Written Public Comments on the *Strategic Plan for the U.S. Climate Change Science Program* General Comments

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AMERICAN ASSOCIATION OF STATE CLIMATOLOGISTS (AASC)

The American Association of State Climatologists (AASC) is a professional scientific and service organization composed of state climatologists (one per state), representatives of the six Regional Climate Centers, and associate members who are persons interested in the goals and activities of the Association. State Climatologists are individuals who have been identified by a state entity as the state's climatologists and who are also recognized by the Director of the National Climate Data Center of the National Oceanic and Atmospheric Administration as the state climatologist of a particular state.

These comments provide the perspective of the AASC on the Strategic Plan for the Climate Change Science Program. Since the AASC members work directly with users of climate information at the local, state, and regional levels, the AASC is uniquely able to place climate issues into the local perspective needed by the users of climate information. These comments were voted on and approved by the AASC.

Our perspective, based in part on the 2001 AASC Policy Statement on Climate Variability and Change (http://lwf.ncdc.noaa.gov/oa/climate/aasc.html), are summarized as follows:

- Climate prediction is difficult because it involves complex, nonlinear interactions among all components of the earth's environmental system. These components include the oceans, land, lakes, and continental ice sheets, and involve physical, biological, and chemical processes. The complicated feedbacks and forcings within the climate system are the reasons for the difficulty in accurately predicting the future climate.

- Climate prediction is complex with many uncertainties, and the AASC recognizes climate prediction is an extremely difficult undertaking. For time scales of a decade or more, understanding the empirical accuracy of such predictions – called "verification" – is simply impossible, since we have to wait a decade or longer to assess the accuracy of the forecasts.

- Human activities have an influence on the climate system. Such activities, however, are not limited to greenhouse gas forcing and include changing land cover and aerosol emissions, which further complicated the issue of climate prediction. Furthermore, climate predictions associated with human disturbance of the climate system have not demonstrated skill in projecting future variability and changes in such important climate conditions as growing season, drought, flood-producing rainfall, heat waves, tropical cyclones and winter storms. These types of events have a more significant impact on the United States than annual global temperature trends.

- General circulation models which have been applied to project changes in global and regional climate for periods of decades into the future need to be viewed as hypotheses about the behavior of the atmosphere in response to human disturbance. The validity of such models is uncertain because our understanding of all relevant climate factors (and their relationships and interactions) is incomplete. New research should be based only upon hypotheses that can be verified by observed data. This underscores the need to continue (and, in fact, enhance) the long-term climate monitoring system in the United States so that, for example, climate models can be properly tested.

Our recommendations for the Strategic Plan are as follows:

- Research on long-term climate should not be based on specific projections, but instead focus on policy alternatives that make sense for the range of plausible regional and local climate variations.

- By focusing on society's vulnerabilities to climate change rather than on climate projections, the scientific community can provide more comprehensive and useful information to local, state, and national decision makers. A lack of an ability to generate accurate projections should not be used as a justification to ignore the policy challenges presented by climate variability and change. Research must be directed to better identify and quantify these vulnerabilities.

- The use of historical scenarios such as the 1930s Dust Bowl years, or more recently the 1988 and 2001 droughts and the 1993 flood, can improve climate scenario development. Not only are these physically plausible scenarios, they provide the opportunity to examine how society and the environment actually responded. Research should be completed to assess how society would respond today to these climate events.

- State and regional climatologists can provide analysis tools and climate data, some of it unique (e.g. soil moisture or mesonet observations) in the context of the assessment of the vulnerability of local and regional areas to climate variability and change.

- More emphasis should be placed on two-way communications with stakeholders as part of the research process. By involving stakeholders with varied and competing interests early in the process, climate researchers can focus on the important climate parameters, and stakeholders will understand the limits of the information provided to them. Since the impacts of climate variability and change vary widely across regions within the United States, state and regional climatologists, and other local experts, who are most familiar with the stakeholders and the potential impacts at this scale should be involved in the research.

- Peer review judgment from a handful of experts should not be the final test following release of climate projection publications. Independent climate groups and organizations such as the AASC should be provided an opportunity to periodically evaluate the accomplishments of the US Climate Change Science Program.

- Financial resources should focus on the assessment of local and regional vulnerabilities and possible responses rather than the generation of projections of future climate from general circulation and regional numerical forecast models.

Finally, as an overarching goal, the AASC recommends we concentrate on reducing our vulnerability to paleo, historical and current weather extremes, for this would allow us to better protect ourselves from problems associated with the spectrum of future weather extremes.

ANDERSON, HARVARD UNIVERSITY

It is important to set in place a foundation to transform the national climate initiative from its current structure to one that is viable for the long term. To accomplish this it is necessary that we focus not just on what scientists want, but on what society needs, and that we place climate requirements front and center rather than as a rider on other programs.

It is clear that several important themes emerged from the Workshop December 3-5. From what I observed, a subset of these important themes included:

*Theme #1:*The necessity that disciplined language that speaks with focus, specificity and imperative be used in this report. It has been demonstrated over the past two decades that generalizations are deadly and they lead to weak programs and a breakdown between objectives and resources. Yet the draft Strategic Plan is largely descriptive.

Theme #2: The imperative that the national climate program be based, with far greater clarity, on societal objectives, for only those objectives will sustain the program over the long term. A focus on societal objectives leads to a cascade of fundamental changes in the structure of the national effort. Societal objectives unequivocally call for an operational climate forecast that is tested and trusted. The operational climate forecast in turn requires operational climate models and an observational strategy designed to test the veracity of those models. This constitutes a conceptual framework that sets disciplined and coherent priorities. It sets responsibility for the forecast squarely on the operational climate models and it demands very specific observations that, rather than satisfying the intent to "gather climate information," directly test the forecast ability of the models. The structure of the climate models will be transformed as the focused responsibility for societal objectives emerge to define demands placed on the operational climate models. Generic lists of measured quantities will give way to specific, coupled combinations of high priority land-based, oceanic and space based observations with tight tolerances on accuracy, sampling protocols, data availability and costs.

General Comments

Theme #3: The importance of the scientific community taking responsibility for coherently defining the specifics that will constitute a vital, agile and effective climate program. While the workshop *process* invites specific input, the draft document is cast in such general language that any call for specific action is muted. The success of the CCSP will depend upon how effectively the community places in writing what is specifically required. A fundamental breakdown in the USGCRP strategy over the past decade occurred as a result of generalities endemic in the agency program descriptions coupled with generalities intrinsic to the annual OSTP report. As a result, communications broke down as these documents passed in the night. Detailed input was blunted to the point where there was little effective engagement. The draft CCSP Strategic Plan is certainly long enough to include critical initiatives, but the required specifics are not there. A clear example is in the carbon cycle sections that occupy parts or all of chapters 2, 5, and 9. In concise language, what is required in this key phase of the program? The North American Carbon Program - NACP (Wofsy and Harriss, 2002) that contains (a) the DETAILED specification of what must be measured, (b) what the required accuracies are, (c) what the required spatial and temporal resolutions are, constitutes the crucial transition from *description* to professional *direction* that can instigate change. For example in the NACP document it is stated:

Highest priority enabling developments for the NACP (page iv)

1. Develop in situ sensors and sampling protocols *for aircraft, ocean, tower, and soil and vegetation flux measurements of CO_2, CO and CH_4: robust, accurate, and operable by minimally-trained personnel. The instruments represent near-term deliverables of the Program.*

2. Model studies of network design and model-data fusion *require a summer study institute, then sustained efforts to develop data assimilation/fusion systems by intensive evaluation of models against new data, e.g. flux and isotopic measurements. A science team of ~5 groups outside operational centers and funding for DAO, GFDL and/or NCEP (10-20 FTEs), plus computer hardware is envisioned. A 20-yr re-analysis of global meteorology, with a 10-km nested grid over North America should be initiated in Phase 1 at one or more operational Centers. This activity, will deliver a crucial product with data tailored for mass budget analyses, for hindcasting and for refining network concepts using pre-existing data.*

3. Optimize national inventories (FIA and NRI) for carbon accounting*. Strategic enhancements to current network designs are needed for complete carbon accounting. Historical data on land cover, management, and disturbance need to be compiled and made available, and gaps filled by statistical estimation. Benefits will include more consistent and comprehensive historical data covering land ecosystems, past human impacts, and natural disturbances. Gaps in geographic and biome coverage should be filled, especially rangelands, mountainous areas, and developed lands.*

4. Strengthen current observation networks. (a) Fill gaps and weaknesses in the current long-term measurements of target gases in the US. *The NOAA-CMDL greenhouse gas programs require sustained long-term ("baseline") funding above current levels. These steps are extremely urgent to allow an expanded NACP program over the following 5 years, or even to maintain status quo. Time-series ocean moorings are needed for atmospheric CO_2 and marine pCO_2. Better tracking of atmospheric trends are a near-term deliverable.*

General Comments

4(b) Begin the transformation of the AmeriFlux network into an integrated, nearreal time network, and add representative long-term sites. *The core AmeriFlux program needs strengthening, with enhanced quality controls, oversight, and improved information management systems. New sites in critical under-sampled ecosystems, and projects to understand fluxes in complex terrain, are needed.*

5. Improved databases for fossil fuel use and land use/land cover. *A "State of the Carbon Cycle for North America" report will be prepared that addresses current knowledge and new advances in understanding of all components of the natural and managed carbon cycle. This assessment will be updated as significant new findings are produced. Initially, the focus will be on establishing a consistent inventory of sources/sinks of* CO_2 *and* CH_4 *with associated uncertainties for each source/sink category. Methods to better integrate historical land use and inventory information with contemporary satellite estimates of land cover and use will be a crucial aspect of improving the quality of source/sink estimates. The improved databases and Carbon Cycle Report will provide continuous policy-relevant information to inform policy.*

6. Remote sensing technology development *is needed for atmospheric* CO_2*,* CH_4 *and CO, and for above-ground biomass and soil moisture. Satellite data will be the key to long-term, accurate NACP data in the 5-10 year time frame. Near-term efforts should focus on airborne simulators of future spacecraft instruments, and on critical assessment of early products for atmospheric* CO_2 *from existing satellites. The airborne simulators will be used to measure in situ profiles/columns of the gases. Airborne simulators are also crucial to technology development for biomass and soil moisture satellite sensors, and will be extremely useful for intensive studies before spacecraft are deployed. Technology development is the near-term deliverables.*

In the second phase of the NACP there is a specification for:

- Fundamentally new observational strategies that emphasize high spatial resolution measurements of isotopes, fluxes, and vertical profiles of CO_2, CO, CH_4, and H_2O.
- Measurements of sources and sinks for CO_2, CH_4, and CO for North America at scales from continental (5000km) to local (10km) with seasonal resolution.
- Two complementary components to the sampling program: (1) A long-term network of atmospheric observations, and (2) Intensive aircraft campaigns
- Accuracy of the primary observables are CO_2 (+/- 0.2ppm), CO (+/- 1 ppb), CH_4 (+/- 5ppb), and H_2O (+/- 5%)
- Etc.

Thus, as a result of the series of workshops (attended by and contributed to by individuals (1) directly involved in carbon source/sink research, and (2) focused on a fundamental reconstitution of the US carbon program) disciplined priorities emerged.

Theme #4: There was a surprising lack of sophistication in the description of specific observations needed to underpin the required U.S. climate observing system. It is just this lack of in-depth analysis that blurs the critical distinction between, for example, climate data requirements and numerical weather forecast initialization requirements. While there is no doubt that we should make every effort to be smart about how we utilize available data sets from other systems for climate research, it is important to recognize that the devil is in the details for a climate program that can deliver accurate long-term data sets and can decisively test high-end forecast models, and we don't have a national program with the substance to deliver on these. Consider, for example, observations of spectrally resolved radiance, accurate to 0.1K, emitted throughout the thermal infrared from Earth to space that constitutes a fundamental observation for both benchmarking long term climate change based on the accurate determination of first moment statistics, and sophisticated testing of climate forecast models using second moment statistics, optimal filtering, linear inverse modeling, adjoint modeling, etc. In critical elements of an effective climate program, a careful analysis demonstrates that for reasons fundamental to the physics of precision metrology, optical design, detector selection, orbit choice, viewing geometry, etc. an optimal design for initialization of numerical weather forecasts conflicts in a serious way with the design appropriate for climate data requirements. Weather forecast initialization demands high horizontal *and* vertical spatial resolution, high spectral resolution, full geographic coverage coupled with a small viewing footprint that necessitates cross-track scanning, high sensitivity (and thus non-linear) detectors, high area-solid angle product spectrometer design, set equator crossing times, etc. These objectives can only be met with technologically sophisticated, complex and expensive instrumentation. These design objectives represent a triumph for weather forecasting, but a critical compromise for climate measurements that demand moderate spectral resolution ($1cm^{-1}$) with accuracy (0.1K) that can be proven in perpetuity to the critic. The requirement of accuracy in turn demands linear detectors, multiple on-orbit blackbodies, deep space view with repetitive angular scans to pin polarization, thermal control of optical field stops, independent tests of detector chain linearity, etc, and an orbit choice (e.g. 90° polar) that systematically scans the diurnal variation in order to accurately determine climate averages. These climate requirements demand simplicity of hardware, redundant cross checks, placement of accuracy as the top priority, and an open discussion of systematic errors within the scientific community to pin the climate record for future generations. Repeatedly it was pointed out at the workshop that weather requirements are *not* climate requirements.

Another example of vague language is the treatment of ocean heat uptake and ocean circulation that is of paramount importance. There are important examples in this field wherein the long, un-prioritized laundry lists of ocean observations, generated by a manifold of international committees prior to TOGA, were replaced by clear prioritization for the ENSO seasonal-to-interannual forecast:

- First Priority: Sea Surface Temperature and Winds
- Second Priority: Upper Ocean Thermal Structure
- Third Priority: Sea Level
- Fourth Priority: Sea Level Pressure, etc.

Obviously there is a distinction between observations that best serve short-term climate forecasts and observations that best serve long-term climate forecasts, but that distinction can be clearly made in the prioritization.

General Comments

The Climate Change Science Workshop that Jim Mahoney initiated suggests a fundamental change in the national strategy. In particular, rather than following a process that has, for the past decade, generated a increasing number of reports emanating from the NRC, USGCRP, and the agencies that have proven ineffective for the establishment of an successful, aggressive, prioritized U.S. climate program, that a workshop structure be set in place that seriously addresses innovation, details and priorities in a number of critical areas. Since the CCRI was launched by the President to reduce significant uncertainties, provide a distinct focus on near-term objectives, and strengthen the link to societal objectives, there is an opportunity to put in place a follow-on workshop structure focused on key climate change uncertainties. These focused workshops would address critical areas under the mandate of the head of the CCRI. Examples include: the structure and testing of a U.S. operational climate forecast, the specifics of carbon cycle observations and modeling, the specifics of ocean and air-sea observation networks, strategies to address observations of biological and ecological response to climate variability, etc. The clear directive to these workshops (that would meet separately and engage individuals currently working actively in these areas) would be the setting of specific priorities and the recommendation of a clear strategy for execution to the head of CCRI. It would be important to include a strong voice for societal/stakeholder objectives in the process and to keep the workshops reasonably limited in size. While there is clearly a need for curiosity driven science in the national effort, that element is in the USGCRP component. What the U.S. climate effort needs now is discipline, priorities and specifics delivered to society.

ANTHES, UCAR

Overview Comment on Document: There are many strong points in the document, including an overview of the issues, recognition of the importance of the climate change problem, the need for much more research on a variety of topics while at the same time increasing the value of climate research and observations for policy makers. The draft plan provides general guidelines for future research and priorities. However the draft is short on specifics. If the goal is to make a meaningful acceleration of research progress that will be more useful to policy makers in 2-4 years, a few specific areas of increased investment and planning need to be identified. I am suggesting two areas: (1) significantly enhanced computer capability to support the two-center climate modeling strategy and associated distributed regional climate modeling efforts around the country and (2) development of real and robust plans to make radio occultations (RO) a permanent part of a global climate observing system. These suggestions support mainly Chapter 12 *Grand Challenges in Modeling, Observations and Information Systems*, and are consistent with the statement in that chapter "These activities [modeling, observations and information systems] are the highest priority for the CCSP."

The first recommendation on need for increased computer power needs little elaboration or further justification beyond the several recent NRC reports on this subject. One of the most commonly heard pleas from policy makers at the December workshop was the need for information and scenarios of climate change on the regional scale. Modeling climate on the regional scale and doing many ensemble runs to generate probability distribution functions of climate variable such as temperature, wind and precipitation requires much more computing power than is currently available to the U.S.—and in fact, even the world. One estimate at the workshop from one of the European centers was that more than a million times the current computing capability could be used effectively by the climate modeling community today. However, a million-fold increase is not required all at once to accelerate progress, and increase of a hundred fold would make a large and positive difference and this seems to be possible in the next few years with a modest additional investment.

The second recommendation is to develop firm and robust plans to make radio occultations (RO) a major part of a permanent global climate observing system. Many NRC reports, as referenced in the draft plan and publications (most recently, Goody *et al*., *Why Monitor the Climate*, **BAMS** June 2002, 873-878 and Trenberth, *The Need for a Systems Approach to Climate Observations*, **BAMS**, November 2002, 1593-1602) have stressed the need for such a system, yet the response by the U.S. has been slow. Now there is an opportunity for the U.S. to take the leadership in establishing a relatively inexpensive component, or backbone, of such a system. As demonstrated by the GPS/MET, CHAMP and SAC-C experiments, radio occultations provide independent observations of the Earth's atmosphere that have many advantages over other satellite and in-situ systems. The advantages, as demonstrated in theory and with real data, include high accuracy, high vertical resolution, requires no first guess sounding, all-weather (not affected by clouds and precipitation), requires no first –guess sounding, independent of radiosondes or other observing systems, no calibration required, no instrument drift, no satellite-to-satellite bias and relatively inexpensive. Radio occultation soundings would complement and strengthen radiometric sounders on other satellites, increasing their value. In the upper troposphere and lower stratosphere, the RO refractivities can be converted to highly accurate temperature soundings, to resolve the temperature and height of the tropopause for example. With ancillary data, the refractivity profiles can be converted to temperature and water vapor in the lower troposphere. However, vertical profiles of refractivity by themselves can also be used as sensitive measurements of climate variability and change. There are now many peer-reviewed references to support these claims; see for example:

Lee, L.-C., C. Rocken and E. R. Kursinski, 2000: Special issue of TAO (Terrestrial, Atmospheric and Oceanic Sciences), Vol. 1, Number 1, March 2000, 380 pp.

Goody, R., J. Anderson and G. North, 1998: Testing Climate Models: An approach. Bulletin of the American Meteorological Society, 79, 2541-2549.

Wickert, J., G. Beyerle, G. A. Hajj, V Schwieger and Ch. Reigber, 2002: GPS radio occultation with CHAMP: Atmospheric profiling using the space-based single difference technique. *Geophy. Res. Lett*., **28**, 3263-3266.

In addition to their potential strong contributions to a global climate observing system, the RO soundings will be very useful in numerical weather prediction and space weather research and prediction.

With CHAMP and SAC-C expected to provide RO soundings of the Earth through 2005 when COSMIC (Constellation Observing System for Meteorology, Ionosphere and Climate) will be launched, there are already plans in place to have continuous RO soundings through at least 2007, and longer if COSMIC's two-year mission is extended. Plans should begin now to make sure analyses of these missions for climate purposes is carried out, that all of the missions are continued as long as they are providing high-quality data, and that an operational satellite system to continue RO observations indefinitely into the future are developed.

OFFICE OF ATTORNEY GENERAL FOR MASSACHUSETTS, CONNECTICUT, MAIN, AND NEW YORK

We appreciate the opportunity to comment on the draft *Strategic Plan for the Climate Change Science Program*, dated November 11, 2002, ("Strategic Plan"). This matter bears directly on the health and welfare of future generations, as well as on ecosystems and economies throughout our States and, indeed, throughout the world. Its importance to "every corner of the world" simply cannot be overstated. Therefore, we submit the following overview comments for your consideration. We leave evaluation of the specific scientific proposals to scientists and researchers and focus the following overview comments on the *Strategic Plan* as a whole.

The rationale underlying the *Strategic Plan* is that "at this point, modeled projections of the future regional impacts of global climate change are often contradictory and are not sufficiently reliable tools for planning." *Strategic Plan* at 7. The *Strategic Plan* outlines a 10 to 15 year research program largely aimed at closing that gap and producing "policy-relevant deliverables within a short timeframe [two to four years]." *Strategic Plan* at 15. Purportedly, after a few years of focused research and data collection and refinement of computer models, the *Strategic Plan*'s research program will provide decision-makers with now-missing tools and information they need to set substantive climate change policy.

While the research proposed by the *Strategic Plan* will likely yield valuable information, its focus and timetable raise some serious concerns. Specifically, and as discussed further below, the *Strategic Plan* unduly emphasizes research efforts geared towards adaptation policies and fails to address adequately the immediate need for mitigation policies that would seek to stabilize atmospheric concentrations of carbon dioxide. To the extent it will result in a delay in implementation of mitigation measures, we will be faced with far worse environmental and economic consequences. Therefore, the *Strategic Plan* should clarify that such a result must be avoided and that nothing in the program it establishes prevents simultaneous implementation of mitigation measures.

The *Strategic Plan* Emphasizes Research Efforts Geared Towards Adaptation Policies and Fails to Address Adequately the Immediate Need for Mitigation Policies, which Should Be Implemented Simultaneously with the *Strategic Plan*.

