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**HOUSE COMMITTEE ON GOVERNMENT REFORM**

**“CLIMATE CHANGE TECHNOLOGY RESEARCH: DO WE  
NEED A ‘MANHATTAN PROJECT’ FOR THE  
ENVIRONMENT?”**

Thursday, 21 September, 2006

Testimony By  
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## Introduction

My name is Lee Lane. I am the Executive Director of the Climate Policy Center, a non-profit, bipartisan Washington DC-based policy research organization seeking to analyze climate policy options and to promote economically efficient policy responses to the challenge of climate change. CPC is supported primarily through grants from non-profit foundations.

To begin, I wish to thank Chairman Davis and the House Committee on Government Reform for calling this hearing. For reasons that I will explain, I believe that in doing so the Committee is posing the single most important question in climate policy – how we can best accelerate progress toward technological solutions to climate change. Within that larger, question a focus on exploratory research and how to organize it also seems to me to be precisely on target.

My testimony this morning will begin with an explanation of why making organizational improvements to the US Climate Change Technology Program (CCTP) is extremely important to the prospects for long-term success in coping with the challenge of climate change. Then I will describe three near or mid-term steps that could potentially improve the existing program. My three candidates for prompt action are as follows:

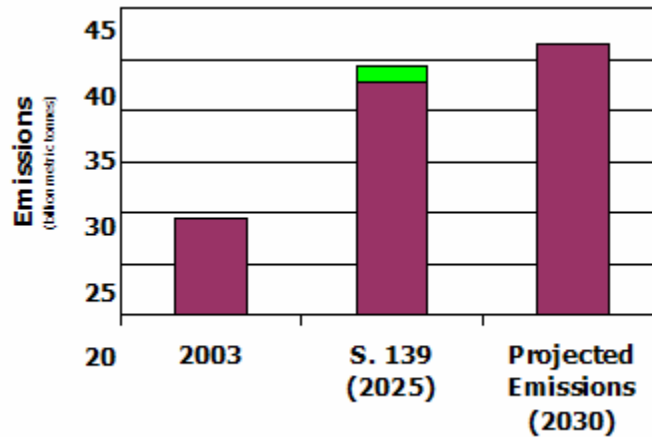
1. Create a new entity to conduct government-funded research aimed at making high payoff scientific and engineering advances capable of dramatically reducing the potential future harm from climate change.
2. Conduct the R&D needed to make more informed choices about the potential pluses and minuses of various geoengineering responses to climate change and to lower the costs of adapting to climate change.
3. Congress needs to provide the management of the Climate Change Technology Program (CCTP) and to ensure that its recommendations and analysis receive due attention in future budgeting and organizational decisions about CCTP.

## Government-funded R&D, a key to climate policy

Cost-effective government funding for R&D will be essential in meeting the challenge of climate change. Without truly revolutionary technological advances, comprehensive Greenhouse Gas (GHG) emission limits are likely to prove ineffectual in the industrialized world and not to be implemented at all in China and India.

Although the industrialized countries, including eventually the US, may adopt GHG controls, with current technology – *and given realistic assumptions about social willingness to pay for GHG abatement* – such controls will do little to curb the growth in global emissions. Thus, according to the US Energy Information Agency's estimate, by 2025, even the original version of S. 139 would have reduced global GHG emissions by a paltry 2.6 percent. To illustrate the point, Figure 1 shows the estimated emission reductions from the original version of S.139. It compares those hypothetical emission cuts with projected global GHG output and growth. Of course, for a still higher cost, controls could more tightly limit emissions. Yet the sponsors of S.139 withdrew this bill in favor of a less ambitious version. They took this step because they judged the initial bill's high costs as politically unacceptable, and even the scaled-back version has failed twice to attract majority support in the Senate.

Figure 1



In theory, more could be accomplished if China and India could be induced to curb their GHG emissions. But with current abatement costs, those countries seem most unlikely to institute GHG controls. Both the Chinese and Indian governments are under tremendous pressure to rapidly industrialize their countries. Strong GHG control measures would impede industrialization. These governments feel little domestic pressure to halt climate change. Even with great technological progress, the task of lowering Chinese and Indian emissions will be difficult and time consuming. Without new technology, the hopes of significantly limiting Chinese and Indian GHG emissions seem largely fanciful. (Even with a good deal of technological progress, this goal may be difficult to reach.)

Some proponents of GHG cap-and-trade policies argue that imposing controls would stimulate enough private sector R&D to produce the technological innovation needed to drive down abatement costs. While not entirely unfounded, such hopes are often exaggerated.

To a degree, placing a price on carbon emissions must occasion some change in the pattern of private sector R&D. However, GHG limits' induced-innovation effect will be small. Almost all economists recognize that market forces call forth a less than the optimal quantity of R&D. Once a private sector innovator demonstrates a new technology's feasibility and profitability, competitors are likely to imitate it. The imitators would escape paying the high fixed costs required to make the original discovery. Therefore, copycats could gain market share by undercutting the innovator's prices. By doing so, they deprive the initial developer of most of his hypothetical financial gain. Foreseeing this competitive response, firms avoid investment in many R&D projects that might have been profitable had the innovator been able to capture the full reward of his success.

The private sector's reluctance to rely on R&D strategies is likely to be especially strong with the kind of R&D activities needed to reduce GHG emissions. No large inexpensive emission-free energy sources lie just over the horizon. Thus, successful innovation in this area will require unusually high risks and long delays. As many economists (most recently Edmonds and Stokes) have noted because developing these technologies will entail breakthroughs in basic science, much of the most essential work will be ineligible for patent protection. These are precisely the conditions in which firms are least likely to invest in on R&D as an approach to problem solving.

