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Mr. David W. Conover
Principal Deputy Assistant Secretary
U.S. Department of Energy
Office of Policy and International Affairs
1000 Independence Avenue, S.W.
Washington, DC 20585

Dear Mr. Conover:

Attached please find the Climate Policy Center's comments on the draft strategic plan of the US Climate Change Technology Program's (CCTP). The release of the draft strategic plan is an important step in the evolution of US climate policy. You and your colleagues deserve thanks for the hard work that has obviously been devoted to the draft plan's preparation and the review process.

Circulating the draft plan for outside review is, we believe, a wise and prudent policy. The challenge of developing an effective R&D policy for coping with climate change is daunting. The task of commenting on the draft strategic plan was itself a reminder that no organization has all the answers. In that spirit, it may be worthwhile for DOE to make the various comments public in the hope of encouraging further thought and exchange about these difficult issues.

To CPC, the three big areas where the draft plan needs further work are as follows:

1. CCPT should consider the risk that international GHG control regimes may remain narrow in coverage, relatively ineffectual, and slow to evolve. This possibility implies that CCTP should regard it as essential to develop technologies to neutralize harmful climate change and to lower its costs. It also reinforces the point that, while carbon capture and storage remains an important potential tool, CCTP should hedge its bets on this technology.
2. CCTP is in urgent need of organizational strengthening. In the short-run, launching a properly funded exploratory research program would ameliorate the problem. In the longer-term, creating a climate technology ARPA or a climate component of an ARPPA-E will almost certainly be an essential step.
3. The final version of the strategic plan needs to be more explicit about priorities and the hard choices ahead. The draft alludes to many of the correct principles for making strategic choices. It does not, however, explain the relative importance of these concepts or apply them in any visible way to the choices at hand.

CPC looks forward to working with you and your colleagues in DOE to explore these and other possible improvements to the current stage of work and thought. We fully share your assessment of the importance of this task. And we regard the further exchange of views as important and productive.

Sincerely,

L. Lee Lane
Executive Director

COMMENTS ON U.S. CLIMATE CHANGE TECHNOLOGY PROGRAM [DRAFT] STRATEGIC PLAN

By

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Overview

The release of the US government's new draft strategic plan for the Climate Change Technology Program is a major stride forward in the development of an effective policy for dealing with the challenge of climate change. To maximize the potential gains from this progress, the administration needs to treat the draft plan as a work in progress and continue to clarify and to extend the analysis and to modify the program in light of that analysis. Also, the Administration should urge other national governments to produce parallel strategic plans as a means of improving the transparency and comprehensiveness of international planning of climate-related R&D.

As it works to clarify and extend the existing analysis the most important single task will be to more firmly and explicitly anchor R&D planning in a comprehensive national climate change strategy. The large uncertainties inherent in the future evolution of both national and international climate policy carry especially important implications for technology policy. Specifically, it is possible that the international GHG control regime may prove to be weak, patchy, and slow to develop. The weaker such a regime proves to be, the greater will be the limits on carbon capture and storage (CCS) technologies. And the more important it will be to develop options for geoengineering and adaptation.

Clearly, CCTP needs a highly cost-effective organizational structure. In comparison with the difficulty of the task, climate-related R&D funding will remain limited. A new organizational structure that would strengthen CCTP's hand in dealing with the suppliers of R&D could help. Building better linkages between basic and applied research is particularly important. The draft strategic plan proposes creating such linkages through new programs to which it refers as 'exploratory research' and 'strategic research'. Launching such programs should be a high priority. Ultimately, establishing an ARPA-E organization with a substantial mandate on climate is probably a minimum requirement for effective organization. A stronger emphasis on organizational choices would improve the draft strategic plan.