General Comments

After decades of research and debate, there is now a clear consensus among scientists, which has been accepted by the United States, that climate change is occurring and that the combustion of fossil fuels by humans is the primary contributor. *See e.g.*, *U.S. Climate Action Report 2002*, U.S. Dept. of State, Washington, D.C., May 2002 ("*Climate Action Report*") at 5. Most scientists also agree, as discussed in detail by the United States in the *Climate Action Report*, that global climate change will cause devastating, disruptive, and wide-ranging impacts to climate, ecosystems, and public health and welfare. *Climate Action Report* at 81, *et seq.*, (Chapter 6). *See also*, *Climate Change Science: An Analysis of Some Key Questions*, National Research Council ("NRC"), National Academy of Sciences (2001) ("*NRC 2001*") at 18-21 (Chapter 6). Regardless of what the specific, regional changes will be, and despite some potentially beneficial localized changes, it is beyond dispute that harmful environmental and climate changes will occur. Among the types of likely changes that the United States has projected are the loss of sensitive ecosystems such as barrier islands, alpine meadows, and coastal marshes, accelerated extinctions and shifts of species, altered agricultural patterns, increased droughts and flooding, and increased infectious and heat-related diseases and illnesses. *Id.*

Two possible strategies for dealing with such changes are adaptation and mitigation. The former involves development of policies to accommodate the new environmental and climatic conditions with which we are faced by altering our current ways of life. To do this on local and regional levels, we will need to know to what specific new conditions we will need to adapt. Answering that question seems to be the focus of the *Strategic Plan.*

Mitigation, on the other hand, involves policies intending to minimize the coming changes by implementing various measures to reduce the primary cause of global warming: elevated levels of carbon dioxide in the atmosphere. Such measures include emissions reductions, carbon sequestration, and increased reliance on renewable energy sources, among others. According to current, commonly-accepted and credible scientific conclusions, and taking their inherent limitations into consideration, we now know enough to know that mitigation measures must be started immediately for them to have meaningful, practical effect and for us to preserve all of our response options.

1. Uncertainty Is Not a Basis for Inaction.

The overwhelming message of the *Strategic Plan* – that increased certainty through more research, better data, and refined modeling efforts is necessary before policy makers can act – is inconsistent with the *NRC 2001* analysis on which it purports to be based. For example, based on scientific uncertainty, the *Strategic Plan* resists concluding, as most scientists and the United States already have, that human activities are the main contributor of global climate change. *See Strategic Plan* at 5-6, *quoting NRC 2001*. The *Strategic Plan* claims that the NRC supports this position; however the NRC actually states that it generally agrees with the Intergovernmental Panel on Climate Change's ("IPCC") assessment of human-caused climate change. *NRC 2001* at 1 ("Greenhouse gases are accumulating in Earth's atmosphere as a result of human activities, causing surface air temperatures and subsurface ocean temperatures to rise").

The NRC's real point is that some uncertainty cannot be avoided, so that conclusions must be evaluated in light of the "level of confidence" or "caveats" associated therewith. *NRC 2001* at 1. The NRC recognizes "[c]limate projections will always be far from perfect." *NRC 2001* at 22.

There clearly is enough certainty to support the conclusion that climate change is a real problem and that we must act now to begin to solve it. To the extent that uncertainty remains, however, it should not prevent the federal government from taking actions to address the problem that we know exists. In fact, such a scenario is not unique to the problem of climate change but arises in many contexts within our society. Consider, for example, the relatively much simpler problem of understanding how exposures to toxic chemicals cause cancer cells to be created. While research presses to understand the actual mechanisms at work, we still must act to regulate carcinogens based on the information currently available. Inaction until more uncertainty is removed would be irresponsible and unthinkable. The same applies to global climate change.

Additionally, much of the uncertainty to which the *Strategic Plan* refers is *not* scientific, but rather stems from not knowing how humans will respond to, and thereby affect, the problem. For example, as discussed below, adopting different emission scenarios will greatly alter the problem. The *Strategic Plan* should urge action to address the human factors in the equation immediately, even as research proceeds to reduce scientific uncertainties.

Thus, contrary to the *Strategic Plan's* message, policy makers do, indeed, presently have sufficient information to act, as long as they also consider the potential limits of that information. As scientific and nonscientific uncertainty decreases, appropriate adjustments can be made.

2. Current, Commonly-Accepted and Credible Science Compels Immediate Mitigation Measures.

Unlike more traditional air pollutants, carbon dioxide emitted today will remain in the atmosphere for a century or more. The long lifetime of carbon dioxide already in the atmosphere and the momentum of the climate system are projected to cause the climate to continue to change for more than a century. *Climate Action Report* at 82. Scientists agree, as recently stated by the United States, that "even after achieving significant limitations in emissions of CO2 and other greenhouse gases," impacts of climate change will continue to be felt for centuries. *Id*. at 103; *see also, NRC 2001* at 1 ("national policy decisions made now and in the longer-term future will influence the extent of any damage suffered by vulnerable human populations and ecosystems later in this century.") While carbon dioxide will remain in the atmosphere, any actions now and in the future to reduce emissions will yield a benefit in combating global warming.

The concentration of carbon dioxide in the atmosphere today is roughly 370 parts per million (per volume) ("ppmv") and is increasing by about 1.5 ppmv each year. Top scientists worldwide, led in large part by the well-respected IPCC, have evaluated various modeled scenarios to analyze how soon carbon dioxide emissions reductions would need to begin to achieve stabilization of atmospheric carbon dioxide at concentrations between 450 and 1000 ppmv. *See e.g.*, *IPCC Third Assessment Report, Climate Change 2001, Synthesis Report, Summary for Policymakers*, Sept. 2001 ("*IPCC TAR*"). To have the option to stabilize the atmospheric carbon dioxide concentration at 450 ppmv – or about 1.5 times pre-industrial era levels – carbon dioxide emissions would need to begin to decline "in about 1 to 2 decades," drop below 1990 levels "within a few decades," and "continue to decrease steadily thereafter." *See id.* at 19-20. Keep in mind that, last year, the United States projected that carbon dioxide emissions will *increase* by 43% by 2020. *Climate Action Report* at 6, 73. Even if we could obtain stabilization at 450 ppmv, current modeling efforts show that stabilization at that relatively low concentration would still be accompanied by significant environmental and climate changes. *IPCC TAR* at 16-17, 21. *See also*, *NRC 2001* at 21.

Under all IPCC emissions scenarios, the projected concentration of carbon dioxide in 2100 ranges from 490 to 1250 ppmv, which includes a variation of -10 to + 30% to account for uncertainties. *IPCC TAR* at 8. At higher stabilization concentrations, impacts to sea level and temperature, among other things, become progressively larger. *Id*. at 17. Thus, "[m]itigation actions to stabilize atmospheric concentrations of greenhouse gases at lower levels would generate greater benefits in terms of less damage." *Id*. at 21.

The lesson to be learned from the IPCC scenarios is that for mitigation efforts to be most helpful in achieving stabilization at a level that will generate greater benefit in terms of less damage, and to prevent certain stabilization levels from being eliminated simply due to inaction, we must – *in the very near future* – begin to implement meaningful mitigation measures.

3. Adaptation without Mitigation Will Be More Costly, Economically and Non-Economically.

As discussed above, delay in beginning serious mitigation measures will eliminate options to stabilize at lower concentrations, thereby reducing the potential to achieve greater benefits in terms of less damage. *See IPCC TAR* at 21. Higher stabilization concentrations will be accompanied by more severe adverse impacts in more geographic areas. *Id*. Adapting to impacts of greater magnitudes spread over more regions will necessarily be more costly. The suggestion in the *Strategic Plan* that adaptation, alone, may be more cost effective than mitigation, simply does not take this into account.

In addition, although many of the resulting harms will not have a specific dollar value (*i.e.*, accelerated extinctions of species; losses of sensitive ecosystems), they have immense non-economic value. For future generations to be deprived of certain natural resources because of our inaction would be a tremendous cost. Such impending losses beseech our moral and ethical responsibility as stewards of the planet. The NRC has noted that "[n]atural ecosystems are less able to adapt to change than are human systems." *NRC 2001* at 21. Therefore, if we will only plan to adapt and forego mitigation are we then agreeing that sensitive natural ecosystems are simply not "worth" saving? Surely not.

Conclusion

For the foregoing reasons, the *Strategic Plan* should clarify that: 1) the program it sets out is geared towards adaptation to – not prevention of – global warming, and 2) therefore, the research proposed by the *Strategic Plan* does not forestall the need for the United States to immediately move forward with mitigation measures.

BAKER, NOAA

I reviewing the subject document I did not see any discussion of the climate system as a dynamical system. From this perspective, the accurate measurement of the global tropospheric wind field is essential to advancing our knowledge of the transport of water vapor and other important atmospheric constituents. The Doppler wind lidar is uniquely qualified to provide the necessary wind measurements. Such measurements are critical to our successfully addressing many important climate change questions. I offer the discussion below to help address this shortfall in the draft, which otherwise is excellent.

Importance of Doppler Wind Lidar Observations to Understanding Climate Change

The first report of the Intergovernmental Panel on Climate Change (IPCC, 1990) assessed prospects for investigating climate change. The IPCC identified the five the most critical areas for intensive study to be: 1) control of the greenhouse gases by the Earth system, 2) control of radiation by clouds, 3) precipitation and evaporation, 4) ocean transport and storage of heat, and 5) ecosystem processes. There is a clear mandate to refine our understanding of the hydrologic and biochemical cycles. We need to better quantify the transports, phase changes, and chemical processes that interconnect the component subsystems of the planet. Wind data are fundamental to all of these calculations. The Doppler wind lidar (DWL) stands as the unique sensor capable of providing the required global measurements of this key parameter.

<u>The Hydrologic Cycle</u>

On climate timescales (e.g., a month or longer), the atmospheric branch of the Earth's hydrologic cycle can be expressed as a balance between the column-integrated convergence of water vapor and net evaporation minus precipitation. The spatial and temporal variability in the components of this balance has great importance and, unfortunately, substantial uncertainty (Chahine, 1992). Coupled with water vapor measurements from passive microwave and infrared sounders, DWL data could play a unique role in isolating this fundamental component of the Earth's energy cycle. Furthermore, DWL measurements and other estimates of evaporation minus precipitation are strongly complementary. Calculation of flux convergence of water vapor using winds would serve as an independent check on estimates of evaporation minus precipitation; given any two measurements the third can be found as a residual.

As stressed in the IPCC document, a major uncertainty in modeling climate scenarios, past, present, and future, is uncertainty in representing clouds. On all scales, cloud-system dynamics are strongly linked to the circulation patterns. Thus, to verify performance and integrity of climate models, it is necessary to improve the understanding of not only cloud and water vapor distributions, but vertical and horizontal transport of water vapor.

Aerosols and the Carbon Cycle

One of the more striking shortfalls in global climate modeling continues to be the treatment of aerosols. Although their increase is generally thought to enhance cooling due to increased albedo, an adequate understanding of their production, transport, radiative impact, and deposition is only rudimentary at best. Production by anthropogenic (industrial sources, fossil fuel burning) and natural processes (volcanic and biogenic emissions and aeolian transport) is widely distributed. Many important radiatively active aerosols have atmospheric residence times on the order of one week (Penner et al., 1992), and thus their dispersion depends critically on the evolving wind field. The atmospheric transport, interaction with clouds and radiation, and removal by precipitation requires better knowledge of the wind field (trajectory modeling) coupled with hydrologic modeling (scavenging by precipitation processes). Contemporary research into processes governing the carbon cycle has focused on searching for a "missing sink" of approximately 1 – 2 G of carbon per year (IPCC, 1990). Methodologies to investigate the carbon budget have used inversion methods (Enting and Mansbridge, 1989) as well as direct transport models (Tans et al., 1990). Both approaches rely upon the *a priori* specification of the wind field, the former in solving for sources/sinks required to explain the observed CO2 concentrations and the latter in direct calculation of CO2 distributions resulting from measured or modeled sources and sinks. Because the wind field is poorly measured over critical source/sink regions, such as tropical rain forests and boreal ecosystems, refinement in transport estimates via lidar wind measurements would be an important contribution to narrowing the uncertainties in the carbon cycle.

Impact of Deforestation on Rainfall

To highlight the significance of the present uncertainties in the tropospheric wind analyses for conducting climate change research, consider the findings summarized in the table below concerning the impact of deforestation on rainfall.

Table 1. Sensitivity of the moisture flux divergence to uncertainties in tropospheric wind analyses, contrasted with the effect of Amazonian deforestation (rain forest replaced with grassland)* on rainfall (based on findings of Wang et al., 1992).

Region	**Current wind analysis uncertainties**	**Resulting uncertainties in moisture flux divergence (for precipitable water**
North America	2.3 m/s	2.1 cm/month

South America	3.0 m/s	4.9 cm/month
Effect on Amazonian rainfall		~4.0 cm/month (~20% - 25% reduction)

*See Lean and Warrilow, 1989; Shukla et al., 1990.

The present uncertainties in the tropospheric wind field alone produce corresponding uncertainties in the moisture budget that match or exceed the drying effect found in deforestation experiments with GCMs. Without the measurements of the ageostrophic wind (the wind component crucial for accurate transport calculations), our present level of uncertainty in the water vapor flux divergence calculations will not improve substantially.

Aerosols, Trace Gases, and the Biogeochemical Cycle

Next to water in importance to life on Earth are compounds involving carbon, nitrogen, and sulfur. There is abundant evidence that increases are occurring in the atmospheric composition of radiatively active trace gases composed of these elements, including carbon dioxide, methane, oxides of nitrogen and sulfur, as well as the chlorofluorocarbons (IPCC, 1990). Many of these changes are thought to be a result of human activities superimposed on natural fluctuations, but the complex causes and relationships are not yet fully understood. Whatever the cause of these increases, the resulting changes in regional and global climates over the next 100 years could possibly exceed those experienced by mankind. Thus, there is an urgent need to understand the biogeochemical cycles of these elements. The same processes that are needed to better define the hydrologic cycle will also be critical in estimating the long-range transport of trace gases and aerosols. An example for which global wind data would be valuable is in understanding the possible role of tropospheric dynamics in modulating the ozone hole during the Southern Hemisphere stratospheric spring. Global wind data should also be of value in studies of the influence of transient waves on the stability of the northern polar vortex.

BALDWIN, NORTHWEST RESEARCH ASSOCIATES

Subject: Use of terms North Atlantic Oscillation (NAO), Arctic Oscillation (AO), Northern Annular Mode (NAM), Southern Annular Mode (SAM)

In my view the overlying concept is that of annular modes, which would tend to occur in any rotating stratified fluid. Narrowing the focus to Earth, we see annular mode structures in both hemispheres, and the structures are more annular the farther one gets from the influences of continents and oceans.

The research community is also getting closer to having a theory of annular modes, or at least a theoretical framework in which to understand them. As it stands now, one criticism is that they are empirical orthogonal function (EOF) patterns. Yet time and again we see what are essentially the annular mode patterns resulting from calculations that do not have EOFs built in.

I think that it is important to maintain (in the context of the CCSP document) an appreciation of two aspects of this phenomenon. First, the vertical connection to the stratosphere in which (for reasons we do not yet understand) the stratospheric annular modes are closely connected to the surface annular mode pattern (Baldwin and Dunkerton, 2001). The second aspect is that the surface annular mode patterns actually extend through the tropics well into the opposite hemisphere (Baldwin, 2001). Over the next few years I believe that we will see more connections recognized such as between the MJO and the annular modes.

Concerning names and acronyms, I recommend the following consistent usage:

1) Use SAM and NAM to describe the annular modes at any pressure level in either hemisphere. 2) For historical continuity, use NAO/NAM to describe the tropospheric (or surface) NH pattern. 3) Abandon the AO terminology, but the AO term should be mentioned in the text, since it is still widely used. 4) Since we cannot refer to the NAO in the stratosphere, and the relevant patterns are not confined to the North Atlantic, I do not think that using the NAO terminology will work for the CCSP document.

References:

Baldwin, M.P. and T.J. Dunkerton, Stratospheric harbingers of anomalous weather regimes, Science, 244, 581-584, 2001.

Baldwin, M.P., Annular modes in global daily surface pressure, Geophys. Res. Lett., 28, 4114-4118, 2001.

BALES, UNIVERSITY OF ARIZONA

The white papers that comprise the draft plan provide the basis for a comprehensive, balanced research agenda that, when implemented, will address most of the priority issues associated with climate change, its causes and its effects. In revising the draft, particular attention should be given to integrating the different sections of the plan. There is some danger that the plan tries to provide too much and will thus result in an unfocused effort. Particular attention needs to be paid to this, making linkages between the chapters and putting the different components into a structured, coherent plan.

BARNETT, USCD

The following general comments may be helpful in the next draft of the Plan

1. The Plan covers everything anyone might want to do…not necessarily bad as it brings along the whole audience
2. The delivery times promised for a number of items (2-4 years) are unrealistically short. Other work promising something in 5-15 years sound like shots in the dark. Need to develop a serious time/phase diagram for the next 5 years (at least)
3. There will not be enough money to do everything mentioned in the plan. Therefore, IT IS IMPERATIVE, that work discussed in the Plan be prioritized!
4. The management plan only looks up the food chain. But the work is done much further down the chain. There is no discussion of how the programs will be

managed and/or implement. That is where the new challenge comes in, for traditional gov. approach to research won't work well here.

5. No mention is made of computer resources. WITHOUT DEDICATED SUPERCOMPUTERS THE ENTERPRIESE IS DEAD FROM THE GET GO!
6. The splitting of effort between GFDL and NCAR will be fatal to both. If GFDL is largely operational, then its best scientists will leave. If NCAR does no operational work, they will continue to drift into esoteric areas that suit individual researchers, but accomplish little. The U.S. can afford two modeling groups. Let them BOTH do model development AND operational production work.
7. The observational effort is diffuse and ill defined. It should be strongly focused to specific targets. The existing/proposed observations need to be strongly justified, e.g. Although it is an article of faith, just why do we need a global ocean observing system? What will some expensive satellite system that lasts only a few years do to help understand climate? Etc.
8. Much of the work is basic research. There needs to be a goodly dose of highly directed effort aimed at very specific problems and this seems largely missing from the current plan. This work is not best done in a University and NOAA does not have the talent to do it. We need interdisciplinary teams from many different types of organizations to do the work. How will they be organized and funded?
9. This Plan will not accomplish the 'end-to-end' structures and tools needed for the multi-disciplinary climate problem. Its organization, structure and management are just business as usual. You need to form interdisciplinary teams from the start. There seems no way to do this in the traditional, stultified management structure outlined in the Plan.

BARTLEIN, UNIVERSITY OF OREGON

First overview comment: The role that longer-term paleoclimatic records (and the modeling and explanation challenges they present) can play in understanding climate change and its uncertainties is somewhat underemphasized in the strategic plan. The relevance of the instrumental and proxy records of the past few hundred years for placing in context recent climate variability is made clear throughout the plan, but the efficacy of paleoclimatic information could be greatly enhanced by expanding the time depth of the information considered as described for the Holocene and longer intervals in Ch. 6 of the NRC (1999) volume Global Environmental Change: Research Pathways for the Next Decade. The last glacial/interglacial cycle (the past 150,000 years or so), the interval since the last glacial maximum (the past 21,000 years), and the Holocene (the past 11,000) years each provide paleoenvironmental records that can inform our understanding of climate change as follows: (1) the scope of the changes in the controls of climate (solar radiation, atmospheric composition including trace gasses and aerosols, land-cover changes) are of the same order of magnitude as those expectable in the future, and consequently allow examination of the response of the climate system to changes in its controls that exceed those of the instrumental period; (2) the longer-term record includes many examples of abrupt climate changes and their reverberation throughout the climate system; (3) the longer-term record provides evidence of significant responses to climate change of all major environmental subsystems including the wholesale reorganization of the terrestrial biosphere and major changes in continental hydrological systems; (4) emerging terrestrial and marine records provide evidence that the major modes of variability in the present-day climate system, like ENSO, have changed in importance over time; and (5) the longer-term record provides the only natural laboratory for testing the ability of climate models to simulate climates very different from the present one. Together, these properties of the longer-term record provide opportunities for understanding climate changes, developing and testing climate models, and hence for reducing the uncertainties that accompany projections of future climate changes.

BAST, THE HEARTLAND INSTITUTE

The Draft Strategic Plan for the Climate Change Science Program does an admirable job avoiding advocacy and rhetoric, focusing on research questions that need to be addressed, and balancing the contradictory convictions of some of its authors and contributors. In particular, it stresses the uncertainty of climate change science and predictions and calls for testing climate models against the climate record. However, in a few places the Plan is still agenda-driven rather than aimed at "credible fact finding."

Second Overview Comment: The Plan does not recognize or address the bias resulting from the self-interest of the three-billion-dollar-a-year climate change research industry that has emerged since major federal funding began in 1987. It should frankly acknowledge the need for independent voices as a counterweight to institutional bias.

Third Overview Comment: The Plan does not recognize or address the misrepresentation of government-funded science by government-funded advocacy groups in the past, which has confused the public and led to adoption of expensive and ineffective public policies in the name of "stopping global warming." Grants to groups that have distorted and exaggerated the potential threat of climate change should not be renewed.

BERG, HUMBOLDT STATE UNIVERSITY

While I am encouraged at the push to work towards a comprehensive understanding of how human activities affect our climate I find that the "strategy" does little to actually address the issue. the issue is that our actions are having an affect and that we have been certain of this for quite some time now. Just EXACTLY how we are affecting the Earth's ecosystems and by what means we are affecting these changes is a valuable goal and seems to be the focus of this plan. While acknowledging the importance of this plan I feel that the focus for the global community with the U.S. at the helm is to strategize about how we can implement both technological and social change that will reduce the impacts that we are having on our life support systems.

Using technologies that we already have we can continue to have the lifestyles we desire without the negative environmental impacts of our current energy policy. We know that output of CO2 has an impact on the global climate and we have the technologies available to us for CO2-less energy and it is hydrogen power. Hydrogen, undoubtably, is our energy future and so we should have hydrogen and hydrogen fuel cells at the top of our agenda with anything concerning global climate change. We also know that for the most part methane is the other half of the greenhouse gas problem and we likewise have technological solutions that would not only reduce the output of Mh4 but provide us with clean electricity.

This is a wonderful oportunity for us (the U.S.) to be at the forefront of a technological revolution that would simultaniously make great steps towards dealing with global climate change as well as providing a tremendous amount of jobs and a great boost to our economy which would by far outway any economic losses incurred from reducing greenhouse gas emissions.

III. Inclusions:
I would strongly suggest including a section focusing on technolgical solutions to the emmissions of the greenhouse gasses we are spending so much time and money researching the effects of.

Also included should be a section detailing the economics of cutting back on greenhouse gas emissions now (i.e. the projected job losses and the cost to business) compared to the costs of dealing with the problems that are sure to arrise in the future if we continue to emmitt greenhouse gasses at our current and increasing rates. As well as a section including the public costs both social and financial of our current energy policy vs. a policy supporting non-greenhouse gas based energy.