I do not wish to overstate this case. Putting a price on GHG emissions would certainly encourage private sector R&D designed to commercialize new technology. Indeed pricing emissions is

almost certainly the most cost-effective way to encourage such commercialization. Nonetheless, as the recent editorial of the journal *Nature* noted, government funding must play the central role in making the scientific breakthroughs without which no long-term climate policy success will be possible. (See Attachment A)

## **Climate Change Technology Exploratory Research**

If only one change in US government climate policy could be made, it should be to initiate and sustain a properly organized exploratory research program like that described in Attachment B. Attachment B is a paper drafted by four accomplished scientists and me. It describes one possible ‘straw man’ proposal for how such an exploratory research program might be structured. My colleagues and I dubbed the proposed program Climate Change Technology Exploratory Research (CCTER).

Such a new program would conduct R&D aimed at fostering potentially high payoff (even if high risk) technological solutions to climate change. The key problem that this proposal is designed to address is the rigidity of the government’s climate change technology R&D and the high entry barriers that often prevent innovators from gaining government financial assistance for testing and developing their ideas.

In particular, the existing program has seemed to slight longer-term potentially big-payoff high risk ideas. The organizational culture of the Department of Energy (DOE), its energy industry constituencies, and the legislatively mandated requirements for industry partnerships virtually ensure temporal myopia in DOE’s research agenda. But the short-run focus clashes with the long-run climate policy vision. In the words of the report summarizing the conclusion of the expert workshops that recently reviewed CCTP’s draft strategic plan:

The need to place more emphasis on long-term, revolutionary technologies as well as programs that foster truly innovative, unconventional approaches emerged as essential for meeting the century-long challenge of climate change. This might require a greater percentage of funding to go to potential breakthrough technology versus research that provides only incremental improvements. There is a need for more high-risk but high payoff research. (Brown *et al.* xi)

Without structural change, the more innovative longer-run orientation seems unlikely to receive the optimal attention or funding.

As one possible solution, my colleagues and I proposed creating a new organization that would provide seed money to a number of innovative concepts that fall outside the current R&D portfolio of CCTP. Eligible concepts might be classic ‘Pasture’s Quadrant’ research, *i.e.* basic research with a high potential to advance technologies relevant to coping with climate change. They may also be more applied research.

The key criteria would be that the concepts not duplicate CCTP’s current work and that their success would make a large improvement in the costs of halting climate change or coping with it. CCTP might not explore meritorious R&D concepts because they are more risky or more long-term; because they are narrowly focused on climate; because they are too multi-disciplinary or cross-cutting; or simply because they are too innovative. In all these cases, CCTER would be a possible alternative source of seed money. The Defense Advanced Research Projects Agency

(DARPA) experience seems to suggest that launching a systematic search for technological solutions that we cannot initially name or clearly describe can produce amazing results.

As we conceived it, CCTER's internal mode of operation would resemble that of DARPA. CCTER would adopt a flat organizational structure. Recruiting high quality staff would be a priority as would limiting the influence of purely bureaucratic constraints. It would use grants, contracts, and in my view prize competitions to produce research. It would not conduct intramural research – or very little. Competition through peer review and (perhaps) inducement prizes would determine specific financial rewards. (I do not recall that we actually considered inducement prizes in the discussions surrounding the preparation of our paper, but it is a tool that I personally believe deserves to be included in an exploratory research program.) Solicitations for potential grants, contracts, and awards would be broad, ideally worldwide.

Annual steady state funding would be at the level of \$35-50 million. This number comes with the caveat that most of us thought that eventually higher expenditure levels would be desirable although we were conscious of the need to ramp up spending cautiously. One advantage of more funding would be that it would allow more opportunities to demonstrate the promise of technologies before requiring that further support come from traditional government or private sector R&D programs.

Our hope is that the private sector would supply some of CCTER's money. This would also keep CCTER personnel in close touch with market realities. However, I personally believe that the program's *raison d'être* as a search for unconventional and highly innovative technologies would probably hold private sector investments to fairly low levels.

One key organizational question is whether the goal should be to create CCTER or an Advanced Research Projects – Energy (ARPA-E). In principle, if an ARPA-E were created in DOE, it could be charged with climate-related exploratory research. This arrangement would entail some risk of losing the program's climate change focus amidst more pressing issues of energy price and security. In any case, for the next two years, the Administration's opposition to the ARPA-E concept effectively removes this option from the table. Under the circumstances, only a more narrowly climate-focused program like CCTER may be feasible.

Determining how to fit CCTER into the larger structure of government is harder than envisioning how it might function internally. The option on which we settled was a private non-profit corporation. It would be governed by a board of directors that would include representatives of both government and the private sector. Government financial support would be funneled through DOE, but CCTER's governance arrangements would insulate it from bureaucratic interference.

Other options that we considered were to create an exploratory research program within DOE and to create a federally-funded R&D center under DOE. For my part, work by Dr. Van Atta on DARPA's history suggests that an innovative high risk R&D operation cannot survive and prosper within a federal bureaucracy without a high degree of top management support. The current views of DOE toward exploratory research and the concept of an ARPA-E raise serious doubts about whether that Department would provide a suitable environment for this kind of enterprise.