Other improvements are also important in moving from the current inventory to a more truly strategic document. The draft strategic plan refers to strategic goals, core approaches, and individual technologies. Large differences in importance exist among the individual items covered in each of these categories. The draft strategic plan, however, does not discuss the benefits, costs, risks, limitations, and timeframe of these various elements. Nor does it provide a framework for assessing the present spending patterns within the categories with an optimum pattern. Still less does it actually apply such a framework or recommend improvements. Absent such a strategic analysis, it is hard to credit or justify the draft strategic plan's oft-repeated assertions, "Within constrained Federal resources, this portfolio addresses the highest priority current investment opportunities."

The CCTP draft strategic plan

To date, the recently released draft strategic plan for the Climate Change Technology Program (CCTP) is the Bush Administration's most significant contribution to climate policy. The Administration is correct in believing that developing new technologies is the highest priority challenge of climate change policy. Developing new drastically lower cost technologies is a *sin qua non* of long-term climate policy success. (Hoffert *et al* 2002 981)

GHG limits will engender some private sector R&D, but it will be too little and too incrementally oriented to produce the radical innovation needed for full climate solutions. (Popp 15; Edmonds and Stokes 163) Thus, whatever the future evolution of national and international climate policy, a government-funded R&D effort will represent a necessary policy element. CCTP is a start toward such a program. And the draft strategic plan is an important step in moving from a disparate array of R&D projects toward a coherent program.

The CCTP draft strategic plan document represents a creditable effort to summarize approximately $3 billion in annual Federal technology spending. As such, it (and the parallel work of the Climate Change Science Program) constitute an implicit rejoinder to those who assert that the US is 'doing nothing' to combat the threat of climate change. The scope and scale of these two programs is certainly not negligible.

The CCTP draft strategic plan lays out a hierarchy of vision, mission, strategic goals, core approaches, and a prioritization process. It offers a scenario analysis designed to illustrate some potential choices and the possible contributions that R&D might make to combating the harmful effects of climate change. A close reading of the draft strategic plan suggests that much valid and valuable thought has gone into the analysis.

As the plan indicates, the domestic CCTP is part of a complex of international agreements designed to foster progress on climate-related technology. One potential contribution of the draft strategic plan is to encourage other nations conducting R&D related to climate change to emulate the US and prepare and release their own strategic plans. An immediate action item for the US government should be to use the up-coming G-8 working meeting as a forum in which to urge other nations to prepare their own analogues to the strategic plan. (Lane 2-3) Their doing so would make national and international R&D planning more transparent and more coherent.

DOE in releasing the plan as a draft explicitly recognized that the planning of US climate-related R&D was an on-going and iterative process. (US CCTP [Draft] Strategic Plan iii) This point is important. And soliciting public comment should be merely the next step in this more extended process, as the draft strategic plan indicates will be the case.

Anchoring CCTP in a larger policy context

Additional work is required to make the draft strategic plan into a fully adequate basis for CCTP strategic planning. The central theme in CPC's comments is the need to more rigorously and explicitly anchor the CCTP strategic plan in a larger vision of climate policy. The ultimate goal of climate policy should be to minimize the expected value of the sum of the net damages of climate change and the costs of countermeasures against those costs.

Broadly considered, there are three approaches to achieving this cost minimization. They are:

4. Reduce GHG emissions.

5. Separate emissions from harmful climate change through what is called geoengineering, *e.g.* by producing increases in earth's albedo that off-set the warming effects of rising GHG concentrations.

6. Adapt to climate change in ways that reduce its net costs through developing heat and drought resistant crops, stockpiling genetic material from endangered species, or hydrological projects that minimize the costs of rising sea levels.

Some mix of these three strategies is most likely optimal, *i.e.* a mixed strategy is most likely to minimize the sum of the costs of climate change and the costs of countermeasures taken against it. For CCTP, the right goal is to develop the suite of technologies best able to implement this cost-minimizing strategy – given realistic expectations about how national and international climate policy are likely to evolve.