This would serve to answer the main point point of resistence to dealing with greenhouse gas problems. We may not know exactly what the effects will be (and to this effect the strategic plan is relevant) but the isue does not seem to be with what the effects of our current energy use is on our gobal environment nor does the problem reside so much in whwther or not the technolgies exist to continue our current lifestyles without producing such a huge quantity of greenhouse gasses but rather what the cost will be to our economy. The arguement against changing our energy ways is ALWAYS that it is too costly and rarely that we don't know precisely what the effects are so whoy don' t we focus a large part of the strategic plan on figuring out exactly what the economic effects of mitigating climate change

BINDSCHADLER, NASA

I applaud the effort of the CCSP Project to engage the various departments of the Administration throughout the formulation phase of CCSP. I, however, carry a heavy load of skepticism that such engagement will only last as long as the political wind is favorable. The scientific community, by focusing on this plan, is diverted from taking this Administration to task for the lack of effort in addressing global warming either specifically, through promoting mitigation strategies, or intellectually, through enhanced research efforts. Having spent my entire scientific career on the outskirts of the Beltway, I have seen repeated instances of sound science plans, such as I hope the CCSP becomes, spun into seeming irrelevance by political expedients. Serious and deliberate thought needs to be directed as to how to avoid this fate. As one possibility, I suggest the Administration be required (?) as part of this plan to set up an independent review and assessment board at the National Academy of Sciences funded to annually review the progress made in fulfilling the goals of the CCSP. Some means must be established to keep the politically driven government on a sustained course of sound scientific research.

BURGIEL, DEFENDERS OF WILDLIFE

While elements of the science program are commendable, such research should not be taken as a substitute for meaningful action at the present moment to limit greenhouse gas emissions. The majority of the world's scientific community (including most U.S. climate scientists) has already concluded that the earth's climate is changing. Additionally, the program highlights the need to reduce uncertainties, but the plan must establish a threshold to trigger mitigation and adaptation activities even if some uncertainties remain. Additionally, the research program must assess and justify any impacts of delaying efforts to reduce emissions over its projected ten year duration.

BUSALACCHI, UNIVERSITY OF MARYLAND

The comments that follow were predicated on the following questions.
Is the plan representative of current scientific understanding?
Are the leading questions/objectives appropriate?
Are the research questions, needs, products consistent with lead questions?
Are these the best questions?
Do the research elements feed into decision support?
Realistic deliverables: scientifically, financially, are they useful?
Are the linkages/crosscuts substantive and attainable?
Can synthesis and integration be improved? Overview comments on plan as a whole:

Is the program oversight/management bound to be effective?

As for the document as a whole, "Strategic Plan for the Climate Change Science Program" is more of a science plan and less of a strategic plan; it describes the what and why (questions, research needs, products and payoffs), but not the how. Much of what has been written in the plan has been around for quite some time. If one wants to be critical, what is new here?

CCSP is much more encompassing than the name implies:
CCRI + US GCRP $\neq$ Climate Change
In fact the name is potentially misleading.

CCSP builds upon and is a reformulation of US GCRP (i.e., takes advantage of solid research base and heritage).

Links/crosscuts have been identified better than previous draft US GCRP plans, as written they are necessary but far from sufficient.

Climate variability and climate change are used interchangeably and unevenly throughout the document.
Modeling: With regards to modelling, the Two Center Strategy was originally intended to respond to the IPCC assessment. In this document it takes on a much larger role across all of US climate modelling. If that is not intended, then what is the strategy for climate modelling within CCSP? The Two Center Strategy represents an NSF/NCAR – NOAA/GFDL axis. Where are the links to NOAA/NCEP, NOAA/NESDIS, NASA? This

represents a fundamental, if not fatal, flaw in the program. For example, neither center has core competency in data assimilation or use of remotely-sensed observations. As we make the links to regional scales and extreme events, data assimilation, interaction with NWP, and remotely-sensed observations take on added importance. In addition, what is the process by which modeling activities issue forth observational requirements? The plan describes how observations will be used by models, but not how models will be used to influence observing systems. Lastly, the path is unclear for model-based connections between:
climate change→climate variability→extreme events→regional basis

<u>Observations</u> The report makes repeated reference to "THE" Climate Observing System. While attainable, there is no such entity as yet, rather what we have is a patchwork, often building on the degraded WWW.

<u>Resources</u> It is implicit that no new (significant) resources are available. We need to be honest with ourselves that a significant increase in our ability to deliver (obs/modeling/manpower) is questionable without appropriate funding. This plan is to discuss an approach to climate that borders on being operational without the funding mandate. The infrastructure and funding in this nation is set up to do operational weather prediction, but as yet, not climate. Capitol Hill also needs to be honest with the public. Already, op-ed pieces are appearing in the popular press from the leadership on Capitol Hill pointing to CCSP as a major advancement and acceleration in research on climate change. The present plan is neither.

<u>Implications:</u> As described in more detail in the comments on Chapter 6, the implications for the following:
Separation between climate variability and change
Lack of reference to US CLIVAR Science Plan or role for CLIVAR SSC (in contrast to Water Cycle and Carbon Cycle Science Plans in referred to in Chapters 7 and 9)
No reference to IRI
Weak link to observations both in situ and space based
Role of process studies
Lack of substantive Key Linkages both nationally and internationally

Suggest that there are fundamental shortcomings in the CCSP that cannot be dealt with by merely listing/mentioning the above. More than wordsmithing is needed to address these issues. Rather this indicates that "a coordinated research management effort" while essential as stated in the text, has not yet been achieved. Interagency coordination as reflected in the unevenness of this, and other chapters remains a problem.

<u>Challenges:</u> A true strategy needs to be developed for cross chapter interactions be it climate modeling (e.g., water, land, or for that matter any of the "key linkages", climate-ecosystem links need to consider change in extreme events) or climate observations. What is the process by which one research elements can levy a requirement on another?

The management plan IF adhered to has the potential for improving upon GCRP, if not, we will have business as usual which is counter to the President's guidance to "improve coordination among federal agencies". However, in several areas and on several topics as reflected in the plan, interagency coordination and collaboration has not been achieved. This has been a problem throughout the GCRP and the initial signs provided by the draft plan do not indicate that major changes can be expected.

The CCSP implementation planning needs to be as deliberate as the generation of this science plan. The "Devil is in the Detail" when it comes to implementation. Once again, the present plan is mainly a rehash of existing science plans. It really is not a strategy plan as advertised, and the approach to implementation is less clear.

STATE OF CALIFORNIA

Thank you for the opportunity to comment on the draft Strategic Plan for the Climate Change Science Program.

The State of California takes climate change quite seriously. We are concerned about the potential costly impacts of climate change on water, energy, and other key economic and environmental systems in the state. In recent decades for example, stream flow records show a trend toward earlier snowmelt in the principal water supply for the state, the snow pack of the Sierra Nevada: a likely early manifestation of climate change. If this trend continues California will experience an increased danger of floods in the winter, lower availability of water during the summer, and less reliable hydropower generation overall. As a result of this impact and others, such as potential sea level rise and dramatic effects on ecosystems, climate change will likely have significant ecological and economic impacts throughout the state.

Since 1988 the State has acted on its concern. State agencies have implemented inventories of climate-forcing agents and assessments of likely impacts of climate change on the state's economy and environment. More recently, the Legislature created the California Climate Action Registry for the voluntary registration of greenhouse gas emission reductions and also authorized the California Air Resources Board to develop regulations governing greenhouse gas emissions from automobiles and light trucks. The current update of the California Water Plan, a policy statement of central importance to California's environment, economy and culture, will be the first to explicitly address the likely impacts of climate change on water supply. It is against this backdrop of state concern that we review the Strategic Plan as it sets direction for those unique research and outreach activities that only the federal government can pursue.

We have four general recommendations for the Plan.

First, we urge that the Plan develop a more pronounced regional focus. While global change has heretofore been approached quite naturally as the result of phenomena best described at the global level, recent research shows that some of the drivers, such as aerosols, black carbon and land cover change, and nearly all of the impacts -on water, ecosystems, land use, human health – are regional in nature. It may be best now to consider global change as the result of coupled interactions between regional and global phenomena. Since adaptation is a major focus of the Plan, it is essential to develop the scientific tools and decision-support institutions – data, models, computing infrastructure, and collaborative structures – at the regional scale where real world adaptive decisions occur.

We offer California as an ideal pilot project for both the federal program and the IPCC to investigate the full scope of a regional collaboration. The state is among the most diverse climatologically and ecologically in the nation, providing an ideal laboratory for global change research. As noted above, the state already considers climate change an important issue and has dedicated resources to the issue. Of particular importance are the recent Research and Development Roadmaps for Climate Change Research, developed by the California Energy Commission's Public Interest Energy Research (PIER) program. These research plans focus on topics noted in the federal Plan as well: improved measurement of climate-forcing agents, modeling and projection of the regional climate, evaluation of opportunities for geological and biological carbon sequestration, studies of ecological impacts of climate change, and economic analysis of adaptation and mitigation measures. The research program is one of the most comprehensive state climate change research programs in the nation, and provides an excellent starting point for a more expansive collaboration with the federal science agencies.

Second, we strongly endorse recommendations made at the recent workshop in Washington, DC to quickly increase the computing capacity available for global change research. The capacity includes not simply the computing hardware itself but also the human and institutional resources needed to use the hardware effectively. While the distinction between scientific and policy-related modeling has merit, we find that it is secondary to the distinction between global and regional perspective noted above. Consequently we urge that the Plan bolster the capacity for both scientific and policy-related modeling but within the regional context of global change. For instance within California, we have already identified the need for both improved regional climate modeling and improved evaluation of policy responses. Improving by orders of magnitude the computing capacity available to California academic and government institutions may be the single most transformative supply-side actions the federal program could take.

Third, we strongly suggest that the Plan close the curious gap between its research on institutions for adaptation and its own operation as just such an institution. By re-casting its own reporting, outreach and management around the pre-requisites for adaptive institutions, the Plan has an opportunity to break truly new ground in the way science informs and frames decision-making. The regional focus we advocate in our first comment would provide the Plan with multiple venues for that investigation. By developing regional collaboration with California, the Plan would transform the rather sterile enumeration of policy actors and development of information into a participant-observation effort much more productive for both decision researchers and decision makers.

Finally, we insist that the Plan address both mitigation of human forcing of climate and adaptation to climate change within its assessment of policy responses. If, as Under-Secretary Card indicated at the workshop, the national policy is to reduce intensity of GHG emissions and eventually to reduce GHG emissions themselves without serious repercussions on the national economy, then the CCRI specifically, and the CCSP more generally, must focus much more on describing the likely costs and benefits of plausible mixtures of both mitigation and adaptation strategies. The Plan's avowed desire for policy relevance requires such a dual focus. The California climate change research program has already targeted significant resources to narrowing uncertainties regarding the costs and benefits of various measures for large-scale greenhouse gas abatement. As noted above, California is already moving ahead with the planning and implementation of measures to reduce GHG emissions, and we place a high priority on improving our economic understanding of how to accomplish this reduction most efficiently. In our view, the apparent de-emphasis of this area in the national Strategic Plan is a major gap. We consider it vital to treat uncertainties in the economics of GHG abatement symmetrically with those attending climate science.

We have attached detailed lists of comments from the California Energy Commission, the California Resources Agency, the California Air Resources Board, and the California Department of Water Resources. All are provided in the format specified by the CCSP. We appreciate your emphasis on open dialogue with stakeholders and offer our comments in hopes of achieving a more productive federal research program that in turn benefits the states as well. We re-iterate our desire to fully coordinate our research and assessment efforts with those of the US Climate Change Science Program in the form of a regional project to achieve synergies, avoid duplication, and generate information useful not just to California but to other U. S. regions as well. We would welcome the opportunity to discuss the collaboration at your earliest convenience.

CELATA, LAWRENCE BERKELEY LABORATORY

I understand that it is not quite within the scope of your plan, but I would like to point out that putting emphasis on controlled nuclear fusion as a method of eliminating greenhouse gases is one of the most important, and most farsighted, ways of coping with the problem of global warming. The economic impacts you foresee if limits are put on greenhouse gas production can be avoided by using energy production methods which avoid carbon, and nuclear fusion is about the only method that could provide large-scale power for industrial society without producing greenhouse gases. Perhaps you could find somewhere to mention this in your report. Thank you.

COAKLEY, JIM – OREGON STATE UNIVERSITY

Where's the beef?

Much of what is said, one can't argue with. We've been saying the same things for the past 20-30 years. What might be called new thrusts: the need to reduce the uncertainty of the aerosol forcing, the need for a climate-quality observing network, as opposed to the traditional weather observing network, represent, in part, the 5 to10 year evolution of our understanding, and in part, addressing long-standing deficiencies in existing observations. One expects to see similar refinements to our thinking 5 to 10 years hence, and it's doubtful that we'll ever see an ideal observing system, but surely we can do far better than we are doing now. Nevertheless, how all these fine words are to be implemented is the key. This document says nothing about implementation other than it will be worked out by "scientific committees." The vague notions floated in the document about advancing this or advancing that, are essentially meaningless without at least outlines of plans for how these advances are to be accomplished. As it stands, the document is lengthy but lacks content.

PHILIP COOK, PRIVATE CITIZEN

I'am glad that someone is discussing climate science and I appreciate your program. The speaker said if I have any input please share:
For the world: Safety first technologies –

CORWIN, BLUEWATER NETWORK

Climate change is no longer a question, but a reality that has the potential to devastate our economy, environment, and health, unless we take appropriate steps to immediately begin reducing our greenhouse gas emissions. Today, the overwhelming majority of the world's scientists agree that the accumulation of heat-trapping greenhouse gases such as carbon dioxide from the burning of fossil fuels has caused the global temperature to rise an average of 1° F during the past century. By 2100 the Earth could warm another ten degrees, increasing floods, droughts, forest fires, and the severity and numbers of storms, disease and pestilence outbreaks.

We recognize the need for decision makers to base policies on sound science, however research should not be substituted for action. During the past 20 years scientists have made leaps and bounds in climate change science and modeling. Now is the time for decision makers to begin utilizing this science to develop responsible greenhouse gas emissions reduction policies, while at the same time, continuing their research on projected environmental, economic, and health impacts, as well as critical mitigation strategies and technological solutions.

The *Strategic Plan for the Climate Change Science Program* is too focused on eliminating the uncertainties of climate change science (an impossible feat) and fails to focus on critical ecosystem-based and long-term research on the impacts of climate change on public lands and waters, research into mitigation strategies and technological solutions, as well as research on climate change impacts on low income communities.

It is time to make progress in these areas rather than waste valuable taxpayer money on redundant research, but on long-term studies and solutions.

Although we acknowledge that climate change research is important, this research should build upon the findings of past research, rather than duplicating it, and should focus on solutions to predicted impacts. The time-frame for effectively avoiding the worst impacts of climate change is relatively short, therefore mandatory actions to reduce emissions are needed now, not ten years from now when emissions will be much higher and more costly to reduce.

CRAIG, SIERRA CLUB

The Sierra Club welcomes the opportunity to comment on the draft Strategic Plan of the Climate Change Science Program (the draft Plan). As Chairman of the Sierra Club's

General Comments

Global Warming and Energy Committee, I served as a panelist at the December 3-5 workshop, and incorporate by reference as a part of these summary comments his prepared comments submitted for the record at the workshop.

While the draft Plan has some decided strengths in its discussion and treatment of particular aspects of Earth System science, it is fundamentally flawed as roadmap for a policy-relevant research program. The Sierra Club believes that the risks of global climate change are far too important to humans and the supporting Earth System as a whole to treat them, as this draft Plan does, primarily as a subject for research .

Human-forced global climate change is a problem of steadily growing importance that calls for responsible action now. There is so much momentum inherent in the several components of the Earth System that respond to greenhouse gas forcing, and so much momentum inherent in the socioeconomic system that is responsible for steadily increasing greenhouse gas emissions, that there is no room for the luxury of another decade of scientific studies to finely tune response measures.

The draft Plan assumes that our understanding of global climate change today is so incomplete that no action is prudent or feasible before several or many more years of additional research passes by. The Sierra Club rejects this view, and believes this draft Plan should be recast so as to support a policy stance of adaptive decision making and management, recognizing that meaningful steps to reduce greenhouse gas emissions are needed now, and that increased understanding over the coming years should impel frequent reevaluation of policies to combat this major problem of the 21st century.

Our principal concerns about the draft Plan are:

1. CRITERIA FOR ACTION ARE NEEDED
The plan discusses uncertainty, but fails to lay out criteria for deciding when mitigation or adaptation actions would be required. It does not articulate prospective policy actions that could be considered, nor what level of increased scientific confidence would be necessary to trigger such action. Thus, there is no basis for deciding which uncertainties, at what level, are impediments to decision making and which uncertainties might be less relevant to the decision making process. Informed policy making requires specificity.

2. ROLE OF ASSESSMENTS NEEDS CLARIFICATION
The assessment process should have two purposes: 1) to assist in informing on an ongoing basis policy-making activities ranging from localities to the national level; and 2) to determine analytically priorities for addressing the myriad of scientific uncertainties that may or may not be germane to critical policy issues.

3. NEED TO SUSTAIN AND BUILD ON THE RECENT NATIONAL ASSESSMENT FOCUS ON REGIONAL VULNERABILITIES AND CONSEQUENCES
The recent National Assessment served the very useful role of focusing at a regional level on the vulnerabilities and consequences of climate change. The draft Plan is silent on the

need to continue and expand this recent research effort that was focused on local and regional vulnerabilities and consequences.

4. FEW MAJOR SCIENTIFIC UNCERTAINTIES WILL BE RESOLVED IN A 3-5 YEAR TIME FRAME
Raising expectations for a major increase in understanding of complex science in a few years is misguided. The global climate change science program should be science-driven. The timing of scientific advancements is difficult or impossible to anticipate. Policy, on the other hand, can be implemented now using existing science, and revised as new science emerges.

5. MORE REGIONAL CONSEQUENCES RESEARCH AND ASSESSMENTS IS NEEDED
A useful model of the mobilization of effort required is that developed by the UK in its LINK programme at the Climatic Research Unit at the University of East Anglia. Link serves as an exceptionally useful and functional interface between the modelers at the Hadley Centre and the impacts research community throughout Europe and the IPCC. The understanding of climate change consequences in the US could be greatly accelerated by building a comparable partnership in this country.

6. TECHNOLOGICAL RESEARCH, DEVELOPMENT AND DEMONSTRATIONS SHOULD BE AN INTEGRAL PART OF A INTEGRATED, COMPREHENSIVE PLAN
Of the 24 breakout sessions at the workshop, only one was devoted to technological solutions. The literature is full analyses on how responding now to global climate change can provide a net benefit to the economy. Moreover, new energy supply industries based at home would cut back on the massive export of funds now being used to pay for imported energy from insecure regions of the world. Reductions in environmental disbenefits would offset many of the residual costs. There has been a debate over energy efficiency for many years. The unambiguous conclusion is that employing more efficient technologies saves money and reduces environmental impact.

The Sierra Club urges a full consideration of these policy considerations, and stands ready to further explain and support its several comments.

CRESS, PNNL

The draft plan as written is a difficult document to make specific, coherent comments about in the context of making limited specific changes in the document to correct a specific shortfall. The shortfalls of this document are too broad for such an approach.

The structure I will use will be to make "banner" comments followed by explanatory detail as appropriate. I have tried to limit my comments to specific issues, but that has been somewhat difficult in the face of some philosophical shortfalls.

WHAT IS THE "PLAN" SUPPOSED TO ENCOMPASS? Comment: I found, on first reading, that I had real difficulty coming to grips with the content of the document. The statement of intent was for the plan to be "strategic." However, it is at times "strategic," at times merely "tactical," and, worse, at times, a defense of specific projects by inclusion of a focused goal statement for a narrow research objective. For a strategic plan, this is unacceptable. Suggestion: The idea of presenting a strategic plan is to give broad credibility to the scope of the problem, the capabilities to address the problem and a methodology to work through the entire scope of the problem until it is fully resolved. The highest level of strategic thinking here should be how to present a HUGE problem adequately and at a level of detail wherein some level of success can be attained, recognized and documented as meeting the plan's intent. This document should be presented as the first of a series of documents that present in successive levels of detail the broad outline, capabilities and approach (i.e. the "what") down to implementation (i.e. the highly specific "which," "when" and "how") of a research agenda in specific focused areas. The current document needs to be stripped of inappropriate details that are really statements of implementation - don't lose them, but move them to a subsequent document or series of documents - perhaps discipline oriented)

WHAT IS THE "PLAN" SUPPOSED TO ACCOMPLISH? Comment: This question needs to be answered before much more is done and is, in part, one aspect of the previous comment. If this is to be a "strategic" document, it contains too much detail (maybe not enough scope or roadmap, so this is not a comment on volume). If this "plan" is to a "tactical implementation" plan - it is not nearly BIG enough - to do this in one document would create a document that looses everyone - should not be done. To be "strategic," the "plan" needs to be supra-agency and supra-current-research program.

Suggestion: All objective statements and goals that have a specific origin should be severely scrubbed and scrapped or made part of a strategic, non-parochial strategy for the needed research area.

CAN THIS "PLAN" BE CREDIBLE? Comment: Yes, if the intent stated by Jim Mahoney about openness and thoroughness is delivered on. To be credible, this plan CANNOT be seen or suspected to be merely an accumulation of defense statements for the Federal programs that might be affected by it. It will have to lay out a strategy that includes substantive changes in research investments - these changes must be recognizable as real and seen to be made. The overwhelming cynicism that pervaded the participants at the workshop was a fear that this would not be the case. Suggestion: First - as above - the plan needs to focus on strategic issues with no connection to current programs. Specific research objectives that currently permeate the plan need to be excised and "rolled-up" into the strategy as appropriate. Second - current programs then need to be assessed for the potential to be "ridden" (in the context of "riding trains already in motion) to produce the short time scale results that are desired and be identified to be of the highest priority. Third, the plan needs to address the sequences of "thrusts" that will be made and what current programs will be expected to achieve - these achievements need to be "stretch" achievements, avoiding the mundane or obvious that are easily stated "pro forma."

ARE "CLIMATE CHANGE" AND "CLIMATE VARIABILITY" SYNONYMOUS?

"Climate Change" vs "Climate Variability": "change" and "variability" are ambiguously interwoven and interchanged throughout the document. These are fundamentally different issues and need to be clearly identified. They are strongly related to "near" term and "far" term effects on the climate system and perhaps need to be addressed separately - they overlap on the data end and diverge on the modeling end, with a full time spectrum of effects in between. As drafted, the references are confusing and confuse the issue of a near term variable of a 1000 year effect.