## **Back-up strategies for climate change**

Currently, CCTP is focused exclusively on technology that would reduce GHG emissions. The program explicitly excludes R&D on geoen지니어ing and adaptation. (US CCTP [Draft] Strategic

Plan 2-2 note 2) This decision does not appear to have received an appropriate level of attention. To underscore this point one recommendation of the expert workshops conducted in the wake of the draft plan's publication was to expand the program's agenda to include geoengineering. (Brown *et al.* xi)

Broadly considered, three technology strategies are available for dealing with climate change. First, technological progress can lower the cost of GHG abatement. CCTP focuses on this goal. Second, technology can enable what is called 'geoengineering', use of technologies that would avoid harmful climate change even though emissions continue at relatively high levels. (Among other options, geoengineering could involve increasing earth's albedo (the proportion of sunlight striking the earth that is reflected back into space) to offset the warming effects of rising GHG concentrations.) Third, technological innovation could aid adaptation. Technologies like heat and drought resistant crops, stockpiling genetic material from endangered species, or hydrological projects that minimize the costs of rising sea levels can minimize the costs of the climate change that is already inevitable.

Some mix of the three technology strategies is most likely to meet the goal of minimizing the sum of the costs of climate change and the costs of countermeasures taken against it – the definition of an optimal climate policy. CCTP should develop the suite of technologies best able to implement this cost-minimizing strategy. Each of the two missing strategies is, in fact, potentially crucially important to overall success.

The fact is that some degree of human induced climate change is already inevitable. It is hard to be certain whether it will be a lot or a little or what affects it may have. But as one looks farther into the future, the chances of larger and more disruptive impacts grow. To many scientists the preferred policy response to these problems is to restrain GHG emissions so as to minimize the future risks. However, to some degree, as the experience with the Kyoto Protocol suggests, such efforts have already failed. Furthermore, some prominent economists who have studied the difficulty of forging an international agreement on GHG controls conclude that it may be impossible to do so, at least for several decades. If they are right, and so far the Kyoto Protocol experience suggests that they are, a substantial degree of climate change is inevitable.

Adaptation could significantly reduce net damages from this climate change. Many recent climate change damage estimates typically fall below those of earlier studies because the more recent studies have better accounted for adaptation. (Joel B. Smith 31) Adaptation's evident power to decrease damages suggests using R&D to boost that power.

It [adaptation] means *inter alia* pushing ahead with both the basic science and applications of genetic engineering in many areas, especially agriculture, but also to provide potential substitutes for possible useful species that may be lost. That could be supplemented by a systematic program for collecting, cataloguing, and storing genetic material, mainly but not exclusively from plants, in the form of seed banks and DNA. (Cooper 1999, 43)

Other relevant work might involve a systematic study of how public and private infrastructure might be adjusted to minimize the costs of climate change. It may even be worth exploring adaptation's role in US assistance to foreign countries.

Taking the logic of adaptation one step further implies conducting R&D on geoengineering. Climate policy must cope with the possibility of low probability but high cost events. (Nordhaus & Boyer 98) Should the climate system manifest a large and harmful discontinuity, having a mechanism for ‘scramming’ the climate change process could prove invaluable.\*

Indeed, unless we are prepared to assign a zero probability to “nasty surprises” from climate change, there seems good reason to undertake such research. (Keith and Dowlatabadi 293)

Geoengineering in the climate change context refers mainly to altering the planetary radiation balance to affect climate and uses technologies to compensate for the inadvertent global warming produced by fossil fuel CO<sub>2</sub> and other greenhouse gases. An early idea was to put layers of reflective sulfate aerosol in the upper atmosphere to counteract greenhouse warming. Variations on the sunblocking theme include injecting sub-micrometer dust to the stratosphere in shells fired by naval guns, increasing cloud cover by seeding, and shadowing earth by objects in space. ... Climate model runs indicate that the spatial pattern of climate would resemble that without fossil fuel CO<sub>2</sub>. Engineering the optical properties of aerosols injected to the stratosphere to produce a variety of climate effects has also been proposed.” (Hoffert *et al.* 986)

As insurance against runaway climate change, research on geoengineering may be the only available option. Because emission reductions require so much time, a GHG control policy must be initiated many decades before it achieves its full effects – and long before the full extent of the problem is entirely visible. Our political systems are not always far sighted enough to respond to such long-term threats.

Geoengineering, in contrast to GHG controls, could be implemented swiftly. For one thing, reaching international agreement for geoengineering would probably be largely about the sharing of monetary costs, a type of negotiation for which we have much experience. (Schelling 2005 592) Meantime, the costs would be confined to the R&D needed to prove-up the technology’s feasibility. The future costs of actually deploying geoengineering solutions are highly uncertain; however, in the early 1990s, the U.S. National Academies of Science, after studying geoengineering, concluded, “Perhaps one of the surprises of this analysis is the relatively low cost at which some of the geoengineering options might be implemented.” (NAS 1992 460)

The logic behind an exploration of geoengineering is so strong that it is beginning to erode the taboos that have hitherto blocked its consideration. The newly-elected president of the National Academy of Science has become an advocate for exploring various geoengineering concepts. A growing number of other scientists, including Nobel Laureate Dr. Paul Crutzen of the Max Planck Institute have begun proposing possible approaches. Yet these scientists note the continuing absence of governmental support for the exploration of the various technologies. (Broad 1-4)

At this point, however, the pros and cons of the various approaches to geoengineering remain poorly understood. The cost, effectiveness, limits, side effects, and ancillary benefits are matters

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\* A ‘scram’ is the rapid emergency shutdown of a nuclear reactor or other system.

of growing speculation, but little empirical research. The social and political dynamics of geoengineering solutions have also not been systematically explored.

