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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. 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 he 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.

- **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.

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_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.**

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. 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.