DAILEY, INDEPENDENT RESEARCHER

There is available existing historical climate change data which indicates that the uncertainities of climate change variability is much greater than would seem to be indicated by the paper and that these variabilities are influenced by forces much greater than the small contribution of human society and this is presented in historical ice core data, ie.

Look at the ice core data on warming from Dansqaard, et al. Nature 1993 for the approximate time period 250,000 BP to 5000 BP and overlay the spikes starting at approx. 150,000 BP and 130,000 BP on the present warming trend that started at approx 20,000 BP, with the appropriate time scaling changes of course, and you will note some similarities to the present warming trend and the <u>peaks are higher</u> than the present trend. We have roughly done so and the resulting plot can be found at <http://www.accuracyingenesis.com/warming.jpg>http://www.accuracyingenesis.com/warming.jpg (We downloaded the original data plot from <http://williamcalvin.com/1990s/1998AtlanticClimate.htm>http://williamcalvin.com/1990s/1998AtlanticClimate.htm)

DECK, UNIVERSITY OF TOLEDO

Having reviewed parts of the Strategic Research Plan that I had access to, it is my opinion that the plan needs further review and input from the scientific community before it is adopted. It has some good points. But I don't see that it incorporates the full knowledge that science has at the present time.

DEPARTMENT OF TRANSPORTATION, LAWSON

1. Strategies generally appear unconstrained by resources. Ideally, the plan should include necessary resources, and at a minimum the products and payoffs should be prioritized to direct limited resources.
2. The strategy should include a specific timeline for each research area. CCRI seeks to produce decision support information within 2-4 years, yet many of the research needs are open ended and could last much longer. With the exception of ongoing research or monitoring efforts, product and payoffs should all have projected completion dates.
3. Much of the research has a national or international focus. Transportation decision makers operate on the local, regional, and statewide levels, as do most natural resource managers and other decision makers. Additional focus on the statewide, regional, and local levels will make the research results more useful.
4. The chapters should be presented consistently – bulleted research needs, followed by products and payoffs outline, appears to be the best format.

DIAZ-SOLTERO, USDA

Overview Comment #1: (1) *Identify and implement case studies that demonstrate application of climate science at regional and sectoral scales.* (2) *Designate California as a formal regional case study within the CCSP*. A California case study would demonstrate integrated application of climate and climate-related sciences into local, state, and federal resource management, rural and urban planning, and policy and technology development. Diverse and state-of-the-art efforts exist in California on climate modeling, basic climate and paleoclimate research, wildland and urban science, integrated and collaborative ecoregional assessments (Sierra Nevada Ecosystem Project, CalFed), and technology developments, with infrastructure to support them. California's natural, social, institutional, and political diversity makes it a microcosm of globally relevant climate challenges and opportunities. The diversity of distinct, keystone regions within the state (deserts to rainforests, significant mountain ranges, agriculturally dominated Central Valley, urbanized coastal strip, etc.), each with unique issues, affords opportunities for downscaling to subregional scales. Ongoing, nascent, and new efforts (e.g., a Sierra Nevada Climate Change Assessment) could be integrated into coordinated regional templates to serve as examples for other areas.

Overview Comment #2: *Prioritize mountain regions of the western US for an integrated initiative on climate science, assessment, and science-based policy within the CCSP*. Mountainous regions of the US are widely recognized as key centers of biodiversity, water reservoirs and water distributors, sources of clean air, minimally disturbed forests and wildlife habitats, forest resources, and playgrounds of wide demand. Steep elevational and climatic gradients and high natural fragmentation make these areas highly sensitive to changing climates. Mountain regions are thus both "canaries in the mine" for studies of early effects of climate change relevant to national and global modeling and planning, and critical areas in need of local planning, evaluation, and adaptation. Consortia on integrative study of mountain regions have been supported in other countries. Despite the excellent individual centers of research in United States mountain areas, a mountain climate network is lacking in this country. We recommend explicit priority to integrating efforts among mountain regions in western United States in the Strategic Plan.

Overview Comment #3: *Increase participation of the federal land-managing agencies, specifically USDA Forest Service, USDI National Park Service, US Fish and Wildlife Service, USDI Bureau of Land Management and the National Ocean Service (marine and estuarine programs) in CCSP. Encourage greater participation from state land-managing and resource agencies.* Although USFS, NPS, FWS, BLM, and NOS are included in CCSP through their department affiliations (USDA, USDI, USDC), these agencies have been underrepresented in the process to date. In addition to climate change research and science programs, these agencies bring long-seasoned expertise on several key foci that are treated as novel in the CCSP environment: decision-support, science-consistency, science-based policy, and integrated ecoregional assessments and planning. The passage of the National Environmental Policy Act of 1969 and subsequent federal environmental review and assessment acts catapulted the BLM and USFS into situations

where integrated science, assessment, and science-based evaluation and policy-making at local to regional scales have been on front stage for over 30 years. State counterpart agencies are in similar position of expertise and were underrepresented at the Workshop. Further, the USFS, BLM, FWS, and NPS collectively administer the vast majority of federal wildlands in the US, with the USFS and NPS focused in mountainous regions, which serve as water towers, fiber sources, biodiversity reserves, and esthetic refugia for the nation. The lands they administer, and programs and missions for which they are responsible are at stake, making their involvement in CCSP even more urgent. Although climate change science may traditionally have been dominated by NOAA, USGS, and NASA, the important roles for ecosystems (water, fiber, wildfire), decision support, regional downscaling, and integrated assessments make it necessary to encourage active participation by these land-managing agencies and their scientists.
Hilda Diaz-Soltero, Director, USDA Forest Service, PSW Research Station.

DOE, DUBEY

Overall this is a high-level distillation of the NAS report, IPCC findings with an emphasis and focus of highlighting the "uncertainties" and further R&D. This should keep us researchers busy and productive and we will learn much more about how our planet and its ecosystems functions and respond to natural and anthropogenic forcings. However, given the complexity of the Earth System, it is unclear that at what level of certainty or detection a recommendation for action will occur. Clearly air-quality and stratospheric ozone had definite scientific connections and direct links to human health that provoked action. I hope by linking these issues together CCSP can help develop similar thresholds for action to mitigate any potential climate change and perhaps prioritizing action by beginning to tackle soot, methane in the near term.

I see a strong coupling between the issues in the Chapters and these couplings should be stressed.

DUBEY, MANVENDRA, LOS ALAMOS LABORATORY

Overall this is a high-level distillation of the NAS report, IPCC findings with an emphasis and focus of highlighting the "uncertainties" and further R&D. This should keep us researchers busy and productive and we will learn much more about how our planet and its ecosystems functions and respond to natural and anthropogenic forcings. However, given the complexity of the Earth System, it is unclear that at what level of certainty or detection a recommendation for action will occur. Clearly air-quality and stratospheric ozone had definite scientific connections and direct links to human health that provoked action. I hope by linking these issues together CCSP can help develop similar thresholds for action to mitigate any potential climate change and perhaps prioritizing action by beginning to tackle soot, methane in the near term.
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EARTH CLIMATE RESEARCH INSTITUTE (ADDRESSED AS A LETTER TO JIM MAHONEY)

I enjoyed seeing you at your December 3rd to 5th Global Climate Change Conference in Washington, DC. Your willingness to consider all comments was refreshing.

As you know, we firmly believe there is an identifiable hydrothermal/geophysical link (which precedes El Nino) that is a primary driver of climate change. Our evidence is compelling and grows stronger everyday. We respectfully submit that the peer-reviewed publications[1] provided you in our May of 2002 meeting (together with other compelling evidence) demonstrate the existence of this driver.

This natural driver is confirmed by several other lines of evidence, including satellite temperature data, which shows surface warming, but plainly does not show Tropospheric warming. The lack of Tropospheric warming (required under the anthropogenic model) tends to confirm that observed surface warming must be derived from another source – namely, the earth, itself.

We respectfully submit your proposed plan (in its current form) is deficient, because it does not take into account the aforementioned geophysical phenomenon, this natural driver and it's various aspects, which materially impact climate.

For example, your plan does not address the correlation between episodic ocean generated contributions to atmospheric CO2 occurring simultaneously to these geophysical events. This ocean generated CO2 appears to be escalating to levels, which will soon dwarf anthropogenic contributions.

Your plan also does not address the link between anomalous surface volcanism (for example, Mt. Pinatuba's 1991 eruption and present day record setting world wide eruption activity) with nearly simultaneous reductions in atmospheric CO2.

Nor does you plan empower USGS or the Department of Interior, who have the tools, to investigate the aforementioned phenomena.

Due the sheer intensity of this geophysical driver and evidence that its magnitude is increasing, we believe its existence will be soon be quite obvious to the world's scientists. For example, evidence now suggests likelihood of near term ocean temperatures escalation of an order far beyond anything that could be possibly explained by anthropogenic forcings.

Thus, we respectfully encourage you to modify your plan's overwhelming anthropogenic bias (and funding thereof) to a plan that diligently investigates this new body of science.

[1] **"Seismicity of the East Pacific Rise, Correlations with the Southern Oscillation Index?"** Walker, EOS, Sept 20, 1988; **"More Evidence Indicates Link Between El Nino's and Seismicity,"** Walker, EOS Transactions, January 25, 1995; **"Seismic Predictors of El Nino Revisited,"** Walker, EOS June 22, 1999. **Sea-Floor Hydrothermal Activity Links Climate to Tectonics: The Eocene Carbon Dioxide Greenhouse**, Robert M. Owen, David K. Rea, Science, Vol. 227, 1/11/1985, 166/167.

Certainly, the sooner we can appreciate the reality of our circumstances -- the better. And, most certainly, the US government should not commit to any mitigation, sequestration, adaptation or other plan (including carbon trading or Kyoto) if the underlying climate science is incomplete or fundamentally flawed. Such action would likely have significant unintended consequences, not to mention unnecessary economic hardship.

We respectfully submit that your present plan lacks the prerequisite "open mind" needed to investigate a scientific finding/departure, like ours, which may better explain observed climate change.

Thank you for the opportunity to provide these comments. I look forward to your response and to working with you in the days ahead.

Earth Institute, Sachs

On behalf of the Earth Institute at Columbia University, I am pleased to present some overall comments on the draft Strategic Plan for the Climate Change Science Program. By way of introduction, the Earth Institute is dedicated to the integrated study of Earth, its environment, and society. The Earth Institute builds upon excellence in its core disciplines—earth sciences, biological sciences, engineering sciences, social sciences, and health sciences—and stresses cross-disciplinary approaches to complex problems such as climate change science and policy. Through its research, training and global partnerships, it mobilizes science and technology to advance sustainable development, while placing special emphasis on the needs of the world's poor. Given the breadth of this mission and the world-class group of faculty and researchers working to achieve it, the Earth Institute is well poised to provide some key insights on many aspects of the Strategic Plan for Climate Change Science. Following my general comments below, which focus primarily on the non-USGCRP chapters, are specific comments made by our leading scientists on most chapters of this plan.

Given its complexity and its importance, climate policy clearly must draw on a sophisticated understanding of how the climate system works, but equally on how changes in climate influence human societies, directly or indirectly, and on the options for adaptation to change and reduction of impacts. The Earth Institute's comments focus on better addressing several fundamental issues that we believe will provide a more effective Strategic Plan for the Climate Change Science Program.

- The US should proceed now, together with other countries, to formulate a climate policy even in the face of our current level of uncertainty, and cannot rely solely on advancing our scientific understanding of climate as the key to a secure framework for policy development.
- A key part of that policy should include research and development on ways to mitigate the impacts of fossil fuel use, including carbon capture and storage technologies. We strongly urge, therefore, that the Strategic Plan for Climate Change Science be quickly complemented by the still-awaited Climate Change Technology Initiative.

- We must focus our attention much more on the possibilities of abrupt climate change, those large and rapid changes that we now know can occur on timescales of several decades and which constitute the greatest risks ahead. A sound understanding of these risks will also provide key insights as to many aspects of the climate system.
- We must engage in a much more aggressive effort with developing countries to advance climate change science and to formulate international climate change policy. Small climate changes can have large impacts in many of the poorest regions of the world, where adaptation options are limited and the margins of survival are already very narrow. We must not lose sight of the fact that climate change may be the consequence of activities in developed countries where adaptations can be achieved, but have significant impacts on vulnerable countries facing extreme risks and with minimal opportunities for adaptation.

As the world's leader in climate research, the US carries a responsibility to allocate our research funds and effort so that our fundamental climate science advances in concert with our need to make national policy and to participate in multilateral efforts that reflect the global nature of the climate system. The *Strategic Plan for the Climate Change Science Program*, suitably modified, can provide an important road map for developing our understanding of climate systems in order to develop appropriate climate-related policies. Our comments reflect our deep desire to work with our colleagues in government, academia, and internationally to achieve this goal.

1. Issue of Uncertainty

The issue of uncertainty raised in the draft plan and specifically addressed in Chapter 4 should be clarified. My colleague, Professor Elke Weber, Department of Psychology and the Business School, has summarized matters quite well: "Reduction of uncertainties via increased scientific understanding of socioeconomic and environmental systems is, of course, desirable, but uncertainty about many key decision variables will remain, including seasonal to inter-annual climate variability. *Uncertainty reduction is neither necessary nor sufficient for informed policy debate and decision making*" (emphasis added). The key issue here is that there is and will always be important uncertainties in predictions about the future of the climate. These uncertainties arise in part from the very nature of climate system dynamics and some of these uncertainties are fundamentally irreducible. We need to understand where the uncertainties arise, quantify them if possible, but most important of all we must learn how to make rational decisions given the existence of uncertainties. We urge a rearticulation in the draft plan of the role of uncertainty and how policy makers must incorporate an understanding of the nature of climate and climate impact uncertainties in formulating climate policies. Professor Weber provides more specific comments on this issue in her comments for Chapter 4.

2. Data Monitoring and Management

The issue of uncertainty is also present in Chapter 3 on Climate Quality, Observations, Monitoring, and Data Management. I would like to highlight the observation of my colleague, Dr. Roberta B. Miller's, Director of CIESIN, that the strategic plan should broaden the types of data that are needed. The strategic plan would greatly benefit from a more thorough discussion of the *socioeconomic, institutional, and behavioral data* that are also needed to help us to better understand the impacts of climate variability and change. There is, in fact, quite a lot to be learned from studying how society has been impacted by, and has already adapted to climate changes that have already taken place. The report must emphasize that prediction of future climate conditions must begin with a thorough description of the current state of the climate and its past behavior. Dr. Miller elaborates on this in her comments for Chapter 3.

3. Carbon sequestration and link to Climate Change Technology Initiative

While the report examines the very important issue of natural carbon sinks in the oceans and terrestrial biosphere we believe it is critical that emphasis be placed on the fact that natural systems, even enhanced through interventions, will not have the capacity to mitigate the effects of a doubling of atmospheric concentrations. We must understand what the natural system can provide as sinks for carbon but at the same time we must begin to seriously investigate a wide variety of *technological options for sequestration* that include capture from power plant emissions and direct carbon capture from the air. These technologies may take decades to bring on line after proof of concept and may fall under the umbrella of the Climate Change Technology Initiative, which we very much hope will make its recommendations later this year. We emphasize that there is a need to interrelate research on the capacity of natural systems to sequester carbon with technological developments and policy imperatives that will be required to manage the carbon excess that natural systems will be unable to sequester. My colleague, Professor Klaus Lackner, Department of Earth and Environmental Engineering, provides more specific information on these points in his comments on Chapter 9. Professor Lackner has some visionary ideas for managing the carbon fluxes and disposing of excess carbon and for alternative strategies for carbon free energy. I strongly advocate that we address these topics in the both the strategic plan for the Climate Change Science Program and as well as in the Climate Change Technology Initiative.

4. Learning from Seasonal to Inter-Annual Climate Variability

Natural variations in temperature and precipitation, such as El Niño, that occur on seasonal to inter-annual time scales together with their associated global impacts represent changes with a magnitude and scope that equal or exceed any changes anticipated by climate change during this century. The study of seasonal to inter-annual climate variability and its impacts and adaptation strategies provides a fundamental learning experience to enhance our current understanding of the impacts and appropriate adaptation strategies for longer-term climate change. My colleagues at the International Research Institute for Climate Prediction (IRI) based at Columbia University have been engaged in these activities for more than five years and they provide specific suggestions on how their experiences might be translated to the time scales and particular issues of long-term climate change. For example, through success in using shorter-term climate forecasts by policy makers and other stakeholders, we will learn more about the policy challenges and use challenges we will likely face with climate change forecasts on the scale of decades to centuries. The IRI working with national and international partners has already increased our capacity in this area and is poised to take a leadership role in continuing to build it worldwide.

5. Regional Models/Downscaling

A key learning experience that has already come out of the study of shorter-term climate variations and their impacts is the importance of regional models. While the climate is a global system connected through the oceans and the atmosphere at various time scales, the use of climate information for decision making requires knowledge of the climate in specific regions. All impacts can be thought of as local phenomena. The creation of models that are appropriate at regional and local scales should be emphasized as a critical component of planning for the future, not just for the US but also for critical areas of the world where impacts are likely to be significant.

6. Water Cycle

The report appropriately emphasizes the critical role that water plays in all aspects of society and the potential for disruption that climate change could cause for the water cycle. What could be emphasized is the complexity of water systems themselves, which display trends, oscillations, and regime changes in floods, droughts, seasonality of flow, ground water tables, water quality parameters, and their spatial organization, that are modulated by a variety of forces of which climate is only one. There is an urgent need to understand these variations (perhaps in an independent program of research) and to explore the interactions between these changes and those identified in climatic parameters. We know already that these relationships are likely to be complex and non-linear and that practices based on past assumptions about the behavior of the system - the 100-year flood for instance - are grossly inadequate for water resources management needs of the future.

7. Reporting and Outreach

We suggest that the draft plan include more feedback coming from stakeholders to the scientific community. Reporting and outreach activities will be most effective if they are designed with the recognition that both producers and consumers of scientific information have specific approaches to problem solving.

8. International Research and Cooperation

We commend the draft plan in recognizing the need to include more broadly scientists from developing countries in the many international research programs mentioned in this report. It is essential that much more attention be given to the capacity building needs of our colleagues in these regions of the world in order that we can better address the many dimensions of climate change science and its impacts. There is a growing awareness in the world community concerned with sustainable development that adverse climate impacts are a major inhibitor to poverty alleviation in its many dimensions including food security and control of infectious diseases.

Advancing our understanding of climate systems combined with an understanding of how this information can be used for impact reduction in parts of the world very different from our own has become a critical tool for sustainable development. I think that this section could benefit by identifying and supporting more intensively those research programs such as the Consultative Group on International Agricultural Research (CGIAR) and the IRI, which are working to improve the lives of the poorest through collaborative scientific advances. The Strategic Plan should articulate how we can expand and coordinate such programs to advance climate change research, to better understand the likely impacts of climate change, and to formulate policies that address these impacts from local to international levels.

The draft plan would also benefit from including a detailed section on what is going to emerge as a major global issue this century—the fact that the poorest of the poor, and therefore the most vulnerable, in the world are likely to suffer disproportionately from the consequences of climate change. We strongly believe that this challenging issue is one that we must start to address now. The Earth Institute at Columbia is taking on this challenge with programs specifically designed to generate and apply our best scientific understanding to the challenges of sustainable development facing the poorest peoples in the world.

EDISON ELECTRIC INSTITUTE (HOLDSWORTH)

The Edison Electric Institute (EEI) appreciates the significant efforts undertaken by the Administration in developing the November 11, 2002, draft "Strategic Plan for the Climate Change Science Program." The draft plan is a "vehicle to facilitate comments and suggestions" on the proposed "climate and global change" research needs of the U.S. by stakeholders, such as EEI, scientists and others who attended the Program's three-day workshop last month. We also appreciate the opportunity to review the four White Papers prepared in support of several chapters of the draft plan posted on the Web on November 26 and 27, 2002.

EEI is the association of our nation's shareholder-owned electric utilities and industry affiliates worldwide, with 200 member companies in the United States serving more than 90 percent of all customers served by the shareholder segment of our industry and 48 affiliate members in 17 countries. We have a long history of participation in global climate matters, including the development of the several assessment reports of the Intergovernmental Panel on Climate Change (IPCC) that relies heavily on research results from the U.S. and elsewhere, and the development and continuing efforts to implement the Framework Convention on Climate Change (FCCC), such as occurred last fall at the FCCC's eighth session of the Conference of the Parties (COP-8) in New Delhi, India.

COP-8 adopted conclusions that are relevant to the draft U.S. plan on the importance of an integrated international effort on research and systematic observation areas of need. COP-8 also adopted, with

U.S. backing, the Delhi Declaration on Climate Change and Sustainable Development. It emphasized, among other things, that adaptation to the "adverse effects of climate change is of high priority for all countries," as well as the promotion of "sustainable development." The Declaration added, "Policies and measures to protect the climate system against human-induced change should be appropriate for the specific conditions of each [FCCC] Party and should be integrated with national development programmes taking into account that economic development is essential for adopting measures to address climate change."

Last February President Bush established the Climate Change Science Program (CCSP) to coordinate and direct research efforts of climate and global change. The CCSP is to report to an interagency group that in turn reports to the Cabinet-level Committee on Climate Change Science and Technology Integration (also established by the President last February). The CCSP includes the U.S. Global Change Research Program (USGCRP) authorized by the Global Change Research Act of 1990 (15 U.S.C. sec. 2921 *et seq.*) and the Climate Change Research Initiative (CCRI) announced in June 2001 by the President. We note that Part I of the draft strategic plan, which was prepared by several federal agencies of CCSP, relates to the CCRI; Part II, to the USGCRP; and Part III, to communication, cooperation and management.

Clearly, a strong near- and long-term research program that addresses the significant areas of outstanding uncertainties in the understanding of human-induced – as opposed to naturally occurring – climate change is a key element in the development of future policies and measures by both the public and private sectors. We welcome the efforts of the Administration to structure, improve and accelerate that research.

However, we are concerned that despite the June 11, 2001, directive of the President that the Secretary of Commerce "set priorities for additional investments in climate change research," the draft plan does not specify priorities for the research identified therein. All of the research appears to have the same importance or urgency even though it would seem that some of the research areas should clearly precede others in order to be effective and timely.