To some degree, CCTER would be a good place to conduct early R&D need to launch the exploration of geoengineering and adaptation (especially geoengineering). Nevertheless, there is really no good reason for CCTP not to add geoengineering and adaptation to its research agenda. Doing so would necessarily change existing funding patterns. However, early stage R&D in these areas is likely to be relatively inexpensive, so the adjustment would be small. After all, the whole point of CCTP and its strategic plan is to periodically retune the program's spending pattern to optimize the development of the portfolio of new technologies best able to cope with climate change.

## **More support for CCTP management**

The long delays in the release of the CCTP strategic plan illustrate a persistent problem. The planning process has been starved for resources. The resulting nearly four year gap between the program's inception and the finalization of the first strategic plan is understandable, but disappointing.

Then too, in some important respects, the scope of the Draft Strategic Plan was undesirably narrow. To the authors' credit, they highlighted the omission of geoengineering and adaptation and solicited the comments of those who might disagree. Yet the only apparent rationale for excluding these areas was that the government was not in fact doing R&D in these areas.

This comment is not intended as criticism of the people who drafted the CCTP strategic plan or their decision to narrow the plan's scope. Given the resource constraints, that judgment was probably correct. The narrowness of the resource constraints, however, is just the point. Because planning resources were so limited, the planning process could not adequately address important issues.

CCTP may also lack influence on wider technology policy. The draft strategic plan correctly designates supporting technology policy as a CCTP "core approach". (US CCTP [Draft] Strategic Plan 2-10 – 2-11) However, last year's energy legislation authorized several new putatively climate-related subsidies and tax subsidies – several of questionable cost-effectiveness. Does CCTP have the resources and standing to conduct analyze these larger policy questions and to win a serious hearing for its findings?

Finally, what will be the significance of the plan now that it is approaching its final form? A recent National Research Council study of the Climate Change Science Program noted the importance of a "Leader with sufficient authority to allocate resources, direct research effort, and facilitate progress." It also called for "A strategy for setting priorities and allocating resources among different elements of the program (including those that cross agencies) and advancing promising avenues of research..." (NRC 80) These requirements are equally crucial to the success of CCTP. So far, though, if the issue of exploratory research is any indicator, CCTP has had little apparent influence on the distribution of DOE spending.

## **Conclusion**

I do not wish to appear too negative about CCTP or about its strategic plan. The people working on drafting the strategic plan have performed yeoman service. While the plan is in some ways less than I would have wished, it puts the US vastly ahead of the rest of the world in establishing

a framework for its climate-related R&D. Many representatives of both European and Asian governments have privately admitted to me that they cannot even say how much their governments are spending on climate-related R&D, let alone articulate a coherent plan for the long-term future. The initial plan and our approximately \$3 billion in annual expenditures compare favorably with other efforts.

Moreover, the US has many pressing demands on our national R&D resources. National and homeland security, healthcare, the need for continued growth in economic productivity all come(s) readily to mind. Fiscal considerations, if nothing else, dictate that we may not be able to have all the R&D we would like. Given these competing demands, I would still wish to see climate-related R&D spending expand and several expert panels have also endorsed this idea. At the same time, it seems unlikely that climate relate R&D spending will burgeon to the unprecedented levels that some hope to see.

R&D cost-effectiveness is, therefore, likely to remain a high priority. I have proposed three steps to boost cost-effectiveness. These are, first, to create a climate-related exploratory research program outside of the DOE bureaucracy, second, to add preliminary R&D on geoengineering and on adaptation to the existing CCTP and third, to ensure that the management staff of CCTP has the resources to do their job and that their analyses receives due consideration in budgeting and management decisions.

By airing some of the potential problems with today's climate-related R&D and exploring the alternatives, this hearing contributes importantly to the search of climate policy solutions. It is an important step. And I commend Chairman Davis and the Committee for undertaking it.

# Energy shame

The history of energy research highlights the importance and inadequacies of markets, and a yawning gap in the priorities of governments. It's time for a radical change.

Frances Cairncross, chair of Britain's Economic and Social Research Council, has been thinking about the economics of climate change longer than most natural scientists and economists. In her presidential address to the annual meeting of the British Association for the Advancement of Science this week, she rightly emphasized one of the most important things that governments can do: invigorate and focus research into the basic and translational science needed for new energy-conversion technologies.

Solar power is a case in point. Its great economic attraction is that, unlike nuclear power or carbon capture and storage, it does not need vast capital investment in order to spread. Its products just need to be priced in such a way that consumers and companies want to buy them. Once that point is reached, a solar-cell factory can produce the capacity to generate electricity as easily as a power station does, thus offering the possibility of exponential growth.

As we report on page 19, a boom in the solar-energy business, led by Japan and Germany, has now attracted serious interest from, among others, the technologists and venture capitalists of California's Silicon Valley. The people who brought the world Moore's law are eager to help it sustain itself through clean technology while accumulating yet more wealth on the way. Its most vigorous proponents suggest that attracting the attention of high-tech entrepreneurs could in itself be an end to our energy woes — a “distributed Manhattan project that attracts the smartest, most ideal people for the task”, as Bill Gross, serial entrepreneur and trustee of the California Institute of Technology, put it to *The New York Times* earlier this year.