The question of the optimal suite of technologies cannot be separated from the question of what other climate policies are likely to be adopted, where they will be deployed and when they will become available. The current draft strategic plan ignores these issues. Nevertheless, considering them would yield valuable insights about R&D priorities.

* * *

Specifically, there appears to be a significant chance that an international GHG emission control regime will fail to emerge or not win the adherence of crucially important emission sources like China and India. Important grounds for this speculation are:

❖ Under current conditions, an international GHG control regime seems unable to enforce broad participation. It cannot gain acceptance from China, India, and the US. Russia is participating in name only. Without the participation of these countries, such a regime will be largely ineffectual. (Barrett 2004, 10,18; Schelling 2002, 3; Yang and Jacoby 1997, 4)

❖ Such geographically limited emission control regimes are downwardly unstable. Stringent GHG controls will undermine the international economic competitiveness of the nations that implement them. Simultaneously such controls encourage other countries to free ride. (Barrett and Stavins 350)

❖ If effective GHG limits are, indeed, absent, R&D can only modestly reduce GHG emissions. (Popp 20) Without controls, new emission-free technologies must compete against technologies that are not required to internalize the potential harm done by

> GHG emissions. Under these circumstances, deployment of the newer technologies may be unprofitable.

The US government can do little to brighten prospects for international GHG controls. Of course, no international regime can succeed without US participation. But although US participation in GHG controls may be a necessary condition of success, it is not a sufficient one. US adoption of GHG controls would not cause China and India to follow suite. For China and India mimicking rich countries' GHG limits would mean throwing away a competitive boost that they would otherwise reap. Moreover, adoption of GHG limits would harm China and India competitively vis-à-vis other LDCs. Thus, regardless of US policy, international GHG controls face a doubtful future.

* * *

This possibility sets the stage on which climate-related R&D must act. With weak controls, only revolutionary technologies capable of competing successfully without the aid of GHG controls, could substantially lower global GHG emissions. The first aim of climate-related technology policy should be to spark such a technological revolution. If the desired revolutionary technologies do not materialize, the fallback strategy for climate-related R&D is to find technologies that can minimize the risk of a continued escalation of GHG concentrations.

In light of these realities, the current CCTP exclusion of R&D on geoengineering and adaptation is imprudent. The draft strategic plan explicitly invites comment on this policy. (US CCTP [Draft] Strategic Plan 2-2 note 2) It is right to do so.

Whatever course future climate policy follows, significant increases in atmospheric concentration of GHGs are inevitable. The implications are uclear.

> … the uncertainties are daunting. The best the IPCC can do-apparently the best anyone can do-is to give us a range of possible warming for any given increase in carbon dioxide. And the upper bound of that estimated range has been, for over twenty-five years, three times the lower bound!-an enormous range of uncertainty.
>
> On top of that are the uncertainties of what the changes in temperature will do to climates around the world, what those climate changes may do to the worlds we live in, and what peoples in different climates can do to adapt successfully. (Schelling 2005 582)

Given these large uncertainties, climate policy must somehow cope with the prospect of low probability but (possibly) high cost events. (Nordhaus & Boyer 98) Should the climate system manifest a large and harmful discontinuity, a relatively fast-acting climate change 'circuit breaker' would be quite valuable. 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) At the same time, developing such a circuit breaker could be far more cost-beneficial than trying to rapidly halt GHG emissions based on the low probability that very harmful climate change may be lurking just around the corner.

Several experts have noted that geoengineering warrants much more attention than it is currently receiving:

> A radical technological option would be geoengineering, which involves large-scale engineering to offset the warming effect of greenhouse gases. Such options include injecting particles into the atmosphere to increase the backscattering of sunlight and stimulating absorption of carbon in the oceans. The most careful survey of this approach by the 1992 report of the U.S. National Academy concluded, "Perhaps one of the surprises of this analysis is the relatively low cost at which some of the geoengineering options might be implemented." (Nordhaus & Boyer 126)

After comparing the costs of Kyoto and various other climate policy options, Nordhaus and Boyer conclude that: "The difference between the geoengineering results and the results for the other policies is so dramatic that it suggests that geoengineering should be more carefully analyzed." (Nordhaus and Boyer 132) In other words, some geoengineering options could be much less expensive than mitigation strategies.