We are also concerned about establishing time frames of 2-4 years, particularly without also establishing priorities, for all of the CCRI research areas and for some of the USGCRP research areas. While we recognize the need to demonstrate progress and to keep pressure on the researchers and the sponsoring agencies, the workshop showed that such times frames are likely to be unrealistic and disappointing. We believe a milestone approach would be a better way forward in achieving the President's desire to "increase our knowledge" and to be "creative" and "flexible."

Based on our background and experience, EEI takes the opportunity to comment in more detail on this important and helpful draft strategy document. Our detailed comments are enclosed in accordance with the CCSP "Format for Comments" guidance.

THE ENVIRONMENT AND ENERGY STUDY INSTITUTE (EESI)

First Overview Comment: The Environment and Energy Study Institute (EESI) wishes to congratulate the U.S. Climate Change Science Program for producing an excellent discussion draft for this critical effort and conducting a stimulating workshop.
Carol Werner, Penny Hanson, EESI

Second Overview Comment: EESI strongly endorses the introductory remarks made by Assistant Secretary James R. Mahoney at the recently conducted U.S. Climate Change Science Program Planning Workshop. Dr. Mahoney stated that both the workshop and the Strategic Plan were intended to contain two overarching themes: First, that the subject at hand, global climate change, was the "capstone issue" for our generation and would be by far the most critical environmental and economic issue for the new century; Second, that the most important goal for the U.S. program must be to "accelerate existing research into active response." Expanding on the second point, he further stated that to achieve that goal would require new energy systems "dimly seen today." The Strategy, he concluded, must focus on "transitions to application" and be, most fundamentally, a "strategy for future action" (emphasis by the speaker).

We concur with the above statements, but are concerned by the seeming disconnect between them and the goals and research questions contained within the draft Strategic Plan. The Strategy places heavy emphasis on the study and modeling of climatic, terrestrial, oceanic, social, economic, and political phenomena. As stated by numerous participants at the workshop, however, some of the research listed for effort has already been conducted, is occurring now, or may be of marginal importance. While refinements and expansions on existing work can always provide additional value on matters of scientific inquiry, EESI believes that the government's Strategic Plan must focus upon the most critical of existing questions and establish clear priorities on the questions it will seek to answer, the relative resources it will put toward those questions, and the sequence or timing of its activities in each area. Prioritization is critical on all major public policy issues because there are rarely resources or time adequate to do everything one might wish. Due to its scope and complexity, this will certainly be the case with global climate change. The Plan as it is written is a comprehensive (with one major exception discussed below) compendium of questions on numerous climate change issues of greater or lesser importance, but it is not now a strategic plan, because it does not prioritize activities so as to fulfill Dr. Mahoney's stated goal of "accelerating research into active response."
Carol Werner, Penny Hanson, EESI

Third Overview Comment: Our second general concern with the Strategic Plan also directly relates to the lack of forward motion contained in the document. No area of research can be more critical to the ultimate future of the earth's climate and inhabitants than that of the array and impacts of alternative technological and policy responses to climate change. We are aware that the Department of Energy is charged with managing the technological research program under the President's Climate Change Science Program. We do not believe that this mandate means that the evaluation of the impacts of technological alternatives should be conducted separately from the bulk of the research effort being conducted under this Strategic Plan. Indeed, integration of mitigation

scenarios based on various combinations of technological and policy alternatives will be critical to all policy option discussions in the future. If they are not constructed and analyzed under the same research umbrella from the beginning, they will inevitably be incompatible, or at least difficult to integrate, in the end. Further, we do not believe that too little is known about the efficacy or acceptability of the numerous technical innovations now or soon to be available to prevent the incorporation of this kind of analysis into the core research program. While wholly new inventions may play some role in the future, it is very likely that almost all of the feasible technical and conservation alternatives to impact climate change have been identified, if not optimized, today. Assumptions about the scope, applicability, and costs of these alternatives can be made and impacts postulated on all of the areas to be studied under the Strategy. This work is critical if we are to avoid the major "unintended consequences" that so often accompany technological and social change. We believe that a reasonable range of world wide response scenarios can and must be constructed and built into every part of the research conducted under the Strategic Plan. Without such integrated analysis, Dr. Mahoney's goal for a "transition to application" and "strategy for future action" will not be achieved.
Carol Werner, Penny Hanson, EESI

FISHER, PENN STATE UNIVERSITY

This draft plan has many good features that provide a starting point for the next decade's research on global environmental and climate change, so that the results will be science in the service of society, with the questions explored being policy relevant (but not policy driven). There is a good emphasis on making the results useful and useable. My comments are intended to be constructive, to improve the *Strategic Plan*.

Although the draft will benefit from more explicit integration across chapters and sections (perhaps even combining some of them), using a standard outline (as has been done) makes it easier for the reader to compare across chapters.

A strong point is the careful attempt to avoid or define jargon (e.g., fluxes, p. 10; radiation, p. 17; polarimetric, p. 18; parameterized, p. 21; albedo, p. 22. Another reading by a non-specialist would add to the list here of words still needing definitions: radiative forcing, paleoclimate, tropopause (where first used on p. 32 rather than where it is first defined on p. 62), model new reanalysis projects, p. 32; inverse (as used on p. 102).

Overall each chapter's list of overarching questions is good. Some chapters list specific products and payoffs, often linked to a time frame. Although the timing is optimistic for some of these, it is a good idea to indicate which ones can be delivered reasonably soon and which ones are likely to take much longer. Of course, determining the realism of delivery within the posited time period is impossible without some notion of the level of resources the investigators would have to work with.

It is laudable that this draft explicitly includes human dimensions, which allows better projections for important "If,...then,..." questions, as well as insights for the "So what?" and "What are we going to do about it" questions. Also laudable is the explicit inclusion of changes in land use and land cover; activities for this topic should be linked especially closely with those listed under the human dimensions chapter. These two points reinforce the "regional, regional, regional" emphasis Jim Mahoney accepted at the end of the Dec. 3-5 workshop.

Although p. 165 gives a brief list, more is needed about how research priorities will be set across chapters as well as within chapters. This could be linked (given that this is to be a multi-year strategic plan) to criteria about when and how the science/research managers will know that enough has been learned about a particular research topic–even if what is learned is that a specified type of uncertainty is unlikely to be reduced through research in (say) the next decade–so that resources can be reduced for this research topic for allocation to another high-priority topic. An example is the Chapter 3 call for more monitoring and repairing of existing monitoring networks. When will we have enough (of specified categories of) monitoring data? Will value-of-information criteria be used in these determinations? It is important to note that even when there are substantial uncertainties, we often know enough to support a particular decision. An analogy is HIV/AIDS. Even though we still lack full understanding of just how these threats "work," we know enough to promote "safe sex" as a way to reduce the risk of AIDS transmission. A related point is that decisions WILL be made (that affect vulnerability to global change); the question is whether we prefer using admittedly uncertain information–with caveats about what we know and what we don't know–as part of the input to the decision, rather than having the decision be made without the benefit of what the research has revealed.

Strong reliance on National Research Council reports is laudable. However, the draft ignores much of the extant literature, such as the many peer-reviewed professional journal articles that were products of the first US National Assessment. It would be an inefficient waste of taxpayer money not to benefit from the full range of national and international research related to CCSP topics and goals.

Throughout, there could be more clarity about what the relative emphases are between US versus global aspects, and global climate change versus global environmental change (for causes, impacts, and actions).

The chapters all seem to have "the right words" about collaborating with other CCSP programs, agencies, and international groups, but little on just how and for what. This will require an appropriate incentive structure and careful management.

Chapter 4 and Chapter 13 (and elsewhere, such as Question 5 for Chapter 6): Evaluation of the decision support tools and outreach is crucial for meeting the CCSP goals. However, such evaluation is a research activity in its own right. As such, it has its own challenges. For instance, it can be quite difficult to measure the extent to which providing information actually affects a particular decision, given all the other decision factors and constraints the typical decision maker faces. Another caveat is that the evaluation question must be formulated appropriately. For instance, providing information as input for informed decisions could lead to choices that are inconsistent with the evaluator's preferences/value structure. The decision maker might weigh the information carefully but then conclude that the extra (monetary and non-monetary) costs of a proposed action (say, to improve resiliency to impacts from climate change) are higher than the foregone (monetary and non-monetary) benefits if the action is NOT taken. Thus if the evaluation question is "Did the decision maker use the information?" the answer is "Yes," but if the question is "Did the decision maker choose the action the researcher thought was implied by the information?" the answer might be "No."

GATES, W. LAWRENCE, LAWRENCE LIVERMORE NATIONAL LABORATORY

First Overview Comment: Although the importance of international cooperation in climate research is recognized, this chapter does not adequately describe the many internationally-coordinated climate research activities that are in place and which are critical to the success of many U.S. efforts. However, in reading other chapters of the draft, I found the appropriate descriptions in the "key linkages" section of specific research areas, a summary of which is given on p.57. I suggest that an overview of these linkages be given in Chapter 14.

GENERAL MOTORS (GEORGE WOLFF)

First Overview Comment: While the report is certainly a comprehensive compilation of the research needs for climate change science, it is missing some of the elements of an effective strategic plan. First, especially for the Climate Change Research Initiative (CCRI) portion of the plan, it is not clear that there are sufficient time or resources to address all of the tasks listed. They need to be prioritized and the criteria used to prioritize them need to be clearly articulated. Second, firmer timetables should be given for each task, and third, the Agency responsible for its execution identified. Fourth, the final product should be identified.

Second Overview Comment: There does not appear to be a comprehensive program to increase our knowledge about the role of the oceans. A number of tasks are mentioned but they are scattered among different programs, and they do not include all the necessary elements. The report should list and integrate this information in one section.

Third Overview Comment: There is no mention in the plan of the various solar forcing hypotheses. Recent developments certainly warrant effort in this area. For example see Carslaw, K., Harrison, R. and Kirkby, J. in *Science,* vol. 298, pp. 1732-1737, November 2002.

GREEN, FRASER INSTITUTE

First Overview Comment: Overall, the focus of the draft report – on reducing the uncertainties that permeate climate science – is well placed. We know that interceptive actions are likely to fail (and waste resources) in conditions of high uncertainty (See Aaron Wildavsky, "Searching for Safety," Transaction Publishers, 1991). Hence, managing the risk of climate change (whether anthropogenic or not) is best accomplished through strengthening our understanding of basic climate processes, and investigation of no-regrets policy options that provide immediate benefits, while producing a reduction in

greenhouse gas emissions as a by-product.
Reviewer's name, affiliation: Kenneth Green, Fraser Institute

Second Overview Comment: As numerous observers have pointed out, the story-line/scenario-based modeling approach used by the IPCC to forecast potential climate changes is rife with flaws, is overwhelmingly subjective, and lends itself to the generation of implausible worst-case scenarios that distort the policy formation process dramatically. (See for example, Ross McKitrick, Fraser Forum, January 2003, www.fraserinstitute.ca) If-then modeling should be based on stipulated, but realistic ranges of greenhouse forcings, without implications that any one scenario represents some "plausible" vision of the future.
Reviewer's name, affiliation: Kenneth Green, Fraser Institute

Third Overview Comment: Throughout the climate change research endeavor, there is a need for data quality validation measures beyond what is currently used. Modeling assumptions are held specific to individual models and modelers, without a robust external review process to determine whether or not those assumptions are robust. A part of any research agenda should include the development of a mechanism for a holistic review of data quality, and assumption validity.
Reviewer's name, affiliation: Kenneth Green, Fraser Institute

Fourth Overview Comment: Throughout the report, greater emphasis needs to be made about the purpose of climate change research, which is to study how to reduce the risk of rapid climate change without regard to its origin. An emphasis needs to be placed on the understanding that resources focused only on remediating the impacts of alleged anthropogenic climate change will leave areas of research regarding adaptation and natural climate variability underemphasized.
Reviewer's name, affiliation: Kenneth Green, Fraser Institute

Fifth Overview Comment: The report lacks emphasis on priorities: First priority should be to establish meaningful oversight, data quality, and modeling validation specifications to prevent misuse of knowledge developed from climate study efforts. Second priority should focus on the development of knowledge that is useable, immediately, in reducing harms due to climate fluctuations. (A good test of such knowledge is to see if businesses such as the insurance industry is willing to pay for access to the data)

GUPTA, GSFC NASA/GEST UMBC

First review comments relavent to many chapters (2, 4, 5, 13, 14) and overall strategy to address issues of Climate Change:

Given the short-term (2-4 years) objectives of Climate Change Research Initiatives (and even for USGCRP) as stated under CCSP, I think the whole strategic plan is very badly missing an important component on the emissions of various greenhouse gases (uncertainty in the inventories of their regional source strengths from various categories of emissions, their relative contributions to regional and global radiative forcings, and expected future changes in both) and their related economics. Robust knowledge of emission inventories and reduction in associated uncertainties are crucial for all stages of climate change science and corresponding policies and regulations e.g. development of 'if-then' scenarios for decision-making process, policy development and its implementation, meaningful validation and interpretation of model and observational data, and finally, development/transfer of advance technologies.

Obviously, while models are being developed to be more robust and meaningful, observations are being collected to make better comparison and interpretations, in the end, similar to the situations of air quality issues, it will be the emissions (both anthropogenic and human-induced natural) that need to be stabilized and/or reduced to address the issues of climate change while keeping economics in the perspective.

Given the US Administration's view on the reduction in emissions of greenhouse gases in terms of emission intensity (given in terms of ratio of emissions and economic output), it will be very advantageous and may be a first step to tag each sectoral and regional emissions of individual greenhouse gases and associated air pollutants in terms of the corresponding productivity, economical input/output and radiative forcing not only for the US but also for the entire world. Based on the 'relative' analysis of this type of tagged-emission data would help to identify several key points:

1. Which greenhouse gas (GHG) (long-lived and/or short-lived and associated precursors) needs immediate attention?

2. Which industrial sector of that GHG needs immediate new technology development and/or replacement by alternative technology?

3. Which country and its which particular industrial sector need transfer and/or development of technology?

4. What are the relative investments of technological development and use of present technology per unit release of emissions of various pollutants?

5. What are the unit radiative forcings and unit economical output of each regional and sectoral emission, and how these indicators would change in the future?

I think these are some of the points that will identify the need of actions to be taken at least in the short-time horizon, not only in the US but all around the world.

In general, the starting point of climate change really originates from the issues of air quality which is more at the urban/regional scales, particularly for short-lived and very important greenhouse gases: ozone and aerosols. Both of these gases are important components of National Ambient Air Quality Standards, and regional haze and visibility. Therefore, targeting the science and emission control of these gases on all spatial scales must be an obvious strategy.

HANSON, HOWARD, LOS ALAMOS NATIONAL LABORATORY

Since last June, I've had several conversations with Jim Mahoney about how I might be able to contribute to the CCSPO effort. As time passed and my circumstances at Los Alamos evolved, I found that it would not be possible to join the Office in a full-time role, but Jim suggested that I could play a useful role in other ways. Providing an assessment of this Draft Plan was one of those. Accordingly, I have worked my way through the entire document with an eye toward making it more internally self-consistent and readable. With the exception of a few minor suggestions, I have not made any attempts to address the scientific content of the draft—I expect that will happen, extensively, at the December workshop, and I'm looking forward to the discussions there.

I must say at the outset that this Draft Plan is an impressive document, remarkably well written and integrated. I'm quite impressed with the overall quality, particularly its lack of the usual typos. Given the time available for its assembly, this is an effort for the CCSPO to be proud of.

It appears to me, because of the way that the instructions for making comments are organized, that separate individuals are responsible for the various chapters of the document. Some of the comments here will be useful to them. However, others need to be seen by the "editor-in-chief," because they pertain to cross-chapter consistency and other "whole document" issues. Therefore, while I have organized the comments below to comply with the suggested format, some of them apply to the entire document; and there is cross-referencing in some cases.

There are essentially three sorts of comments here, and they're labeled. **Purely editorial (E)** comments are simply those associated with readability. **Political correctness (P)** comments are associated with wording that may be inflammatory or is otherwise not really needed to communicate the message. And **substantive (S)** comments relate to the actual scientific content of the document. (If I were "editor-in-chief," I would just go ahead and make the Editorial changes; and I would discuss the Political-correctness changes with someone and take the Substantive changes under advisement pending receipt of additional comments from other people.)

I have some overview comments on the document generally; this is followed below by specific comments on the chapters.

(I-S) The distinction made throughout the strategic plan between "predictions" and "projections" is a wonderful idea, and several specific comments in the chapter sections below are designed to highlight this, to make it more understandable, and to build its case more strongly.

In the numbering below, comments 3, 4, 5, 7, 13, 21 relate to this.

(II-E) There appear, however, to be some organizational difficulties with the overall draft. While most of the material is organized by the chapter numbers, the two parts (I – CCRI and II – USGCRP) sort of appear out of nowhere and the introductory sections to those headings don't seem to fall into chapters. In addition, there is confusion about CCSP and USGCRP throughout the USGCRP Part II (and considerable redundancy, as specified below). In my lexicon, this falls into my E(ditorial) category of comments and can be fixed by a good technical editor.

From the perspective of scientific content, the draft is right on, in my opinion.

General Comments

(III-S) Of course, there are always ways to improve a document. From the perspective of the science in this one, I'd like to suggest additions (along with the specifics below). The "feedback" box (or sidebar) on Page 114 provides a vehicle for this. How about (at least) two more sidebars, one on predictability (in the technical sense of nonlinear dynamics and chaos) and one on scale mismatches associated with small-scale heteorogeneity? Both of these concepts are implicit throughout the discussions here, but neither is defined very well. The latter, scale mismatches, would naturally fall into the land-use chapter (it also applies to ecosystems, but land-use is first here). The predictability explanation is less obvious to place, but perhaps in Chapter 6 would be appropriate, or perhaps sooner, in the Applied Climate Modeling section of Chapter 4.
Further, in this same context, I would urge consideration of a sidebar in Chapter 1 to discuss the use "projections" as a way to consolidate comments below.

Submission Update – January 17
This is the third submission I've made to this forum—the first, made last November, I believe, was a comprehensive editorial scrub of the CCSP Draft Plan. The second, made during the December Workshop, was an overall mission statement, of sorts, for the CCSP—this was motivated by comments made during the workshop. The sound-bite version of this mission statement was "quantifying and elucidating the nature of uncertainty in climate projections." (Importantly and intentionally, this did *not* include "reducing uncertainty.")

This third submission is motivated by an editorial article that has recently appeared, "Wanted: Scientific Leadership on Climate" by Roger Pielke, Jr. and Daniel Sarewitz (*Issues in Science and Technology*, **19**(2) [Winter 2002-03] pp. 27-30). I believe that the discussion in that article underscores the relevance of my suggestion of elucidating and quantifying uncertainty as a focus for the CCSP. Further, Pielke and Sarewitz make the point, far more eloquently that I could, that uncertainty *reduction* is a red-herring goal that will only continue to serve the climate science community's self-interest by prolonging research indefinitely instead of leading to the hard decisions needed to take action. This—the notion that we will always have to live with some level of uncertainty, so why not just get on with it?—is the reason that I intentionally omitted uncertainty reduction from my suggestion in December.

While I do not agree with all of the arguments in Pielke and Sarewitz, I believe that their basic assertion—that promises of uncertainty reduction as a scientific basis for action have been shown to be hollow—is valid. I would suggest that further travel along this road will only hurt the credibility of the climate science community. By shifting our focus to understanding the nature of uncertainty and quantifying it, we will begin to provide the policy making community with information that they can assimilate with economic and other factors as they make decisions. In this way, the CCSP is in a position to take the leadership role that Pielke and Sarewitz seek.

A final comment: In the process of quantifying and elucidating the nature of uncertainty in climate projections, there undoubtedly will be the bonus of uncertainty reduction as a side-effect. My point here is that a focus on uncertainty reduction is not the appropriate mission for the CCSP.

HANKIN, CHAIRMAN, DATA MANAGEMENT AND COMMUNICATIONS STEERING COMMITTEE, US INTEGRATED OCEAN OBSERVING SYSTEM

The Strategic Plan for the Climate Change Science Program should include an explicit and highly visible discussion of the infrastructure needed to support climate science research. This might take the form of an Infrastructure chapter within Part III or of sub-sections on infrastructure needs within existing chapters. In addition to considering data management the infrastructure discussions would presumably pull from the current sections on the observing system and tools for decision support. The need for a standards process (see preceding discussions in this email) should be explored and a plan developed for defining such a process -- in cooperation with the IOOS effort and possibly with input from NIST.

HANSON, PAUL, OAK RIDGE NATIONAL LABORATORY

1. I support the concept of an accelerated scientific program to reduce key uncertainties in climate science. Our confidence in and disagreements about projections of the impacts of climate change often stem from the wide range of climate scenarios and predictions that are on the table. Accelerated work to improve future projections of climate scenarios is a critical first step. Such activities, of course, must be based on a fundamental understanding of atmospheric warming, terrestrial controls on water, carbon, and energy cycles, and the feedbacks and/or interactions between them. With constrained projections in hand, the projected responses of ecosystems to environmental change from various groups should be less diverse. Constrained scenarios will also foster the planning of efficient experiments focusing on key areas of uncertainty.

2. While the draft report underscores the importance of the carbon cycle, it does not recognize the critical role of soils and their carbon stocks. More emphasis needs to be placed on the need to understand the pools of soil carbon that are available for near-term exchange with the atmosphere. Recent research has shown that much of the large soil carbon pool is not subject to rapid change.

3. Page 10 of the document suggests that the CCSP strives to investigate 'a targeted yet comprehensive set of questions'. This statement seems contradictory. My reading of the full report (Parts I CCRI and II USGCRP) does not reveal prioritized targets. That is, everything seems to be critical. Perhaps the report could point to areas of research that were considered less important as an indication of what was not targeted.

4. The guiding principles of the CCSP on page 11 are well stated and I support them. The 'If-Then' analyses are appropriate. Assuming that the 'If' scenarios can be defined and agreed to, I believe that lucid and robust projections of impacts will follow.

HOYT, RETIRED

My comments are overview comments directed towards research on climate change in general. They briefly highlight several errors in climate modeling and data interpretation that any climate plan needs to address. Any climate plan should direct its efforts towards correcting the following errors in climate science:

1. Radiative forcing error The often quoted 4 W/m2 forcing of climate due to a doubling of carbon dioxide is numerically correct, but it is conceptually wrong to use this number. This number is calculated assuming a doubled carbon dioxide content and unchanged tropospheric temperature profile. Such an atmosphere can never exist nor evolve to that state in the real world. Hence, the associated calculated radiation field and the associated 4 W/m2 difference used for forcing cannot exist. The conceptually correct number is about one third this value. Using the correct approach and number, at least 3 problems will be solved: a) Model overestimation of the global warming trend in the twentieth century, b) model overestimation of the warmth of past climates when carbon dioxide was higher than present, and 3) model overestimation of the temperature of Venus.