But a healthy respect for the power of entrepreneurs and free markets cannot hide the fact that they do best when choosing between possibilities that are close to market, rather than inventing entirely new options. There is research into new materials and technologies that small start-up companies can't do, and that larger, more staid ones, if history is a guide, won't.

History may not be a guide, of course. But even the possibility that

the research may not get done is a reason for government to step in and ensure that it does, while trying not to crowd out private capital in the process. The current solar boom is dependent on old and trusted technologies — the companies now piling in are mostly finding new ways to manufacture and process familiar products. If the current rate of heady growth is to keep going for the quarter-century needed to start making a real change in the world's energy outlook, we will need new materials to capture the Sun's power ever more cheaply and easily, and new solutions to the problem of storing it.

Many scientists are eager to set out in search of those technologies. It is essential that curiosity-led research should flourish in these areas, and that funding bodies should encourage it so to do.

There are also areas where directed research might come into its own — where the best approach may be to try out lots of possibilities, rather than go with what a few bright people think is best. One of the strengths of the Manhattan project was that it tried out as many roads to nuclear weaponry as seemed plausible. Governments need to be willing to take advice on directed research and then make it happen, rather than just hoping that curiosity will triumph unaided.

Talk of a Manhattan project to tackle the generation and storage of 'clean' energy may seem overblown. It shouldn't. The challenge of increasing energy use in the developing world while at least stabilizing and ideally decreasing carbon dioxide emissions is immense. It is to the abiding shame of the world's governments that, as the threat to the climate has become ever more apparent over the past two decades, funding for energy research and development has actually fallen. To suggest that spending on energy research should be limited only by the capacity of scientists and technologists to make practical use of it is not to be profligate, but rational. ■

**“It is to the abiding shame of the world's governments that over the past two decades, funding for energy research has actually fallen.”**

Attachment B

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**CLIMATE CHANGE TECHNOLOGY EXPLORATORY  
RESEARCH (CCTER)**

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## ABSTRACT

Low cost avoidance of the risk of dangerous interference of greenhouse gases in the climate system will require much better energy provision and end use systems than are currently available. Therefore, we propose the establishment of an extension to the Administration's Climate Change Technology Program (CCTP) that would seek to identify and provide initial seed money funding for new research ideas that could lead to cost-effective technological breakthroughs of global significance. This research would generally be high-risk and often multidisciplinary. Seed money is needed to support the search for innovative climate change solutions, and its use has been found to be an effective strategy. We call this seed money based process *Climate Change Technology Exploratory Research* (CCTER). We offer this as a straw man suggestion for consideration by DOE and Congress. We suggest that one option for organizing CCTER is the setting up of a not-for-profit corporation funded by both the Federal Government through CCTP and the private sector. We estimate that the cost of CCTER to the government might be in the range of \$25 to 45 million per year after initial ramp up, about 1% of the current energy technology R&D budget. Since it is not known from where good ideas will come and climate change is a global problem, proposal solicitation should be very broad and include foreign investigators. All proposals would be submitted to peer review, assessment, and evaluation. Seed money R&D investments that show significant promise would be fed back to CCTP or the private sector for further maturation and development as required. CCTER should be evaluated periodically perhaps by the National Research Council.

### 1. Why is Exploratory Research so important and so needed?

Mitigating the rise of greenhouse gases in the atmosphere is generally understood to be an expensive proposition unless lower cost emission free energy systems can be invented, developed, and deployed. Our purpose for writing this short paper is to encourage discussion and stimulate debate about how best to find and generate new ideas for research that might lead to technology breakthroughs for mitigating climate change at lower cost. How might this be accomplished on a continuing basis?

Many promising technologies are being pursued by DOE, other agencies and the private sector under the auspices of the Climate Change Technology Program (CCTP). These include, for example, advanced nuclear power reactors, carbon capture and storage technologies leading to no net emission coal plants producing electricity, hydrogen or other low carbon fuels, lower cost solar and other renewable technologies, and cost effective high efficiency energy end-use systems all bolstered by a substantial investment in basic research. Similar research is in progress in other countries.

Despite this substantial effort, fossil fuels with concomitant atmospheric release of carbon dioxide are likely to remain the dominant energy sources for the world unless regulatory or tax forces are applied. Fossil fuels are generally least expensive, are widely available, are convenient to use, and they fit the existing infrastructure. No technology

silver bullets have yet been discovered that could change this fossil trend at low cost. The objective of this short paper is to suggest an approach for stimulating the search for silver bullets. This search is what we call “Exploratory Research.” It is a search for new ideas that, if successful, could make a big difference to the CCTP mission to stabilize the climate with continued economic growth. Exploratory Research is described in the draft CCTP Strategic Plan ( [www.climate-technology.gov](http://www.climate-technology.gov), p 9-13).

Several categories of Exploratory Research include: high-risk, long-term but potentially high-impact R&D; cross-cutting R&D that combine technologies and/or disciplines that may have exceptional systems value; novel concepts that may enable mitigating technologies or offset the impacts of rising levels of greenhouse gases; unconventional but mission oriented and potentially high-payoff basic research outside the normal disciplinary boundaries; and advanced decision support tools for better assessing the risks and impacts of Exploratory Research. Box 1.1 is a list of several examples of topics that might be good candidates for Exploratory Research. This list derives from the authors’ knowledge and experience, but the examples are unvetted and are merely meant to be suggestive.

