCR) is limited in their paper to "*ex post*" expenditure to alleviate malnutrition and health that would accompany incomplete mitigation—that is, mitigation that would still allow the global average temperature to rise by at least 3°C. Otherwise adaptation to climate change is factored into the Yohe at al estimates of climate change damages. The Yohe at al BCA implies that adaptation to climate change is not a stand alone option; mitigation, too, is required. On the surface this seems reasonable.

But suppose that mitigation requires a *force majeur* growth rate cut because of insufficient attention to, or insufficient success in, the research and development of breakthrough energy technologies. If the cut in the GDP growth rate were from 2.2% to 1.8% (below I show that the growth rate cut would probably have to be larger than this), then the undiscounted costs of reducing emissions to 4GtC by 2100 would be $2225 trillion, as shown above. Suppose instead that a policy of "doing nothing" except adapt were chosen, and that the resultant damages (after adjusting for adaptation) were 10% of GWP every year from 2010-2100. The cumulative (undiscounted) cost of adaptation would be $1,107 trillion, a little less half of *force majeur* mitigation costs of $2225. Thus looked at in **opportunity cost** terms (the benefits of adaptation are the mitigation costs foregone), the BCR for stand alone adaptation (relative to *force majeur* mitigation) exceeds 1 (is ~2 in the example, to be more precise). And note that even if the damages were 20% of GWP, à la Stern Review (2006), the undiscounted damages (2010-2100) would be $2214 trillion, slightly less than the *force majeur* mitigation costs.

Thus it would appear that stand alone adaptation may be an option in the absence of a successful technology program/race that this "Perspective" has argued is essential to climate stabilization. Moreover, a broader view of adaptation that includes non-climate change related benefits from undertaking adaptive investments (Pielke, 2005, 2007) would tend to reinforce this conclusion. *But this cannot be the end of the adaptation story.*

If we now compare "stand alone" adaptation to the approach proposed in this paper (a major, long term energy technology program/race with mitigation tailored to deploying scalable, reasonably cost-competitive carbon-neutral technologies after they have reached the "shelf"), the picture is altogether different. Even if climate change damages were only 1% of GWP each year, the (undiscounted) costs would be a cumulative $110 trillion (2010-2100). This amount is almost 30 times the (undiscounted) cumulative R&D expenditures of $3.750 trillion (which includes the marginal cost of public funds associated with a carbon tax used to finance the R&D—see above). The opportunity cost of stand alone adaptation would be huge, and its BCR<<1.

Finally , it is useful to show why a *force majeur* approach to mitigation could reduce the growth rate by much more than the 0.4 percentage points that was taken off the "trend" growth rate (from 2.2% to 1.8%) in the example above. Suppose that energy intensity decline (2010-2100) is 1.1% and that over the same period "conventional" carbon-free energy is raised from the current 2+ TW to 13 TW (Green et al, 2007, and discussion on p. 9 of text above). These assumptions imply a 1.5%/yr rate of decline (2010-2100) in the carbon intensity of GWP (C/GWP). But if emissions are to be reduced to 4GtC by 2100 (that is, at a ~0.8%/yr rate), then the GWP growth rate would have to be limited to an average of 0.7% (0.8%=0.7%-1.5%). The cumulative loss in GWP would be $6080 trillion. Enough said!