2. Water vapor feedback error

Manabe "guessed" that relative humidity would remain constant as temperatures increase. It takes considerable energy to maintain a constant relative humidity and this energy requirement has been assumed incorrectly to be negligible. Consequently modelers are faced with apparently inexplicable results such as the "pan evaporation paradox". A model that is closer to reality is one that has the absolute humidity constant and this approach should be incorporated in the models.

3. Cloud cover feedback error

Most models have decreasing cloud cover which enhances the warming. ISCPP cloud cover observations suggest there is no trend in cloud cover and many regional surface cloud cover observations suggest cloudiness is increasing. Cloud cover is clearly not treated correctly in the models and needs further study. Lindzen has also pointed out that in the tropics that cloud cover changes may be a negative feedback.

4. Carbon dioxide growth rate error

Ian Castles has recently pointed out that the SRES storylines severely overestimate the growth in carbon dioxide for any reasonable economic assumptions. New carbon dioxide predictions are needed coupled with climate model runs that have been corrected for the three errors above.

5. Surface temperature measurement errors

The surface temperature record is contaminated with urban heat island effects and land use change effects. We can safely say this because: a) it is commonly assumed that towns of populations of 1000 or less have no urban heat islands although several studies have shown that towns this small have heat islands on average of about 2 C. Thus, a fundamental assumption in compiling the records is flawed. b) The diurnal temperature range (DTR) has decreased in recent years and decreasing DTR is a classic symptom of urban heat islands. The DTR is decreasing over land at the surface and not over oceans or in the middle atmosphere. The DTR is decreasing in areas where clouds increase or decrease and where aerosols increase or decrease, so no atmospheric changes can account for it. Climate models predict carbon dioxide increases will not effect DTR. Thus, it appears that half of the claimed warming is spurious and due to urban and land cover effects. The problem requires further study by a group of individuals who are not committed global warming advocates. As an added note, the recent report of species movement of 6.1 km/decade is equivalent to a global mean warming of 0.025 C/decade which is consistent with balloon and satellite observations and inconsistent with meteorological surface observations.

The above items are five defects in climate change science that need to be corrected before progress in this field can proceed on a sound basis and thus research on them should be included in any climate change plan.

IRI, ZEBIAK AND STAFF

The mandate of the CCSP requires the adoption of a broad definition of "science." It should aim to not only improve the quality of observation and projection of long-term climate trends, but to also establish innovative ways of capturing the socio-economic value of projections through their successful utilization. While the development of data, information, analytic resources and models to facilitate risk assessment are important, CCSP should also promote their scientific demonstrations in specific settings in order to evaluate their full potential. Integrating science into policy development and operational decision making in pilot demonstrations would be of immense value. The work of the IRI and its partners on managing seasonal to annual climate variability, provides an exceptional opportunity to ground truth many of these issues. The IRI has underway project activities on a number of fronts of critical interest to the CCSP, including: stake holder/scientist fora on uncertainty; joint development of innovative decision tools for effective planning over variable time scales and forcing factors; development of integrated data sets; spatial and temporal downscaling; validation of models; and building capacity to utilize climate information products at key policy and decision levels.

Decisions undertaken at multiple time scales, and long-lead decisions are very difficult to evaluate, since the outcome of the decision can be decades in the future. How can we know we have assessed all the important variables, and anticipated socially accepted policies? One way is to more strongly recognize confidence building by both policy makers and social groups on shorter term decisions. Year to year successes are likely to build confidence in longer lead decisions. Hence, better capability in decisions on annual or seasonal time frames is critical to building the credibility that is needed for harder, longer outlook decisions. It also allows the trial of decision options, and evaluation of effective decision strategies, that will also inform longer term decisions.

Whereas the report focuses on opportunities and capacities in the US, we would also benefit from better decision capacity elsewhere in the world (that reduces food insecurity or improves quality of life/social stability in developing regions of the world, for example). Improving the capability to forecast climate conditions at different time scales – from seasonal and inter-annual to decadal is of significant socio-economic value only if societies have the capacity to utilize them in a significant manner. Hence, in order to add value, the CCSP would need to facilitate building the capacity of socio-economic institutions, in the US and internationally, to utilize climate forecasts effectively.

The spatial and temporal patterns of climate events and their frequency and amplitude determine their socio-economic impacts. The CCSP should support scientific efforts at better projecting climate change in terms of all of these variables. In addition to the availability of resources (fiscal and scientific/technological), the adaptation potential of societies is determined strongly by the ability of institutions to manage impacts. The ability of institutions in the government, NGO and the private sectors to respond successfully to climate events in the short term would lay the foundation for success in adapting to climate change. Hence, CCSP should support research on innovative ways of managing climate variability with the explicit mandate of utilizing such approaches for long term adaptation. Identifying existing practices and their policy arrangements in successfully managing climate variability and extreme climate events is one way.

While it is important to understand sources and magnitudes of climate change uncertainty, there also needs to be clarity as to what kinds of uncertainties are needed by decision makers. The plan addresses the skill of models in assessing climate change, but tends to focus strongly on shifts in trends and absolute magnitude of change (1-5 degrees) as the most important aspects of uncertainty. However, the influence of climate change on climate fluctuations at shorter (annual - decadal) time scales may end up being the more important signal for society, planning, and adaptation.

The report has a strong emphasis on longer-term changes and less so on interannual variability, whereas the observing systems need to support analysis, decision opportunities, and decision validation across a range of time scales. There is also a critical need to retain long historical records for the analysis of climate, and for the validation of climate models – especially important for the 'next-generation' climate observing systems.

Many of the human dimension challenges would benefit by consideration of decision systems utilizing seasonal and interannual information. This is especially the case for building trust with decision makers. Trust is built up over time and over several orders of decision capability. Seasonal and interannual time frames offer opportunities to test ideas and build trust, and to evaluate the aspects of human-environment systems that represent 'low hanging fruits'. At the shorter time scales there is good opportunity for building capability within institutions, validating aspects of model results, and conducting experiments in the integration of quantitative and qualitative information at timescales of interest to decision makers today -- to facilitate a deeper understanding of decision making for the longer term.

JAY, OGI SCHOOL OF SCIENCE AND ENGINEERING

The following comments refer to the document as a whole and are not specific to any chapter. They are in part based on the presentation of the Climate Initiative by Under-secretary Dr. James Mahoney at the American Geophysical Union Meeting, San Francisco, 9 December 2002.

1. Neither the document nor Dr. Mahoney has been specific regarding the reduction in uncertainty necessary for the present administration to act regarding emissions of "greenhouse gases", especially CO_2. This is an absolutely vital issue. Most of the scientific community (including this reviewer) believes that the evidence regarding the human role in global warming is already strong enough to justify changes in public policy as a prudent response to risk. The risk is substantial, because the climate system is very non-linear and has a considerable degree of intertia. Thus, warming and sea level rise will likely continue for decades to a century or more after we reduce CO_2 emissions. Also, the risk of catastrophic changes (e.g., changes in North Atlantic Ocean Circulation, melting of ice sheets) may well remain unclear until we have passed thresholds that make them inevitable.

2. Although I would counsel rapid implementation of an energy policy that decreases CO_2 emissions and reliance on fossil fuels, this does not imply that the research work described in the climate initiative is unnecessary. We need to improve our understanding of the climate system at both the fundamental and applied levels, while simultaneously decreasing CO_2 emissions at the earliest possible date.

3. Dr. Mahoney indicated that no increase of funding is likely to be forth-coming, despite the demand on the scientific community for more results at a faster pace. This is unworkable and does not speak well for the level of priority given global change research. Given a demand for immediate, practical results, I further fear that basic research will be de-emphasized in favor of applied research. This would also be a mistake – both fundamental and applied research need to be accelerated, likely with an increase in funding for both.

4. Eutrophication in the Mississippi River plume was highlighted as an example of human-induced change in the coastal ocean. Still, I failed to find a unified treatment of global change in estuaries and coastal waters. Estuaries and coastal waters account globally for~30% of the total oceanic primary production. They are also the part of the ocean that we influence most strongly and directly through manipulation of the hydrologic cycle, pollution, and eutrophication. They deserve the same sort of focused attention as the hydrologic cycle and land use.

JIUSTO, CLARK UNIVERSITY

The plan underestimates knowledge accrued to date concerning human drivers and potential impacts of climate change, focusing excessively on uncertainties and under-representing knowledge gained and the need to direct research toward action steps warranted by existing research. Overall, the plan needs to be much more specific, relevant and prioritized as to research needs, informed by previous research.

The plan also under-emphasizes previous stakeholder processes, omitting, for example, mention of the First US National Assessment of the Potential Impacts of Climate

Variability and Change. Non-US research and impacts are similarly discounted. The implicit proposal to replace the National Assessment and other existing stakeholder processes with poorly specified, government agency-run processes should be abandoned.

The plan should provide clearer indications of how better "decision support" will be accomplished. This is especially crucial in light of President Bush's misguided determination not revisit climate policy until 2012. To improve decision support, the plan should support more research on the impacts of climate change: thresholds, surprises, adaptation processes, geographic variability, etc. Mitigation options, and the costs and dangers of inaction as well as action, must also be thoroughly researched, without creating an excuse for inaction.

In general, the plan should provide more support for social science research into the human dimensions of climate change, as human activity and decision-making processes are central to understanding and addressing climate change problems. The bias against human dimensions research, and effective policy response, are reflected, for example, in Chapter 11's omitting any reference to IPCC Working Group 2's work on impacts, vulnerability, and adaptation, and Working Group 3's work on mitigation.

KATO, HAMPTON UNIVERSITY

First Overview Comment: There is no section of research related to global radiation balance at the top of the atmosphere and surface. These measurements provide a very good constraint of energetics to climate models. One the one hand, it is important to measure each component of forcing such as CO_2 concentrations, aerosol amounts and the response of the forcing such as cloud properties and surface temperature. On the other hand the ensemble of the forcing and response can be monitored by measuring shortwave and longwave radiation at the top of the atmosphere and surface. Because it is generally difficult to achieve required accuracy to measure each element of forcing and response for the climate purpose, it is important to make all these measurements and obtain a consistent picture. Changes in CO_2 amounts, aerosol loadings, cloud properties, and surface temperature need to be consistent with the change in radiation at the top of the atmosphere and surface. Furthermore, the energy input to the earth system needs to be consistent with observations in climate models because this is a driving force of the system. Therefore, the strategic plan need to devote a separate section of energetics otherwise it does not give a complete picture.

Second Overview Comment: For the regional energy balance, the energy of the region can be altered by latent heat, sensible heat, and advection in addition to radiation. These energy changes, in turn, alter the regional climate or climate change can alter these each energetic term. Therefore, if the strategy is to assess the regional climate change, all elements of energetics need to be estimated from observations. These observations are then compared with climate model estimate. Without right energetics in the climate model, any regional climate assessment is incomplete.

KEMPTON, UNIVERSITY OF DELAWARE

The CCSP Strategic Plan (draft of November 2002) has a huge omission. It does not provide any research or decision support for reductions of CO2 omissions. Research is needed in the technologies and decisions relevant to CO2 omissions and reduction of those omissions. A number of industries and state governments are already making decisions and committing resources to reduction of CO2 omissions. For a federal research Initiative to not address this makes no sense.

For example, research could be done on Demand-Side Management (DSM) by utilities, on the reasons for recent increases and reductions of energy use by individuals and by industries, on state policies that increase electricity generation from renewable energy, and many other topics.

The Strategic Plan does not make sense without this component, nor does it provide "decision support" without these components.

KIRSCHNER, UNION OF CONCERNED SCIENTISTS

As a consulting scientist reviewing the updating of the pending US Climate Change Science Program Strategic Research Plan, I have noted some good plan elements, but also some serious weaknesses that must be addressed before the plan is finalized.

Overall, I believe it fails to capture the state of the science and to build on past assessment experience; falls short at prioritizing research areas; appears to be imbalanced and biased in scope; and places too little emphasis on the cost of delay or even inaction in response to climate change.

I do hope greater attention will be paid to better-adressing such concerns:

* That the plan fails to adequately capture the state of the science and the most essential research questions?

* That the timeline for producing research results appears inadequate and unrealistic, especially in light of input needed from research efforts to be conducted in other program elements.

*I believe more input and attention is needed through related organization outreach and greater involvement of stakeholders.

JEFF KUHN, INSTITUTE FOR ASTRONOMY

Overview comments on Chapter 1,2 and 6
The introductory chapter embraces the important issues, in particular, the rather fundamental question "what are the natural and human-induced forces in bringing about change?" Without an answer to this problem any further effort (except the energy that goes into collecting the climatological data) is simply wasted. Unfortunately, there is a glaring inconsistency in this document in that, after this nice statement of the fundamental problem, the planned effort largely ignores this question.

There should be no doubt now (even to a climatologist) that solar variability is a significant source of climate change, and may even be dominant during some of the brief, but large, interglacial "events" visible in polar ice oxygen records. To argue that the last 50 years of climate change is predominantly due to man's influence is irrelevant to our basic need to understand how solar influences may affect the climate on historical and solar-cycle timescales. Quite simply stated questions like "Could the solar irradiance variation over the next decade be 0.3% -- three times larger than the current 0.1% bolometric variation?" have profound terrestrial consequences. Sadly, we lack the basic physical understanding to answer whether or not this *can* happen. Asking *will* it happen is an even more difficult question. At a level of 0.3% solar forcing variation, most would agree that the climate must change in response to the Sun. Surely this is sufficient reason to devote a significant fraction of this climate study effort to address the causes and likelihood of these natural changes. For example, if we could predict a Sun that is 0.3% brighter in the next decade, we must surely revise our approach to minimizing the negative societal impacts of climate change. This report, like several previous "official government/scientific documents", is a real disappointment, but considering the dominant personnel engaged in defining the scope of this program it is unlikely that we will ever see a truly rational recommendation for understanding, and dealing with climate change.

LASHOF, NRDC

Establish an organizing framework: The draft document is not a *strategic* plan—it is a laundry list of unfocussed questions. For the plan to be strategic, it needs an organizing framework. That framework should be taken from the objective of the Rio Climate Treaty, to which the U.S. is a party: What is required to stabilize heat-trapping greenhouse gases in the atmosphere at a level that prevents dangerous human interference with the climate system?

We already know that each year by which meaningful reductions in CO_2 emissions are delayed limits our options and increases the risk of dangerous climate change. Priority should be given to research that clarifies the emission budgets that would be required to achieve any given stabilization level, and the systems most vulnerable to dangerous climate change if a given budget is exceeded. It must be recognized that it will never be possible to eliminate all uncertainty about the Earth's climate system. Hence the goal of the plan should be to iteratively improve decision-making under uncertainty, not to provide final answers to every possible question before decisions are made.

Recognize existing knowledge base: The Climate Change Science Program must build on the large body of research and assessment conducted over the last 15 years. The "State of Knowledge" sections found in Part II of the plan should be expanded and referenced. Similar sections should be added in Part I.

It is unacceptable to simply ignore the National Assessment of Climate Change Impacts on the United States. Indeed the Administration has accepted the findings of this assessment as the basis for its Climate Action Report submitted to the United Nations Framework Convention on Climate Change.

Establish explicitly links to the IPCC international assessment process: While previous IPCC assessments are at least acknowledged in places, the plan should recognize that the IPCC assessment process is ongoing and should be explicit about how results from the U.S. program will feed into the international research agenda in general, and the IPCC assessment process in particular.

EDWARD LAWS, UNIVERSITY OF HAWAII

During the December 4 breakout session on the water cycle, Richard Lawford of NOAA gave an overview presentation in which he commented that there was virtually nothing about the ocean in the current hydrological cycle model and that efforts to engage oceanographers had been ineffective. I discussed this issue with Jay McCreary, the director of the International Pacific Research Center (IPRC) in Honolulu. Jay commented that observed precipitation fields over the ocean were so bad that ocean models were not able to develop realistic sea-surface salinity (SSS) patterns when forced by them. According to Jay, typically, ocean modellers just "relax" SSS in their solutions back to observed SSS. His conclusion: Information about precipitation is so bad from the meteorological community that oceanographers can really not begin to attack the problem of the water cycle (oceanic evapoeration) with any reliability.

Patterns of precipitation affect the growth and storage of carbon by plants on land, and precipitation patterns over the ocean would certainly influence inputs of iron, the limiting nutrient over large and sensitive (from the standpoint of the carbon cycle) areas of the ocean. Based on the comments of McCreary and Lawford and my own knowledge of biological processes in the ocean, I would say that better information/understanding of precipitation over the ocean is an important gap in our base of knowledge. We will need to fill that gap before we can intelligently predict the response of the earth system to anthropogenic CO2 emissions.

LEWINTER, CITIZEN

The purpose of the report is very valuable, since it is necessary to conclusively answer important outstanding questions to determine the extent of any needed mitigation strategies. Nevertheless, certain strategies, such as significantly increasing use of renewable resources and efficiency, should not await the conclusive results of such research. There are numerous reasons to proceed as rapidly as possible with such strategies, for reasons other than global warming. Such reasons include the finite supply of fossil resources, the over-reliance on foreign sources of those resources, OTHER pollutants generated by fossil fuels, and the need to conserve those resources for other uses, such as petrochemicals.

A thorough literature search must be the first aspect of any scientific research. To avoid re-inventing the wheel, the report should state how previous research findings will be culled to ascertain their validity, and how they will be used to assist in formulating conclusions.

NEWELL, DOC

The Climate Change Science Program, in my opinion, does not adequately address its mandate. The primary purpose of the program, as I understand it, is to direct the U.S. Global Change Research Program in its effort to understand, assess, predict, and respond to human-induced and natural processes of global change. A basic part of that task is the stated need to distinguish human-induced from natural global change. The difficulty in making this distinction seems to be the source of many of the uncertainties attached to global change issues.

I compare this to a police investigation. When there are multiple suspects, you don't build your case around just one, even though that one may be more threatening than the rest. You pursue parallel investigations, and follow the evidence wherever it takes you. In the case of global climate change, I believe you need to cast a wider net. There is a "natural" perpetrator at large that has proven to be very elusive.

My position is that solar activity has not been getting the careful scrutiny that it deserves. It has been working below the radar screen of both the USGCRP and the CCRI. The NRC should have fingered it in 2001, but it managed to slip away once again. It's unfortunate, because I believe it is responsible for more climate change than you would think.

If you really want to get to the bottom of "the large and still uncertain level of natural variability," you need to be more bold in your attempt to "facilitate the discovery of the unexpected." Solar activity got past the NRC because they focused on solar irradiance. All that the earth gets from sunshine is potential energy. It takes solar activity to liberate it, but it happens in a clandestine way.

There is enough investigative work here to keep both the USGCRP and the CCRI busy. However, I suggest that we are overdue on this one, and need to lead off by establishing solar activity as a new climate change research initiative.

MACCRACKEN, LAWRENCE LIVERMORE NAT'L LAB-RETIRED

The draft plan fails to convey the global significance of this issue because it fails to present the key scientific findings and understandings agreed to by the nations of the world through the IPCC and endorsed by the US National Academy of Sciences and numerous other national academies of science that make this the "capstone" issue that it is. For an issue that is a Presidential priority, it is striking that a comprehensive scientific overview of the issue is not presented to justify why this issue is so important (e.g., of how the greenhouse effect works and is being enhanced by human activities, how human activities are very likely affecting the climate, etc.). The general sense of the scientific understanding provided in the Plan reflects what was known a decade ago rather than recognizing the significant advances made over the last ten years. The US was instrumental in the late 1980s in establishing the IPCC as the means for gaining world -wide consensus on the science so that all countries could act with common insight. That the IPCC findings are neither presented nor endorsed, even though three Administrations have joined all the world's nations in ***unanimous*** approval of the various assessments, violates the spirit of international scientific cooperation and analysis that has provided the basis for international research on this issue. The findings of the IPCC, not of the NAS that endorsed the IPCC's detailed summaries, should be the baseline upon which this plan builds.

The state of knowledge summaries in the plan do not, in most cases, even attempt to reasonably describe the state of knowledge, nor do they quantify the uncertainties that are given as the rationale for the research. The various sections of the report purport to present the state of knowledge for a given area, but almost all fail to do so, either by citation to the relevant IPCC chapters and other materials (which would be the preferred and authoritative way to summarize the state of knowledge), or by presenting an authoritative and referenced summary of knowledge along with a central estimate and associated indication of uncertainty. It is essential that the uncertainties be defined rather than simply including a statement that something is not certain or indicating a desire to know something. There also appears to be a bias in the report toward accepting the results of single studies that raise questions about an issue without recognizing the much greater set of materials that form the basis for the conclusion. For example, it is interesting, and seriously in conflict with the rest of the report, that one of the very few indications of a quantity, namely the amount of carbon currently being sequestered in North America, is given with no measure of uncertainty even though there are serious questions about this report and the quantity is later said to be highly variable and a major program is proposed to reduce uncertainty in this quantity.

Even though the concept of "uncertainties" is used throughout the draft plan, there is no discussion of this concept and the different meanings that it has for different groups and purposes. Virtually everything about such a complex system as the Earth is uncertain to some degree, and always will be. The real issue is whether the uncertainty is large or small in the context of the question at hand and the situation being faced. It was uncertain that the Trade Center towers would collapse, but most people reacted and left—and so survived. It is uncertain if your house will catch fire, but most people have fire insurance. If this plan is going to be focused around the concept of uncertainty, then it is essential that there be a discussion of what is meant by this word by different groups and how it ties into risk of making a right or wrong decision. It appears that the plan is using a definition based on the concept that all other possibilities must be definitively ruled out before a finding can be accepted. This hypothesis-testing approach used in some areas of science may be fine for building a reliable pyramid of knowledge, but it is not at all clear that this is what is appropriate when dealing with global change issues, where relative risk of economic or physical impacts may be what should be done. Whatever the choice, the plan has an obligation to explain what is meant by this term as it is a value-based decision that everyone deserves to understand and be able to consider. Without this, there is the likelihood of serious miscommunication with the public and decision makers, few of whom use the scientific approach in their decision-making process.