Most of the categories mentioned above are being pursued to greater or weaker extent within the CCTP framework, but there is very limited flexibility in the system. There is no seed money to fund Exploratory Research on an open, competitive, and appropriately organized basis. Seed money is needed to nurture and stimulate thinking outside the box on a continuing basis. It is needed to support ideas that are out of the mainstream, but that could have a large impact even though the chances of success may be low.

We propose that a seed money approach to Exploratory Research be set up as a part of CCTP. We call this seed money activity Climate Change Technology Exploratory Research. Thus, CCTER is conceived as an important part of CCTP, but as discussed in Section 3, it need not necessarily be organized within DOE. This is a straw person suggestion that we hope will be useful to DOE and to Congress.

We believe this seed money flexibility is essential for the stimulation, care and feeding of new ideas. We note that many of the best, most productive ideas for research in the national lab system over the past few decades have come from Laboratory Directed R&D (LDRD). DOE and Congress allow the labs to use up to 6% of their funding each year for this purpose. This funding flexibility stimulates the generation of new ideas. We believe that seed money flexibility (with clear program goals and fiscal restraint) will have the same effect for CCTER.

### **Box 1.1 Examples of potential CCTER candidate areas.**

The ideas listed below are generally unvetted, and sources are not documented. The list is meant to be suggestive only. While many of CCTER ideas may never lead to a deployable system, the program would be a success if it enabled the development of just one “silver bullet” that could contribute greatly towards the mitigation of climate change. There is not a consensus by the authors on whether it would be better to consider adaptation technologies and strategies within CCTER or within a different program; support is needed for exploratory research into adaptation strategies, however. Careful delineation of the scope and function of CCTER will likely resolve this issue during its formation and funding.

➤ **System analysis and small scale development and testing of enabling technology for global-scale power transmission in low-resistivity power lines** could assess the benefits and costs of electricity wheeling between continents, time zones and day-night cycles. These grids could simultaneously address the problem of storage for solar and wind power and enable nuclear power reactors to be sited in secure environments with electricity dispatched worldwide. The development of high-temperature superconductor and/or carbon nanotube cables currently being pursued by DOE (as well as wireless power transmission) may make global electric grids feasible in the future.

➤ **Accomplishing low-cost carbon sequestration of agricultural residues in anoxic ocean environments could offset carbon emissions from efficient use of natural gas (including methane hydrates)** as a significant energy source in a greenhouse constrained world. Alternatively, biomass could be used to produce electricity while sequestering the resulting CO<sub>2</sub> to offset carbon emitted from fossil-fueled vehicles where the fossil fuel is made from coal with sequestration of carbon not incorporated in the fuel.

➤ **Use biomass (cellulosic waste or energy crops) to produce a char based fertilizer for sequestering carbon in soil.** Biomass is pyrolyzed to produce a porous char and producer gas. The producer gas is shifted to produce hydrogen for ammonia production and energy. The char can absorb CO<sub>2</sub> and NH<sub>3</sub> to produce ammonium bicarbonate resulting in a long release nitrogen fertilizer. The fertilizer production process can be used to scrub CO<sub>2</sub>, NO<sub>x</sub>, and SO<sub>2</sub> from flue gases. The net sequestration of carbon can offset the emissions from transportation, for example. The fertilizer can be used to improve the productivity of marginal land, and hence increase biomass productivity, and this can further contribute to the net extraction and sequestration of atmospheric carbon.

➤ **Hydrogen fuel might be manufactured from high-efficiency solar-thermal processes** as an alternative to PV- and wind- hydrogen from electrolytic decomposition of water. One technology for thermochemical hydrogen conversion of medium-grade heat to hydrogen employs a vanadium or iron redox cell and urea as an energy storage medium and transportation fuel.

### Box 1.1 (Continued)

**Tethered wind turbines flying at high-altitudes, deployed in the jet stream could harvest atmospheric kinetic energy more efficiently than ground wind machines.** The high energy per unit frontal area available at altitude may make this more cost-effective than low-intensity winds at the surface. The idea is to harvest as much of this concentrated wind source as possible without adverse environmental impacts.

**Engineering approaches may enable scavenging CO<sub>2</sub> directly from air.** Living plants capture carbon dioxide directly from air, but it may be possible to engineer systems that could remove CO<sub>2</sub> more efficiently or more rapidly.

**Artificial Photosynthesis involving extracting CO<sub>2</sub> from the atmosphere and reacting it with hydrogen from electrophotolysis (for example) might be used to make fuels for transportation.** The carbon recycling system would have no net carbon emission.

**Experiments and analysis are needed to evaluate the practicality of engineered aerosols injected to the stratosphere to scatter solar radiation back to space in amounts sufficient to counteract the radiative heating of CO<sub>2</sub> and other human greenhouse emissions.** Alternate geoengineering ideas are mirrors and lenses in space at the interior L1 Earth-sun Lagrangian point to deflect sunlight. These alternatives might be a sort of insurance policy that should be explored further in case its use becomes necessary.

**Develop methods to use biomass residues efficiently in the rural developing world e.g. by gasification to provide fuel for electricity, village heat and cooking.**

**Solar power satellites in geostationary orbit can beam power to PV collectors on Earth's surface** with high-efficiency diode lasers 24 hours a day 7 days a week thereby solving the storage problem of surface PV as a base load electrical source. This technology is enabled by recent breakthroughs in solid-state lasers with orbiting thin film PV arrays on low-mass inflatable-rigidizable structures.