In addition to their possible cost advantages, geoengineering approaches would entail less difficult international negotiations than those required by emission limitation agreements. A geoengineering strategy would not require governments to negotiate to impose massive lifestyle changes on their populations. Instead, a geoengineering agreement would be about the sharing of monetary costs, a type of negotiation for which we have much experience. (Schelling 2005 592)

Of course, geoengineering options remain speculative. Some versions of the concept seem to stray into the realm of science fiction. And the more realistic technologies may prove to be ineffectual or to entail unwanted side affects. Then too, geoengineering is somewhat 'politically incorrect'. For now however, all that is required is to buy knowledge about cost, feasibility, and side effects.

By the same logic of hedging and total cost minimization, some R&D should be aimed at facilitating adaptation to climate change. There is a great deal of research that could be undertaken now, that would make adaptation to climate change easier.

> 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 43)

Adaptation can significantly reduce damages from climate change. More recent economic analyses of climate change damage are typically below those of earlier studies. And one important reason is that the more recent studies have taken better account of adaptation's ability to blunt the harm from climate change. (Joel B. Smith 31) There is no justification for neglecting R&D related to such a fruitful strategy.

* * *

The prospect that fossil fuels may not be required to internalize the costs of GHG emissions, boosts the potential importance of R&D on geoengineering and adaptation. At the same time, it raises doubts about the extent to which carbon capture and storage (CCS) will be economically viable. The draft strategic plan alludes to CCS' unique vulnerability to an absence of GHG emission limits. It notes:

> While some CCTP-supported advanced technologies may be sufficiently attractive, for a variety of reasons, to find their way into the marketplace at a large scale without supporting policy or incentives, others would not. Even with further technological progress, technologies that capture or sequester CO_2, for example, or others that afford certain climate change-related advantages, are expected to remain more expensive than competing technologies that do not. (US CCTP [Draft] Strategic Plan 2-11)

Given this political vulnerability, not even steady technological progress on CCS' technological limits and formidably high costs should induce CCTP to bet too heavily on this technology. In particular it is unlikely that CCS will be deployed in China and India where GHG controls seem a distant prospect.

The draft strategic plan correctly notes that the scale of promise of the various technologies should play a prominent role in setting CCTP's funding priority. But the potential of CCS may be limited as much by the policy environment as by the technology's cost and technology limitations. With rising doubt about the viability of global GHG controls, the technology mixes in scenarios 2 and 3 gain appeal relative to scenario 1.

CCTP should not exclude CCS. But CCS may be unable to penetrate markets beyond the OECD countries. If that conclusion is valid, then, for reasons entirely independent of the technological challenges, the promise of CCS may be more limited than some have hoped. CCTP spending priorities should be adjusted accordingly. Nuclear and renewables should gain resources relative to CCS.

Organizational reform of CCTP

Considering the larger climate policy context reinforces the point that R&D must score dramatic technological successes if it is to make significant inroads on climate change. This conclusion is especially pertinent if the above speculation about the limits of GHG controls proves to be well founded.

Despite the daunting nature of this challenge, CCTP's budget is likely to remain tight. Entitlements and national security needs continue squeezing domestic discretionary spending. The opportunity cost of R&D dollars, moreover, is high, "... The consensus from studies on the returns to R&D is that the social rates of return are approximately four times higher than the rates of return to other investments." Because society's resources for conducting R&D are limited, increased R&D spending in one area typically involves transferring resources from some other area. Between 1970 and 1980, roughly half of energy R&D spending occurred at the expense of other R&D programs. (Popp 17)

Thus, CCTP will face tough competition for R&D dollars, and the public has a right to expect high returns from dollars allocated to this program. To be able to earn these returns, CCTP will need a highly cost-effective organizational structure. The program's present organizational structure occasions questions on just this point.