There is no metric for measuring progress in reducing uncertainties. If the program goal is going to be to reduce uncertainties, then it is important to have a metric indicating how much the uncertainty is to be reduced so that progress can be measured. At present, with neither a statement of the current degree of uncertainty in vague, qualitative terms nor a metric for measuring progress in reducing uncertainties, there is no way to gain a sense of whether any progress is being made.

The plan fails to explain the multiple reasons why ranges of results exist, generally attributing all of the range to uncertainty in the science of global change. The plan needs to differentiate between ranges in estimates that are created by uncertainties in understanding of the science on which research can be done, and ranges in estimates that arise because it is essential in developing scenarios of possibilities that span a range of conditions. It needs to be explained that it would be absurd to expect there to be no range in the estimates of what might happen over the next 100 years through various if-then simulations. It also needs to be explained that there is a natural chaotic element to the climate and that this too will create a range of possible conditions.

There is mechanism established and described for comparing uncertainties and determining their relative importance or the reduction in overall uncertainty that would result from the reduction in uncertainty of a particular variable. This is essential as a means of setting priorities among activities and determining to what extent reduction in particular uncertainties must be pursued. While the list of scientific questions is impressive, and while there is uncertainty associated with each of them, there is no indication of how the relative importance of uncertainties will be compared. Will reducing this or that uncertainty by some amount have a more important effect on the overall level of uncertainty than reducing the uncertainty of some other variable? Such comparative evaluations can only be made if integrated assessments (through both models and reviews such as done by the IPCC) are being undertaken. It is essential that the program be improving and have available capabilities for such evaluation as a means to evaluating the relative merits of investing research dollars into particular activities.

Rather than focusing the plan on "uncertainties," it would be preferable to focus the plan on "increasing confidence in the available results." Focusing the plan on uncertainties conveys the misimpression that virtually nothing is understood to any degree at all, in that the impression appears to be given that a result is either "uncertain" or "known" with nothing between. Simply because scientists in some fields do not like to indicate something is understood until there is 95-99% confidence level that the particular explanation or answer is correct does not justify saying that until this level of confidence is achieved, everything is uncertain. This is equivalent to saying that we can't say there is any water in a glass until it is 95-99% full. A much more appropriate approach would be to set as a goal for the research effort of *expanding available knowledge and seeking to increase confidence in the results* that have been developed over the many years (decades) of research on these issues.

All of the accomplishments for which a time period for completion is given should be providing both an indication of how much the uncertainty will be reduced and, as important, an indication of the required resources (either in absolute terms or above and beyond the sustaining of current levels) for this to be accomplished. This plan is a scientific wish-list, including all sorts of useful questions to work on. However, it is intellectually dishonest to indicate time periods for resolving certain questions without providing any indication of the level of reduction in uncertainty to be achieved and without providing a budget estimate to go along with this indicated improvement in knowledge. For many of the indicated products, estimates exist at present, and what will be done is to develop improved estimates, still with uncertainty. This needs to be clearly acknowledged.

The plan in many places indicates that various activities "will" take place and be accomplished, yet there is no indication that the budgetary resources will be available. It seems very obvious that phasing of efforts will be needed as there are many efforts proposed and many for which it is indicated that they "will" be happening. However, the budgetary resources are likely to be very limited—it is not even clear the Administration will get the rather miniscule $40M increase proposed for FY-03. It is essential that the plan indicate the level of effort going into each of the indicated efforts, indicate how much additional funding will be needed to fulfill the given tasks, indicate, at least in proportional terms, how new resources will be divided among tasks, and indicate when various tasks will be started so that an indication of when results will be available can be determined. Without a serious effort at indicating needed resources and available funding, this plan is only an indication of possibilities rather than realities.

The plan needs to define the term "assessment" which is used to mean many different types of studies. The word assessment is variously used to mean a scientific evaluation of how an observing system works to the comprehensive types of scientist-stakeholder-policymaker interaction that is carried out by the IPCC. It is essential that the plan develop and explain a terminology that differentiates between the various types of reports (perhaps calling the former type "evaluations" or something other than assessments.

The implicit charge that the US National Assessment was "controversial" (see page 46) misrepresents the nature of the discussions about the Assessment, and totally ignores the many parallel regional and sectoral efforts that accomplished so much. While there are indeed uncertainties in the findings of the National Assessment and there are aspects that could be improved in the future, to dismiss the effort and findings as controversial based on the criticisms that have been voiced seriously overstates and misrepresents the nature of the discussion. That there was a lawsuit about the procedural aspects is a moot point, as the suit was dismissed, based on an extensive rebuttal prepared by the agencies that addressed all of the concerns (if the mere existence of a dismissed lawsuit makes something controversial and erasable from the record, then it must be time to file suit over this Plan). The charges about the limitations of the climate scenarios seriously misrepresents how the model results were used, ballyhooing the results of an inappropriate test to try to defame the Assessment's findings (a paper on these misrepresentations has been submitted for publication). The findings of the National Assessment are largely independent of the controversy over the climate scenarios—clearly standing on their own. These results have been endorsed by the National Academy of Sciences and the IPCC and a summary of the National Assessment results is included as a chapter in the official communication to the United Nations by the present Administration—by not mentioning the National Assessment, the Administration appears to be disavowing its UNFCCC submission even though it was endorsed before the Senate by the head of the CEQ (and if this is the case, it should withdraw its UN submission and face questioning about that by the world community)? That this plan ignores the US National Assessment is a disgrace—there is so much to be learned from that effort that would benefit the public in the future.

In failing to acknowledge the US National Assessment, the plan misses the opportunity to learn from its studies and findings on impacts, and from its mechanisms for interacting with stakeholders. The US National Assessment was a major effort to explore a critical "if-then" question, namely what would the consequences be to the US if the climate changed according to the scientific projections summarized by the IPCC? This is just the type of question that the plan says that it will be oriented towards, but the draft plan fails to even acknowledge the National Assessment and learn from its strengths and weaknesses. In addition, the plan presently focuses very extensively on the physical and chemical science issues, and very little on the ecological and societal issues that are really of interest to stakeholders. In doing this, the plan seems to presume that all that the decision process involves only providing decision makers with indications of how the climate is projected to change, whereas what is really critical, and seriously under-emphasized in the plan, is to provide stakeholders estimates of what the changes will mean to the environment and society. There is also a total failure of the plan to examine these results and learn lessons about how to promote positive interactions among scientists and stakeholders that emerged in the course of the US National Assessment and the parallel regional and sectoral efforts.

The plan fails to fulfill the provision of the Global Change Research Act that calls for periodic assessments. Just as the Act calls for a research plan, it calls for periodic assessments. The plan fails to summarize and acknowledge assessment efforts and to lay out plans for future assessments, both internationally and nationally. The research was called for in the Act to feed into the comprehensive assessment process, and so it is essential that the assessment process be described.

Preparation of the evaluations and assessments needs to remain the responsibility of independent entities. The phrasing of the text makes it appear that the preparation of summary findings and assessments will be the responsibility of the agencies and the program staff rather than being done through independent and publicly reviewed processes such as the IPCC, the NRC, and mechanisms such as were used for the National Assessment. While the plan indicates that a number of findings and evaluations will be done, the phrasing seems to indicate that these will be done in a way that will make them agency or program reports, presumably subject to the particular views of the collection of agencies and whatever consensus that might emerge rather than being a scientifically credible summarization. It is essential for reasons of credibility that the evaluations be done independent of federal agencies and be publicly reviewed. [I would note that the NRC does provide external review, but not a public review, and I consider this to be a serious limitation were they to be responsible for preparing comprehensive evaluations.]

The plan fails to differentiate the issue of projection of climate change from the issue of climate variability and its prediction. There are fundamental differences in approach between efforts to forecast how the seasonal to interannual climate may fluctuate and the projections of long-term climate change that depend primarily on changes in the natural and anthropogenic forcings and on the history of past forcings that have initiated climate change during the past century. It is important to distinguish the possibilities for progress in these distinct areas and to work appropriately to reduce the uncertainties of each type of approach, rather than to mix them together and confuse them as critics and this draft plan are doing.

Although there are a few points made about international cooperation, the plan throughout generally fails to coordinate itself with the ongoing international research programs; this needs to be corrected. The plan needs to indicate that its research efforts will be conducted in the context of the international programs and explain how US programs will be developed and designed to cooperate and coordinate with international programs. As part of this effort, the structure of the US set of activities needs to be reworked to more closely match the international efforts so that the significant benefits of international cooperation and cost efficiencies can be achieved. The sort of go-it-alone attitude apparent in the draft plan is quite unfortunate, even though mention is made in some places that coordination will be sought.

The name of the program (i.e., Climate Change Science program) is too narrow—the issues that need to be dealt with are broader and the term global change is most appropriate. It is bizarre that the ever-broadening issue of global environmental change should be subsumed under the narrower name of "climate change"—this makes no sense and will lead to narrower thinking than is necessary and appropriate. The 1990 Global Change Research Act properly framed the complex set of interacting issues and should remain the overall framework.

Decision support needs to be more than about "if-then" questions. The plan appears to indicate that the main purpose of the CCSP is to assist national decisionmakers with policy decisions. The program needs to be about much more than this as the set of stakeholders is much broader than is indicated and the types of issues and questions that they face is much more extensive. To meet these needs, the CCSP needs to be oriented to providing all sorts of types of information and not being so arrogant as to think that all types of questions that will be of interest can be identified and will be provided with the appropriate decision support resources. As the National Assessment indicated, there are many types of stakeholders that need information of various types and can use information with varying levels of likelihood and uncertainty. There are not well-established thresholds for information or any small set of variables that need to be determined—the situation is much more diverse. What is critical is to be maintaining an interactive, two-way communication process with stakeholders and expecting that they can make use of varying types of information in determining what is best for them (this plan actually sounds much more government-centric than would be expected from a Republican administration).

The draft plan is replete with terms that are not explained and that need to be defined, given the anticipated audience. The audience for this plan is officially Congress. Recognizing this, the plan needs to explain the various concepts used, including terms like weather, climate, variability, change, uncertainty, greenhouse gas, prediction, projection, scenario, and on and one (see specific comments).
Michael MacCracken, LLNL (retired)

MAGAARD/GROSSMANN

At the recent meeting in Washington (3- 5 Dec. 2002) about the US Climate Change Science Program a consensus emerged on the importance of innovation as a major pathway to arrive at solutions for major climate problems - both mitigation and adaptation.
Unfortunately, innovation was rapidly narrowed down to technological innovation. Economic innovation could become a major factor in mitigation and adaptation as the amount of economic innovation is staggeringly big. In the U.S., during the past 20 years, 90 million new jobs have been created, and, simultaneously, 50 million of the 89 million old jobs of 1980 have disappeared. Qualitatively, these changes are so big that the economy has got a new statistical system, the NAISC (North America Industrie Classification, US Bureau of the Census).
A major driving force of economic and political change are the new information and communication technologies. These will continue to develop and grow exponentially for at least 10 more years. This will bring new products, new companies and new industries. These new products, companies and industries could very much increase emissions of greenhouse gases , but they could just as well lead to much lower emissions.
Policy-relevant research must evaluate these changes: their scale, scope, characteristics, and how to piggyback these changes to achieve major goals in mitigation and adaptation. As these changes are happening anyway, they might allow to achieve those goals at very little additional costs and just in passing. Use of changes, which happen anyway, is also a most appropriate way to deal with the huge uncertainties which are inherent in global climate change.
Research into the full arena of change, technological and socio-economical, is most likely to provide highly effective and desirable strategies for solving problems in climate change.

MARCELL, NYS DEPT OF ENV. CONSERVATION, HUDSON RIVER ESTUARY PROGRAM

The Plan is a federally funded initiative, and a great deal of exceptional climate change research has been produced by federal agencies, therefore, emphasis on federal research documents and organizations is to be expected. However, in doing so, the Plan neglects important international and NGO climate change research organizations, efforts, and reports and offers limited use and citation of the most important international climate change document, the IPCC's Third Assessment Report. This undermines it's legitimacy internationally as a fair and comprehensive assessment of research priorities and, consequently, may further jeopardize the United States' ablility to be taken seriously in future national and international climate change negotiations.

MARSHALL INSTITUTE, O'KEEFE

The Administration's draft Strategic Plan for the Climate Change Science Program (CCSP), and the results of the December 3 – 5 CCSP Planning Workshop provide a good foundation for developing sound climate change policy. This document represents the Marshall Institute's review and comments on the Strategic Plan. Wise, effective climate policy flows from a sound scientific foundation and a clear understanding of what science can and cannot tell us about human influence on the climate system and about the courses of action to manage risk.

Resolving the uncertainties that currently limit science's ability to accurately describe the climate system is key the development of sound climate policy. As the National Academies of Science observed:

> The climate change and variability that we experience will be a commingling of the ever-changing natural climate state with any anthropogenic change. While we are ultimately interested in understanding and predicting how climate will change, regardless of the cause, an ability to differentiate anthropogenic change from natural variability is fundamental to help guide policy decisions, treaty negotiations, and adaptation versus mitigation strategies. Without a clear understanding of how climate has changed naturally in the past, and the mechanisms involved, our ability to interpret any future change will be significantly confounded and our ability to predict future change severely curtailed. [2]

While the draft is a valuable discussion of climate change science, it is not a strategic plan. A strategic plan should present:

1. a vision for a successful climate research effort,
2. program priorities and how they are developed,
3. criteria for success,
4. allocation of available resources,
5. responsibilities for execution, and
6. the process by which the plan will be updated.

The draft CCSP Strategic Plan does not do this. It discusses 12 specific objectives of the Climate Change Research Initiative (CCRI) and an additional 33 for the U.S. Global Change Research Program (USGCRP). The closest approach to a vision statement appears on Pg 10 of the draft:

> By investigating a targeted by comprehensive set of questions, the CCSP seeks to focus attention on key climate change issues that are important for public debate

[2] NAS (1998): *Decade-to-Century Scale Climate Variability and Change: A Science Strategy.* Preface.

and decision making, while maintaining sufficient breadth to facilitate the discovery of the unexpected.

There is no indication of prioritization either within the CCRI and USGCRP lists or between lists. Most research questions are stated in general form. The report does discuss a number of specific and important research products, such as a quantitative analysis of North American carbon sources and sinks, and the USGCRP does list a number of more specific illustrative research questions. However, these specifics fall far short of establishing the criteria on which the success of the CCSP could be judged. Past research expenditures are mentioned briefly, but there is no discussion of future expenditures or how they would be allocated among the various research needs. The draft indicates that the CCSP process includes responsibility for an annual cycle of program and budget review, but gives no further detail on this process or the management authority and control that is needed to do this effectively. Nowhere in the draft plan are responsibilities for program execution discussed, nor is there any indication of when or how the strategic plan will be updated. The annual cycle of program and budget review is unlikely provide the in-depth analysis needed to update a Strategic Plan.

JOHN MCCOY

This is quoted from "USA Today" Wed. December 4, 2002 -
"Bitter cold air pouring into the Midwest from Canada set a record low temperature in Lansing, Mich. on Tuesday:"

Tuesday's low -18 - Previous record, 1869 -8 - Normal low" 24
Source: National Weather Service in Grand Rapids, Mich.)

QUESTION: Is the New Ice Age on its way??
Would an increase in the Production of CO2 help prevent this possible disaster??

MCGRATH, CITIZEN

I believe that Global Warming is a political movement. It was launched by a politician for the aggrandizement of power and as a sop to environmental extemists. The latter are most useful to politicians in fundraising, headline creation and organizing protests. Global Warming was enthusiastically embraced by the print and television media and Hollywood to such a degree that it is now referred to as though it were established fact. The truth is that global warming is a lot of baloney just like the global cooling was in the "70s. Of course, we taxpayers will pay the cost of the growing bureaucracy plus the enormous course to energy producing corporations that is sure to come. Your organization should be dissolved! I am writing this because I was unable to get through to Mr. Mahoney on C-Span this morning.

MCINTOSH, CITIZEN

After reading the text of all the papers and other materials provided on the web site, there seems to be a number of glaring, commonsense omissions in the underlying concept that we are undergoing significant climate change -- and we humans are responsible for it. I really didn't see the following discussed or considered in detail.

1. The earth has undergone numerous, dramatic changes in climate since its formation however many eons ago that was. This big ol' rock has gone through ice ages lasting millions of years to searing heat to subtropical warming in places that are now frozen. All without the benefit of human intervention. Where I live in Houston, on the Texas Gulf coast, was many hundreds of feet under ocean waters -- as was much of what is now the state of Texas. Fossil remains of shells and other sea creatures can be found all over central Texas in the Limestone that was created by these seas. But we also have many areas in this state where land roaming dinosaurs left tracks where these deep seas once existed.

2. Warmer and colder temperatures than we have today can be found throughout the records of man since the dawn of what we consider civilization. The Romans actively grew warm weather wine grapes in the northern parts of what is now England, we know of ice age cold that swept Europe during parts of the middle ages, and we know from the archeological record from the time of the Egyptians that the Mediterranean Sea was much, much lower than it is today. The Sahara Desert was much smaller. And all of this occurred where human populations were relatively small and the use of fossil fuels was unheard of. And how does science explain the existence of fossilized subtropical plants found in the Anarctic? Or plant eating wooly mammoths found flash frozen in many parts of the northern hemisphere?

3. I have never seen a serious discussion about the historical accuracy of instruments used to measure the Earth's temperatures. As I read the literature, accurate thermometers that can be calibrated and measure temperatures within a one or two percent accuracy have only been around about 80 years of so. And we all know about the problems of where measurements have been historically taken and the effects from heat islands caused by growing urban areas where these temperatures were previously taken. Accurate temperatures from satellites and other atmospheric observations have only been possible about 40 years now. Not even a second tick in the history of this planet. Science can only guess at what temperatures were 1,500 years ago -- or 15,000 years ago. No one knows for sure.

4. Finally, without a reliable, accurate history of what the Earth's weather has been, it is scientific fraud to try and construct models of what the weather will be. And that modern man has some hand in this climatic change. Or could do anything about it. The fact is, lots of factors effect weather patterns. Human activity may contribute to some of it, but until science can answer the questions from the past, it can't reliably answer the questions about the future. That needs to be made very clear in any discussion about climate change. The fact is, no one knows for sure. A mere 10 years of study means nothing.

MEARNS, NCAR

There is insufficient attention given to connecting the activities of the CCSP to other international efforts such as those of the IPCC. It is hard to imagine creating various new stabilization scenarios, for example, without considering how to integrate these with plans for new scenarios for the IPCC.

There needs to be a more explicit and detailed chapter/section on determining climate impacts. This is an odd lacuna in the current document.

Mecking, Stanford University

First Overview Comment: A U. S. government effort to understand and combat global climate change is long overdue. However, the research agenda outlined in this strategic plan represents a poor allocation of American efforts in this direction.

Second Overview Comment: The strategic plan does not sufficiently acknowledge the current state of knowledge about global climate change in the scientific community. Specifically, the strategic plan overemphasizes the uncertainties cited in the Intergovernmental Panel on Climate Change Third Assessment Report: Climate Change 2001 and thereby misrepresents the overall results of the IPCC assessment. Specifically, the IPCC TAR: Climate Change 2001 emphasizes that "there is new and stronger evidence that most of the warming observed over the last 50 years is attributable to human activities." The IPCC report also summarizes key findings regarding the present and future impacts of climate change on human and environmental systems, and presents comprehensive modeling efforts to predict future climate change.

Third Overview Comment: The strategic plan fails to make a convincing case that the levels of uncertainty in the IPCC report warrant a completely new look at specific questions. In general, the strategic plan does not take into account the major findings of the IPCC report, and it fails to present convincing reasons why further assessments are necessary for effective policy-making.

Fourth Overview Comment: The reviewer suggests that the CCSP allocate its resources to supporting rather than discrediting the efforts of the Intergovernmental Panel on Climate Change. The tax money for this program would be better spent in helping finance the established, 15-year-old research effort of the IPCC.

JIM MEYER, CITIZEN

It is good to see that a new program is being invented to address this issue and all the details that effect the climate of Earth. Most of the past research has resulted in poorly engineered models of the processes that effect the environment of the surface of Earth. In this vain there are several unknown issues that should be included in new research. First, the mantle of Earth should be invited since it is 99% of the mass of Earth and has the most vital role in how the planet operates. Second, the energy flux of the mantle needs to be recalculated because at this time the flux is underestimated by several magnetudes. This poor result is historical having been caused by misuse of thermal laws by unskilled people and this is another example of bad science. Third, the thermal storage capacity and the radiation efficiency of the oceans needs to be reaccessed since it is very clear that the mantle flux is managed by the oceans and if this flux did not exist the oceans would be solid ice even if the sun was 10% more energetic than it is. Fourth, the cycles that warm and cool the surface of Earth leading to iceages need to be understood as a result of change that occurs in the pattern of energy distribution from the mantle to the oceans which cause ocean currents and plate movement. Please put me on your e-mail list.

MILLAR, USDA

Overview Comment #1: (1) *Identify and implement case studies that demonstrate application of climate science at regional and sectoral scales.* (2) *Designate California as a formal regional case study within the CCSP*. A California case study would demonstrate integrated application of climate and climate-related sciences into local, state, and federal resource management, rural and urban planning, and policy and technology development. Diverse and state-of-the-art efforts exist in California on climate modeling, basic climate and paleoclimate research, wildland and urban science, integrated and collaborative ecoregional assessments (CalFed, Sierra Nevada), and technology developments, with infrastructure to support them. California's natural, social, institutional, and political diversity makes it a microcosm of globally relevant climate challenges and opportunities. The diversity of distinct, keystone regions within the state (deserts to rainforests, significant mountain ranges, agriculturally dominated Central Valley, urbanized coastal strip, etc.), each with unique issues, affords opportunities for downscaling to subregional scales. Ongoing, nascent, and new efforts (e.g., a Sierra Nevada Climate Change Assessment) could be integrated into coordinated regional templates to serve as examples for other areas.