**Power-plant flue gases could be used to dissolve limestone and the resulting solution could be placed in the ocean.** This approach has the potential to store carbon in the ocean while protecting marine biota from ocean acidification. A similar process is used by salt-water aquarists to promote the growth of corals in fish tanks.

**Low-mass car bodies from mass-produced carbon-fiber structures can enable very high fuel economies** for hybrids and (eventually) hydrogen vehicles. In addition, vehicles built from macro-scale carbon nanotubes with strength-to-weight ratios 200 times higher than steels could in principle have masses as low as a few kg with the same strength as today's car bodies -- perhaps enabling a safe 100 mpg car.

### **Box 1.1 (Continued)**

**Using fusion to breed fissionable reactor fuel** is an old idea that should be revisited because it could be important as a means to rapidly breed fissionable fuel & thereby vastly extend available fission reactor resources. Fissionable U-233 could potentially be bred from thorium in neutron-absorbing blankets, however this could pose a significant proliferation risk that would need to be mitigated perhaps by blending with U-238.

**Adaptation technology and strategies** ranging from mitigating the impacts of migration of whole ecosystems and associated animals including people to developing less expensive technologies to manage sea level rise, changes in precipitation patterns and increasing intensity of hurricanes represent a largely neglected but important area of R&D.

Also, the Office of Fossil Energy of DOE recently experimented successfully with a seed money approach to find novel new ideas in the area of carbon capture and storage. It used a committee of the National Research Council to help identify categories in which to search. The committee also helped design a solicitation and evaluate proposals. Some 109 proposals were received and 8 awards were made mostly for 3 year projects with a total cost of \$ 4.6 million. The process did uncover important new ideas to explore and it brought new people into the field. It is not clear whether this process will be repeated, but the NRC committee recommended that it should be.

The conclusion is that seed money used properly is an excellent strategy to employ to discover new important ideas.

## **2. What is the mission and character of the organization managing CCTER?**

The mission of CCTER is to seek, find, and provide initial funding for the best ideas. Proposals for research would be solicited very broadly including from foreign scientists and engineers. After all climate change is a global problem. This openness is essential because there is no way to predict the sources of the best ideas. CCTER should be an incubator for new ideas: a place for them to be tested rigorously for potential problems and showstoppers as well as for their potential to provide terawatts of energy impact on the global scale.

Ideas that pan out would be fed back into DOE-CCTP or the private sector or both for further maturation, development, and demonstration of economics, safety, and other benefits on a system-wide and global level. Feedback to CCTP and the private sector is a vital function of CCTER if it is to be fully successful.

CCTER should be funded partially by DOE and other federal agencies, of course, but it should also seek additional (perhaps matching) funding from private sector entities including businesses, foundations, and even individuals. The money from both sources should be managed seamlessly. Public and Private sector support should leverage each other. This global, long-term, social good issue requires a special government private sector partnership with a unique character. For example the constraints on the use of federal money to support foreign investigators or that make distinctions between the eligibility of some organizations should be relaxed.

We note that companies as well as foundations are beginning to invest in climate change mitigation research. Examples include the highly publicized Exxon Mobil investment (with other companies) in the Global Climate and Energy Program at Stanford University and the investment of Ford and BP in similar research at Princeton University.

Every proposal would be peer reviewed and scrutinized from the point of view of relevance to the mission and potential impact as well as technical merit. Intellectual property is handled to attract development, demonstration, and deployment funding if the R&D is fruitful. By managing intellectual property properly CCTER would seek to become a center for a network of investigators and entrepreneurs exchanging ideas and information actively and freely.

To avoid conflict of interest or diversion from the mission the CCTER staff should do very little research except as needed to secure and retain talented people (and this research should focus on system-level implications of funded or proposed projects). At any event, this in-house research should be a very small fraction of the total funds administered.

### **3. How might CCTER be organized?**

Several options for organizing CCTER might work adequately. The most obvious is to organize CCTER within DOE itself. We see several potential problems with this option. These include the difficulties of recruiting and retaining very talented and creative people to lead and operate CCTER, managing the melding of public and private money seamlessly, avoiding turf battles that may arise from the politics within DOE, and insulating the organization from confining bureaucratic policies and regulations. This option is not impossible, but it will be difficult. One variation on this option would be to organize CCTER within one of the DOE national laboratories. For example, DOE funded a program managed by the National Renewable Energy Laboratory to support the top ten incubators in the US for encouraging new energy solutions. That three years of funding resulted in significant innovations, businesses, and jobs. [http://www.nrel.gov/technologytransfer/entrepreneurs/pdfs/17\\_alliance\\_results.pdf](http://www.nrel.gov/technologytransfer/entrepreneurs/pdfs/17_alliance_results.pdf).

However, the DOE labs were designed to conduct research, not to act as program managers for research conducted elsewhere; the labs are generally multiprogramming, and we seek an organization dedicated to one and only one mission. Also, some of the same problems as for the DOE option remain, although perhaps moderated, but jealousy between labs is an added possibility.

Nevertheless, CCTER within the DOE family could be to climate change mitigation what DARPA is to the military.

A second option might be a special Federally Funded R&D Center (FFRDC) such as the Air Force's Aerospace Corporation. Such a corporation could be created to provide more flexibility and more insulation from the requirements imposed than if CCTER were organized in DOE. This option should be carefully considered. One possible variation on this theme is the NASA Institute for Advanced Concepts (NIAC). It was set up administratively outside of NASA for the purpose of functioning as an independent source of revolutionary aeronautical and space concepts that could dramatically influence how NASA develops and conducts its missions.