* * *

Today, incentives of the constituent parts of CCTP are misaligned with the larger program goals. The misalignment occurs along four dimensions. Additionally, the US agreements on information sharing have not reached the crucial stage of bargaining about cost sharing.

1. Government must seek technological solutions to climate change wherever they exist. The CCTP's existing components, however, are technology-defined, *i.e.* they are oriented toward advancing specific technologies. Climate solutions lying outside of the existing technology stovepipes, or cross-cutting them may 'fall through the cracks'. The attached appendix on exploratory research contains a long list of concepts that appear to warrant consideration that they are not receiving, suggesting that this problem is real and potentially serious.

2. CCTP will find it difficult to keep spending patterns among its component parts focused on climate change. The program's various R&D components serve diverse goals and a disparate constituencies. The project managers and their patrons 'own' the component budgets. These proprietors' interests may only very approximately align with those of national climate policy. When the inevitable conflicts arise, CCTP can only cast its recommendations into the budgetary maelstrom and hope for the best.

3. Winning approval for enough strategic research and the right kind will be difficult under the existing organizational structure.[*] Strategic research is essential for CCTP's success. (US CCTP [Draft] Strategic Plan 9-6 – 9-7) Commendable as the recent cooperation between CCTP and the Office of Basic Energy Sciences (BES) is, BES should not be the final arbiter of the relative priority of strategic research directed toward climate change versus that directed toward other possible social needs. The

[*] 'Strategic research' is the draft strategic plan's term for basic scientific research into questions that if answered might help to solve important social problems. Dr. John Marburger, following the terminology of the late Donald Stokes, refers to research in Pasteur's Quadrant in a way that seems to correspond closely with the concept of 'strategic research'. (Marburger 1)

current organizational structure subordinates this partly political resource allocation decision to a technical and only partially accountable process.

4. Many of CCTP's component efforts are pursuing incremental, near-term agendas. The organizational culture of DOE, its energy industry constituencies, and the legislatively mandated requirements for industry partnerships virtually ensure this temporal myopia. But the short-run focus clashes with the long-run climate policy vision. Without structural change, the longer run orientation seems unlikely to receive the optimal degree of attention and funding.

5. Although the US has reached information sharing agreements on several climate-related technologies, it has not attempted to negotiate international R&D cost-sharing arrangements. R&D exhibits some of the global public good features that plague the climate change issue. Nevertheless, with R&D, the resulting free rider problem may be less acute. The benefits of successful climate-related R&D may confer large economic rewards on successful innovators. And R&D is likely to be inexpensive compared with imposing stringent GHG controls. A more favorable relationship of benefits to costs may permit more extensive international R&D cost sharing than is possible on emission limits.

The current interdepartmental committee management model cannot substantially enhance CCTP cost-effectiveness unless it is supplemented with more fundamental institutional reform. If incentives remained misaligned between the program and its component parts, the CCTP management committee will remain mired forever in bureaucratic trench warfare. Compared to the Working Group or the Committee, the various project managers have superior 'local knowledge'. And they may have constituent and congressional support. This process does not represent a plausible basis for managing an R&D process that is aiming to transform the technological basis of one-seventh of the global economy.

* * *

<u>The only real solution is to create an authorized budget for CCTP with appropriated funds of its own</u>. The various R&D providers within government and beyond it would, then, compete for these funds. Instead of a committee trying to override the baleful effects of misaligned incentives, this approach would create a quasi-market within government. Such a quasi-market would offer new incentives for CCTP's current components to behave more consistently with the larger goals of climate policy. And where they are incapable of doing so, CCTP would be able to circumvent their limitations.