The third option is a private not-for-profit corporation. An example is RAND Corporation set up originally after World War II as a think tank for the DOD, but now does work for many agencies. The difference is that CCTER would be a corporation that funds R&D using both private and public sector funds. Several NSF centers operate this way, for example, the Aspen Center for Physics is a not for profit corporation funded by NSF and others. Under this third option, CCTER would have a board of directors with representatives from both DOE and the private sector sponsors. It could have considerable insulation from DOE politics and bureaucracy as well as from private sector pressures. It could be very flexible, and it should be able to attract top talent. For these reasons and because of the need to manage private and public sector resources productively, we conclude this is our preferred option. Taking maximum advantage of private sector intellectual contributions is a very important in-kind asset that a private not-for-profit corporation can generate more readily than other organizational options.

#### **4. How much government money is required?**

The answer to this question is a judgment call. We believe that CCTER should operate in the following manner. The first year it should solicit proposals from which the most promising would be selected for support. Obviously, some exploratory research may require more money for proof of concept than other ideas. By their nature, some may require several millions of dollars a year to test while others may require only a few hundred thousand. This is clear from an examination of the examples in Box 1.1. It may be useful to divide the funding so that some expensive projects can be examined each year. Of course, it is probable that most ideas that show promise after CCTER seed money funding will require more resources to fully demonstrate and initiate deployment. This maturation investment could come from either DOE or other CCTP agencies or the private sector, and one vital CCTER function would be to fully encourage needed follow on support.

We suggest, therefore, two categories of proposed research. Category 1 projects would include paper studies or small laboratory scale proof-of-concept experiments with annual costs typically in the range of \$100,000 to \$500,000 per project. Category 2 projects would test the engineering and cost potential for ideas that have already been vetted at the paper study or bench-top scale. Annual funding levels for these contracts might average

in the range of \$500,000 to \$1,000,000. In general, the Exploratory Research contracts would be for two or three years with extension possible but not common, although successful Category 1 projects could submit Category 2 proposals.

Assuming funding for 20 to 30 ideas per year with equal number of each category and 3 year funding, steady state expenditures for CCTER could be in the range of \$35 to \$50 million/y. To this must be added the costs of operation including organizing the peer review and evaluation process, and the cost of maintaining contacts with top talent and institutions around the world that may provide introductions to people with revolutionary new ideas and insights. These extra costs may be in the range of 10 to 20% of the contract awards. At steady state, the cost would be shared between the Federal government and private sector contributors. If it were on a 50/50 basis, the Federal cost would be in the range of \$19 to \$30 million per year. Conservatively we believe the order of \$25 to 45 million/y of Federal money is needed at steady state because it is likely that private sector support will be less than 50/50, at least initially.

Of course, the CCTER should start at a much lower level until the concept and procedures are fully worked out and tested. No doubt, there will be some growing pains.

We suggest starting at \$5 million per year for the first year, funding primarily Category 1 proposals, and ramping up from there to the steady state level in 5 years.

This Federal funding for CCTER is very small compared to the magnitude of the overall CCTP portfolio that is in the \$3 billion per year range, but we believe this small flexible seed money type of investment will have payback far in excess of the investment.

## **5. What process should be used to select projects for funding and how should CCTER be evaluated?**

Proposals would be solicited very broadly including from universities, commercial organizations, national laboratories, and even foreign organizations. Panels would be set up to evaluate the proposals, and these would include people from DOE and other agencies and from private sector donors as well as from the technical community at large.

The membership of the panels would be changed periodically.

Criteria for judging each proposal should include: 1) the potential impact of the proposed idea on climate change mitigation assuming realistic optimism for all relevant factors including cost, 2) the probability of success, 3) technical and scientific merit and risk, 4) the fully loaded project cost, and 5) potential confounding issues such as environmental impact, safety, infrastructure, and geography. The division of 1)\*2) by 4) might give a crude estimate of return on investment. The portfolio of investments could also be balanced in terms of probability of success to provide some long shots and some medium-shots. Votes on these criteria could be measured on a median-basis so a few naysayers or zealots on the panels will not skew the results too badly.

Progress by funded projects should be evaluated annually. We suggest Category 1 projects be evaluated by CCTER management. Category 2 projects should be evaluated by peer review. This way mid-course corrections or even cancellation can be invoked to avoid waste.

CCTER itself should be evaluated periodically to assure the mission is being pursued effectively, and to evaluate whether the investment is yielding adequate return. We suggest that this evaluation be done by the National Research Council (NRC) with a committee composed of people with different backgrounds with no direct conflicts of interest. The measure of success is the number of unique ideas that are judged to have potential for making a big difference if the cost is right. This NRC report would go to DOE, associated sister agencies, other sponsors, Congress, and the public.

## **6. How could CCTER be initiated?**

The first step is to generate enthusiasm for the idea of CCTER. It should be done within DOE, in the Congress and among the general public. The idea should be thoroughly vetted including in the private sector and academia. Assuming the vetting results are generally positive, a decision should be made between the three options of Section 3.

Assuming option 3 is chosen (or even option 2) a not-for-profit corporation should be set up. Money for this activity might be found from one or more foundations. We note that the formation of RAND was funded by a grant from the Ford Foundation. The corporation could then choose a CEO, appoint a board of directors and organize the solicitation for proposals. Simultaneously, work would go on with DOE CCTP, other agencies, OMB and Congress to propose, authorize and appropriate the first year of funding. With the arrival of funding, CCTER is operational.