The CCTP draft strategic plan proposes at least one new program in the spirit intended here, the proposed 'exploratory research' program. (US CCTP [Draft] Strategic Plan 2-7) <u>This proposal is the single most important innovation in the draft strategic plan.</u> It deserves immediate and positive action. However the exploratory research proposal is merely one 'fix' needed to address the incentive problems listed above. The complete list would be:

6. The proposed exploratory research program would compensate for the obvious technology stove pipe flaw in today's organizational structure. It would do so by authorizing CCTP to look for innovative solutions without regard to the particular that they employed. And it would give the program a small amount of seed money to develop promising technologies that it discovered. Those that continued to show promise after further exploration would then be taken up by other larger programs.

7. CCTP also needs an discretionary account for influencing resource allocation among the various technology-defined projects that are its current 'meat and potatoes'. A small discretionary account that could be allocated among CCTP's constituent parts would allow money to flow to more promising efforts and would establish CCTP as a customer of the internal R&D providers.

8. The draft strategic plan should also propose an independent CCTP budget for 'strategic research'. (US CCTP [Draft] Strategic Plan 9-1) Instead, the political process should establish a budget for climate-related strategic research and allow CCTP to acquire the research it wants from whom it wishes. This arrangement would provide clearer accountability than does the current situation.

9. Eventually, however, a more fundamental reform is necessary. The recent NAS panel recommendation of an ARPA-E patterned after DARPA offers a promising model. To understand why organizational change is so important consider the disadvantages of the current arrangements. Effectively, CCTP should become one component of an ARPA-E. That component should have budget authority to fund exploratory research, push further development of concepts discovered by exploratory research, supplement the budgets of climate relevant research within DOE's technology defined program, and encourage strategic research relevant to climate change. (NAS ES-7)

10. The US should seek to reach R&D cost sharing agreements with other countries with effective R&D programs. For any one country, making an increase in R&D spending contingent on a comparable contribution from others increases the potential benefits likely to flow from the additional investment. By increasing the potential payoffs to each participant, such agreements offer a way of increasing the total pool of R&D dollars available for the search for climate solutions. And (Barrett 2004 13)

Improving the planning process

The first part of these comments concentrated on possible improvements in the mix or R&D conducted under CCTP. The second part discussed needed changes in CCTP's organizational structure. The following third and final set of suggestions will make proposals for improving CCTP's strategic planning process.

It will advance three proposals. These are:

11. The plan would be more useful if it explicitly considered synergies and tensions between CCTP and the broader issues of national technology policy.

12. The CCTP planning process should explicitly analyze some of the key dilemmas and controversies surrounding climate-related R&D policy.

13. The draft strategic plan needs to become far more explicit about the logic that underlies its conclusions and its decision making.

* * *

The draft strategic plan lists "provide supporting technology policy" as a "core approach". (US CCTP [Draft] Strategic Plan 2-10 – 2-11) The subsequent discussion leaves the concrete implications of this approach more than a little murky. The basic point, though, is valid. Government-funded R&D is only part of Federal climate-related technology policy.

Thus the recent energy legislation authorized a number of new policies creating subsidies based in part on the putative climate-related benefits of specific technologies. How cost-effective are these resources commitments. In particular how do they compare with increased funding within CCTP? A true national strategic plan for climate-related R&D would address these questions.

Conversely, wider technology policies may either advance or impede progress in the area of climate-related R&D. For example, Congress requires that many DOE R&D activities obtain industry partnership and matching funds. These matching requirements can represent a considerable barrier to some of higher risk, longer run, but potentially high payoff R&D. Many experts think that high-risk high-payoff projects are the key to climate technology success.

Could partnership requirements constitute a barrier to climate policy success? Analysis on this subject would be useful. Such analysis would appear to belong in a CCTP strategic plan.

* * *

In a sense, the question about the implication of business partnership requirements exemplifies a larger controversy. Some experts have advocated a climate-related R&D strategy that emphasizes making incremental changes to existing (or nearly existing) technologies. Others insist that the inadequacies of near-term technologies are so intractable that a more far-sighted visionary technology strategy is a better bet. This controversy has unmistakable implications for CCTP's priorities.

More rigorous analysis could do much to clarify the issues. In particular the likely prospects for learning-by-doing – and its limitations – is critically important in assessing the uncertainties fueling this controversy. Recent economic research has done much to illuminate the relevant questions. Some of this analysis casts doubt on certain key assumptions about the extremely expansive view of learning-by-doing embraced by proponents of the more near-term incrementalist approach.

The point here is not to resolve the issue. It is that assumptions about the trade-offs between long-run and short-run goals and the importance and limits of learning-by-doing have profound implications for climate-related R&D policy. Similar controversies exist about claims of major market imperfections in the adoption of energy saving technology, and probably other issues as well.

The CCTP strategic plan cannot resolve these controversies. But the controversies are highly relevant to assessing the plan's judgments. The best available solution would be for the strategic plan to make its assumptions about these controversies explicit. And it should explain the basis for its judgments. At some later point in the planning process, sensitivity analysis might help to identify the importance of the controversies.

* * *

The above recommendation aims to make the judgments behind the strategic plan more transparent. In fact, much of the draft strategic plan's logic is opaque. For example, how much money is currently being spent by technology, by strategic goal, and by core approach? The draft strategic plan gives no hint. Perhaps some readers have the knowledge to use the budget lines in the appendix to make guesses about the current allocation of resources. Most do not.

Similarly, what weights does DOE give to the four investment criteria? (US CCTP [Draft] Strategic Plan Box 2-1) The draft strategic plan does not say. In truth, the criteria themselves and the way that they inter-relate is not entirely clear. The initial goal, maximizing return on investment, is not so much a separate criterion as a summary of all the others. Of course, measuring return when the benefits must include reduction of climate change damages is no easy matter.

Some of the criteria involve a degree of internal tension. The Third criterion, for example, calls for an emphasis on technologies with large scale potential. The idea is certainly valid and relevant. But the discussion, then, seems to make an exception for technologies offering near-term benefits. If near-term benefits are defined to include GHG emission reductions, this exception is dubious. Because atmospheric GHG concentrations are cumulative, early emission reductions, unless they are especially cheap, are likely to be unimportant and may even be undesirable. This criterion needs to be stated more clearly and consistently.

Putting aside these ambiguities, knowing how the technologies rank according to the listed criteria, how can the reader assess the appropriateness of the current spending? The draft strategic plan repeatedly observes that current spending patterns are appropriate, but unless DOE has actually rated the technologies according to its criteria, how can it make this judgment. If it has done this rating, it should incorporate this analysis in the plan.

APPENDIX A

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. Ideas 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.climatetechnology.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_2 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.

Box 1.1 (Continued)

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_2 and NH_3 to produce ammonium bicarbonate resulting in a long release nitrogen fertilizer. The fertilizer production process can be used to scrub CO_2, NO_x, and SO_2 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.

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_2 directly from air. Living plants capture carbon dioxide directly from air, but it may be possible to engineer systems that could remove CO_2 more efficiently or more rapidly.

Artificial Photosynthesis involving extracting CO_2 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_2 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.

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

Box 1.1 (Continued)

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.

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. The International

Thermonuclear Experimental Reactor (ITER) -- a deuterium-tritium Tokamak fusion reactor experiment to be constructed in Caderache, France -- can, in principle, be employed for a US-sponsored experiment to breed fissionable U-233 from thorium in neutron-absorbing blankets.

Adaptation technology and strategies ranging from mitigating the impacts of migration of whole ecosystems and associated animals and people to developing less expensive technologies to manage sea level rise, changes in precipitation patterns and increasing intensity of hurricanes represents 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. 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.

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