Overview Comment #2: *Integrate paleoclimatological and paleoecological perspectives into the conceptual framework of the Strategic Plan*. Information about historic climate and ecological responses is not merely a missing chapter in this plan. Rather the plan throughout lacks fundamental understanding of cross-cutting insights and implications from current paleoclimate sciences. Although there is discussion of climate variability (implying natural mechanisms) versus climate change (anthropogenic), the distinction itself reveals a poor understanding of the overall climate system on earth. For instance, there is little recognition of 1) the nature of hierarchic natural climate cycling and mechanisms that operate on interannual to multi-millennial time scales and what these imply for future climate variability and ecological adaptation; 2) the nature of historic climate modality and the tendency for climate mechanisms at each scale to undergo significant and rapid reorganization; 3) the existence in the past of much higher rates of historic climate change than 20th century; 4) the nature, existence, and likelihood of abrupt climate change as opposed to gradual change (as depicted by IPCC), and the tendency for abrupt "flip-flops" to be triggered by change such as present warming; 5) the variable responses (including lags, thresholds, reversals, individualistic responses, surprises) of ecosystems and ecological (e.g., plant, animal, disease) and physical (e.g., water, fire) elements to natural climate changes at various scales in the past; 6) the variable relationship of carbon dioxide and temperature in the past – sometimes coupled, sometimes not; 7) the occurrence and consequences of non-analog climates and non-analog vegetation assemblages and transitions that have occurred commonly in the past and make the present a poor indication of potential climate states and ecological adaptations, and 8) the reality of rapid, frequent, and significant climate change as a dominant evolutionary force throughout the history of life on earth. I recommend participation by (more) paleoclimatologists and paleoecologists during the final version to better integrate knowledge of long-term climate effects with 21st century planning and assessments.

Overview Comment #3: *Revise discussions of ecosystem responses to climate to reflect likely non-linear, non-equilibrium responses to climate variability.* A key lesson from paleoecology that is also emerging in 20th century ecological studies is the dominance of non-linear, threshold, and individualistic responses to climate. Even in situations where climate has changed relatively monotonically, vegetation responses show threshold, reversible, and unexpected effects. For instance, our work documents threshold 20th century responses of high-elevation conifers that appear to result from combined gradual warming with decadal changes in precipitation further influenced by internal plant, community, and site dynamics. Even the same traits in the same species living in the same region can react in opposite ways depending on site characteristics. The point is that, like climate, unexpected thresholds can be crossed that trigger surprise physiological and ecological rearrangements. Further, ecological buffers and lags in response occur, making change we see today difficult to attribute to cause. For example, woodland vegetation in central Nevada is still responding to geomorphic changes triggered by a centuries-long drought 2500 years ago, whereas the immediate causal agent might appear to be 20th century forcing. Simplistic vegetation and ecosystem models and expectations, similar to simplistic linear climate models, are inadequate.

Overview Comment #4: *Prioritize mountain regions of the western US for an integrated initiative on climate science, assessment, and science-based policy within the CCSP*. Mountainous regions of the US are widely recognized as key centers of biodiversity, water reservoirs and water distributors, sources of clean air, minimally disturbed forests and wildlife habitats, forest resources, and playgrounds of wide demand. Steep elevational and climatic gradients and high natural fragmentation make these areas highly sensitive to changing climates. Mountain regions are thus both "canaries in the mine" for studies of early effects of climate change relevant to national and global modeling and planning, and critical areas in need of local planning, evaluation, and adaptation. Consortia on integrative study of mountain regions have been supported in other countries. Despite the excellent individual centers of research in United States mountain areas, a mountain climate network is lacking in this country. I recommend explicit priority to integrating efforts among mountain regions in western United States in the Strategic Plan.

Overview Comment #5: *Improve articulation of spatial and temporal scale aspects throughout the plan.* Although scale is often mentioned, the plan will be improved by greater attention to and clarity of appropriate temporal (days to years to centuries to millennial) and spatial (local to continental to hemispheric to global) scales, key inter-scale processes, and integration among all scales. Because of the special need now to downscale efforts to regional and local understanding, the plan should give extra attention to regional scales.

Overview Comment #6: *Develop a prioritized strategic implementation plan with associated funding needs identified***.** At present, the plan is an excellent and comprehensive report highlighting key science issues. With few exceptions (paleoclimatology), most of the pressing science and decision-support issues seem to be included. It is not, however, a strategic plan, nor would it be sufficient to develop an implementation plan. Emphasis in revision should be on setting priorities of topics, temporal and spatial scales, levels of integration, basic versus applied efforts, and analysis of funding needs for each area. Clear priorities and pathways for implementation should be outlined.

Overview Comment #7: *Increase participation of the federal land-managing agencies, specifically USDA Forest Service, USDI National Park Service, and USDI Bureau of Land Management in CCSP. Encourage greater participation from state land-managing and resource agencies.* Although USFS, NPS, and BLM are included in CCSP through their department affiliations (USDA, USDI), there appears to be scant participation by agency scientists or staff in developing the draft strategic plan or in attending the review workshop (of those on the registration list, only 14 out of 2500 were USFS). In addition to climate change research and science programs, these agencies bring long-seasoned expertise on several key foci that are treated as novel in the CCSP environment: decision-support, science-consistency, science-based policy, and integrated ecoregional assessments and planning. The passage of the National Environmental Policy Act of 1969 and subsequent federal environmental review and assessment acts catapulted the BLM and USFS into situations where integrated science, assessment, and science-based evaluation and policy-making at local to regional scales have been on front stage for over 30 years. State counterpart agencies are in similar position of expertise and seemed underrepresented at the Workshop. Further, the USFS, BLM, and NPS collectively administer the vast majority of federal wildlands in the US, with the USFS and NPS focused in mountainous regions, which serve as water towers, fiber sources, biodiversity reserves, and esthetic refugia for the nation. The lands they administer, and programs and missions for which they are responsible are at stake, making their involvement in CCSP even more urgent. Although climate change science may traditionally have been dominated by NOAA, USGS, and NASA, the important roles for ecosystems (water, fiber, wildfire), decision support, regional downscaling, and integrated assessments make it necessary to encourage active participation by these other resource agencies and their scientists.

MOREHOUSE, UNIVERSITY OF ARIZONA

General Comments

If I were grading the plan as a college class assignment, I would give it a “B.” That is, it seems to respond to the basic assignment, not much more. Many would say this is the definition of a “C” grade; however, I think the higher rating is appropriate, given the logistics involved in simply getting the draft written and printed in time for the December meeting. The plan makes some important points, particularly with regard to the need for improved atmospheric, oceanic, and terrestrial observation systems, and clearly recognizes the very real need to produce results at regional levels, and in ways that explicitly recognize and address constituents’ needs.

Constructive Criticisms – Overall Draft Document

General Comments

This effort has considerable potential to improve decision making, policy formulation and implementation, and management of any number of societal and natural resources. However, for this potential to be met, some significant revision of the plan is required.

As it is written, the plan is billed as policy relevant, but as written its relevance is to *science* more than it is to policy. Thinking in terms of science delivered to society makes much less sense than thinking of the plan as *society's science*. In this vein, temporal and spatial scale issues associated with trying to produce societally relevant science – science that is accepted and used by society – need to be recognized more explicitly in the plan. Indeed, the term *relevant* is – and must be – stakeholder-defined. For this to occur, there needs to be genuine integration of a wide spectrum stakeholders, ranging from representatives of small rural communities to people representing large corporate sectors, into all phases of research and development. Note that for this to occur in a meaningful way, programs should be designed to assure that non-scientists can participate effectively. This might entail, for example, certification courses through professional organizations, provision of courses and workshops through educational institutions or other venues, publications, and perhaps even museum and science center exhibits.

As was frequently mentioned by other reviewers, the plan is misdirected in failing to recognize the very excellent research and outreach that has already been done, is underway, and has been proposed but not yet funded. NOAA-OGP's Regional Integrated Science and Assessment (RISA) program, EPA's STAR program, and various initiatives (eg, the biocomplexity program) within NSF were not even recognized in the plan. These are excellent programs that are producing results that are valuable to the goals of CCRI/USGCRP, and without which I do not think the two programs can move forward effectively. Recognition and support of activities such as those being carried out by the RISAs need to be explicit in the plan, in no small part because it is projects like the RISAs that provide sustained interactions with stakeholders, have a track record of integrated research and development tailored to stakeholder needs, and provide educational opportunities that are essential to producing the next generation of sophisticated climate researchers *and* climate information users.

Social science appears in this draft as an overlay; there is a notable lack of integration of the broad array of potential social science and humanities contributions to the various components of the plan. The narrow range of perspectives represented here does not begin to reflect the insights that could be contributed by anthropologists, historians, philosophers, human geographers, sociologists, political scientists, and so on. It is unlikely that truly usable science will be generated without the participation of these types of experts.

The CCRI document is not as internally coherent as it needs to be, nor is there effective coherence between the CCRI and USGCRP components. It is very important that a solid vision be articulated of how the various research components fit together, how they will support and enhance each other, and how redundancies, contradictions, and gaps will be avoided. Further, there needs to be a clearer vision of where and how constituents (stakeholders) of these research activities will be integrated into the R & D process. This needs to be addressed.

PHILIP MOTE, JISAO/SMA CLIMATE IMPACTS GROUP

The CCSP is an ambitious description of some key unsolved problems concerning global climate change and its relevance to the economic and natural resources of the USA. Comprehensive plans are difficult to review in a balanced manner, because one is inclined to view the Plan from the perspective of one's own work and interests. We have tried, however, to include broad comments as well as those that reflect our focus, which is encouraging and assisting stakeholders in adapting to climate variability and change.

1. The CCSP should explicitly include a bridge to the seasonal forecasting community to treat variability on timescales of 1-20 years. In our interactions with stakeholders, we have learned that these key timescales are of great interest, yet they are much underrepresented in climate research (and the CCSP). Chapters 3, 4, 6, and 10 would especially benefit from greater treatment of these timescales.

2. Lessons learned from previous assessment reports (viz., IPCC and the US National Assessment) should be more explicitly included. For example, the CCSP calls for "identifying regions, sectors, and decisionmakers that would most benefit from improved global change information" (p41 l40) -- an important task well begun by the USNA. For the accelerated timetable of the CCRI, building on such previous experience will be essential. Some of the reports promised as "products" in the CCRI are already routinely produced as part of the IPCC, and care should be taken not to duplicate effort.

The CCSP gives lots of attention to the carbon cycle and carbon dioxide, but very little to other greenhouse gases and radiative forcing agents (especially in Chapters 2 and 5). They're mentioned on page 61 but almost nowhere else that I can find.

4. Several themes in the CCSP deserve affirmation: the focus on stakeholder interaction, the attention to regional issues, and the broadening of the notion of "scenario".

5. Climate models come under fire in several places (e.g., page 44, 48), for instance, because in some places (SE USA) the sign of precipitation change is inconsistent, but as Eric Barron said in our breakout group, even that tells us something useful: there are in fact physical reasons why in those places the precipitation could increase or decrease in a warmer world. There are useful ways of accounting for and displaying the agreement or disagreement among climate models; see Chapter 10 (especially Figure 10.6) of the IPCC TAR. The CCSP should be sophisticated enough to recognize the current state of the science.

6. The timetables for delivery of products are generally unrealistic, especially if funding will increase only slightly or not at all. On the 2-4 year timescale of CCRI, a small number of carefully chosen goals can be met, and care must be taken to balance the calls for reports (which can be very time-consuming) with the support of original research.

7. Communicating science to non-scientists will require training scientists in the art of communication. Would CCSP call for such a training program? (Chapter 6 page 70, Chapter 13) --

NASA EARTH SYSTEM MODELING FRAMEWORK ADVISORY BOARD

These comments apply to the overall discussion in the CCSP, and particularly the treatment of model development across all elements of the Plan. These issues are primarily addressed in Chapters 4 and 12:

In general, the CCSP provides an inconsistent view of the importance of modeling and data assimilation for the nation's efforts in climate science research, applications and policy. The plan correctly notes that models and data assimilation systems are required to provide predictive information and that models are needed for informing policy. It also cites several NAS reports which advocate that significant steps be taken to improve the models we rely on, to enhance the core institutions who undertake the policy-relevant computer runs, and to keep the hardware and computer infrastructure up to the levels required. However, the Plan is not very advanced on how to achieve these goals.

We see that the Plan is divided into sections related to the CCRI and the USGCRP.

The CCRI identifies three key steps required to strengthen US Applied Modeling Capability: (p.52 lines 1-36). These three are:

1. Use a two-center strategy, (identifying NCAR and GFDL by name)
2. Develop a "common modeling infrastructure" at those centers
3. Make a "substantial increase in US computational capability" dedicated to climate model runs

The USGCRP plans essentially neglect the need to strengthen US climate modeling capability. The USGCRP focus appears to focus on combining more and more component models (p 139, lines 4-19) into a more comprehensive system model. This reflects an attitude that a) the component models are in fine shape and b) that coupling them is a relatively simple matter.

The next paragraph discusses some research activities in climate modeling, with "areas of research emphasis would include model development, computational science, and data assimilation." (p 139, lines 28-29)

As an external advisory board, we feel that the Earth System Modeling Framework (ESMF) is essential for the future health of climate modeling in the U.S.

The CCRI plan acknowledges the power of a common modeling infrastructure: The paragraph on p52 seems to indicate that a common modeling infrastructure should be developed at the two centers, and that it will take care of the needs of those centers. While the two centers would indeed benefit from developing a common infrastructure, it is clear that the needs and benefits of a truly common modeling infrastructure extend well beyond two large centers. As an Earth System framework, ESMF embraces the needs of a wide variety of modeling groups, including weather prediction, climate modeling, climate forecasting, hydrological modeling and more.

One of the key elements of ESMF is the promise to reduce the software costs of migrating to newer and more powerful computing platforms by encapsulating machine-architecture issues to commonly maintained software levels. This has been clearly demonstrated with the pre-ESMF frameworks such as GEMS at NASA and FMS at NOAA, where researchers have been able to port extremely complex codes to different machines and architectures with relative ease. Coming to a single, on-going, curated solution to the community's common problems will represent a significant savings in effort across the country.

If research level climate system modeling is to contribute to the delivery of advanced model products, as is perhaps envisioned in the USGCRP (p 139 lines 26-27), then it will be particularly helpful for all modelers throughout the US to have access to a common framework on which to base their own efforts.

The US efforts in ESMF are conscious of and interacting with other groups around the world, particularly the PRISM effort in Europe. While CCSP is a plan for US climate science, it is imperative that the country remain a collaborator with the rest of the scientific community.

As an advisory board, we have reviewed the plans and activities of ESMF, and are very confident that they are proceeding on the right track, with extremely competent investigators, and with the potential to deliver a significant software framework for the entire modeling community. While we can understand a certain reticence to name specific groups and efforts within the CCSP documents, the plan should acknowledge that a nationwide, multi-agency effort, including some of the top research universities in the US, does exist.

Finally, we would like to note that a software engineering project with the ambition and promise of ESMF is not likely to be "finished" in 5 years. Within that time, the concept can be defined, and a functioning system can be built that will enable sharing of models and components, and will facilitate the migration to different computing platforms. But if the program is truly successful, it will give rise to new and more powerful ways to integrate and extend models as well as providing the cutting-edge center of development for the deployment of models on ever-newer and more challenging computer architectures.

NATIONAL PARK SERVICE

National Parks offer good places to establish long-term monitoring stations that would benefit from the relatively undeveloped nature of parks and from the existing long-term data sets that exist for some disciplines in some parks.

National Parks will inevitably have to adapt to the effects of global change and the ability of the National Park Service to manage this adaptation will require significant amounts of regional and local data that the US Global Change Program should be designed to produce. Reviewer's Name, affiliation: John G. Dennis, Ph.D., National Park Service-Natural Systems Management Office

NIST, HRATCH SEMERJIAN

The National Institute of Standards and Technology's (NIST's) response to the request for feedback on the Strategic Plan for the Climate Change Research Initiative is based on its years of partnering with the climate change research community to provide the measurement science infrastructure necessary for accurate environmental monitoring. This infrastructure includes new measurement technology, instrument calibration and characterization, measurement standards and validation, chemical and physical reference data, critically evaluated databases, and metrology training. NIST also participates in international collaborations and measurement comparisons with other national metrology institutes to ensure the world-wide acceptance of our Nation's fundamental standards, and thus any environmental measurement tied to these standards. Examples of NIST's recent contributions to the climate change community are described in more detail in the enclosed document, *NIST Measurement and Standards Programs to Support Climate Change Research.* These contributions include the following:

- Providing standards and instrument characterization and calibrations for the SeaWiFS ocean-color satellite and the Marine Optical Buoy (MOBY) used to "vicariously" calibrate ocean-color satellites;
- Disseminating aerosol Standard Reference Materials and furnishing polyaromatic hydrocarbon (PAH) measurements to assess the relative importance of biomass and fossil fuel burning to atmospheric aerosols;
- Providing standards and measurement validation for radiometric calibration programs supporting the NASA Earth Observing System (EOS);
- Partnering with various government agencies (NOAA, EPA, USGS, FDA) to maintain the U.S. National Biomonitoring Specimen Bank (NBSB), a repository of carefully documented and well preserved environmental samples, such as marine mammal tissue, fish tissue, and seabird eggs;
- Using chemical kinetics measurements to determine the atmospheric lifetime of gases found in industrial emissions to determine their ozone depleting and global warming potential;
- Providing spectral radiance, irradiance, and reflectance standards and training to ensure the accurate calibration of remote sensing instruments.
- Maintaining a chemical kinetics database of gas-phase chemical reactions for applications to atmospheric modeling.
- Furnishing trace-gas, ozone, and humidity standards for calibrating atmospheric composition measurement instruments.

The Strategic Plan for the Climate Change Science Program (CCSP) outlines a strategy for reducing uncertainties in climate modeling predictions. Unfortunately, the plan neglects the important role of measurement standards, sensor calibration and degradation, and other aspects of metrology in reducing these uncertainties. The document also fails to address the need for maintaining and expanding our Nation's chemical and physical properties measurement and data infrastructure, which will only increase in importance as the climate models gain in sophistication in their treatment of the underlying phenomena. Chemical and physical measurements of importance include aerosol optical properties and chemical composition, reflectance properties of vegetation and materials, and radiative properties and homogeneous and heterogeneous kinetic rate constants of atmospheric gases, with the latter becoming increasingly relevant as climate models combine with atmospheric chemistry models to address issues such as the relationship of air pollution and climate change. Also not discussed, at a more philosophical level, is whether climate change can be predicted at the required, but unspecified, level of accuracy by relying solely on atmospheric observations and modeling, often at a phenomenological level, as the report suggests, with minimal input from totally decoupled, independent, and unbiased laboratory measurements, such as on aerosol, gas, and material radiative properties.

Metrology and data furnish the structural and scientific foundation for our Nation's environmental monitoring programs and are essential for establishing and reducing the uncertainties in climate-model predictions, particularly given the extremely small long-term variations in many of the quantities being measured. In the area of metrology, standards and calibrations are critical for ensuring that measurements of the same or related environmental quantity performed by different sensors, at different times or positions, or by different nations can be compared, incorporated together into climate models, and used to test and validate these models without resorting to arbitrary numerical adjustment of measurements to maintain consistency. Such adjustments endanger the scientific underpinnings of the measurements, and when they themselves are based on climate models or components of climate models, reduce the objectivity and rigor of the entire modeling effort. The need to make such adjustments to the data is reduced by basing the components of the climate models, such as the radiative transfer modules, on well defined and accurate laboratory measurements, such as represented by the highly successful HITRAN atmospheric spectroscopy database.

Numerous measurement comparisons have failed in the past due to lack of attention paid to sensor characterization and calibration, measurement intercomparisons, measurement standards, and in-orbit and in-field sensor degradation, the latter of which affects the ability of a sensor to maintain its calibration. Instrument calibration and measurement standards are growing in importance as budgets for environmental monitoring decline and the cost of satellite missions and environmental monitoring networks increase. Budget constraints, for instance, are driving more international collaboration, are leading to a reduction in the sophistication of sensors and the quality of sensor characterization and calibration efforts, are forcing the elimination of redundancy in the environmental measurements, and are requiring a new policy of replacing critical climate satellites upon failure. This replacement policy is of particular concern to many scientists in the remote sensing community as it prevents the critical overlap of satellite measurements necessary to guarantee their equivalency. The negative consequences on the environmental measurements arising from these changes can be mitigated by careful attention to the underlying measurement science, which includes measurement standards and intercomparisons, instrument calibration and characterization, sensor stability and reproducibility, detailed uncertainty analysis of the measurements, transparent documentation, and validation.

The advantages of careful characterization and calibration of environmental sensors is illustrated by the SeaWiFS ocean-color project, which assesses ocean health and ocean carbon levels through satellite measurements of chlorophyll a concentrations. To remove the large atmospheric effects from these top-of-the-atmosphere measurements, the SeaWiFS observations are calibrated "vicariously" against measurements of ocean-leaving radiances performed by MOBY, the Marine Optical Buoy moored off the coast of Hawaii. Recent research at better characterizing and calibrating the MOBY spectrograph have contributed to the approximately 6 % decrease in the SeaWiFS derived mean monthly global chlorophyll a concentration, with a 25 % to 35 % decrease in oligotropic waters and a 15 % decrease in mesotrophic and eutrophic waters.

In this age of international collaboration on major science projects, it is particularly critical that all the partnering governments and agencies ensure that their measurements are based on validated and accepted national and international standards, ideally standards referenced to the SI units, to maintain the highest accuracy and consistency. In the United States, such national and international standards are maintained by the National Institute of Standards and Technology (NIST) and rigorously validated through international measurement comparisons organized through the Consultive Committees of the International Committee of Weights and Measures (CIPM), as laid out in a recent Mutual Recognition Arrangement between the world's National Metrology Institutes (NMIs).

The NMIs provide the necessary fundamental standards validated through international intercomparisons, related historical record, expertise in measurement science, instrument characterization and calibration capabilities, and knowledge of uncertainty analysis to ensure that the world's physical and chemical environmental monitoring measurements are comparable within and across national boundaries and over long periods of time. The importance of these standards is appreciated by many in the environmental monitoring community through their direct procurement of standards and instrument calibration and characterization services from NIST and the other NMIs and the integration into satellite mission work statements of the need to establish traceability to NIST chemical and physical standards, although often this traceability is neither rigorous nor transparent.

(2) Comments from NIST Chemical Science and Technology Laboratory on the Climate Change Strategic Plan

The uncertainties regarding the hazards of climate change, and the uncertainties in outcomes of national response strategies, must be reduced to reach environmental goals that are balanced with economy-related objectives. We cannot risk endangering our quality of life, nor can we justify the economic costs of overprotecting it. This is an extreme challenge. The uncertainties in question are sometimes phenomenological in nature, subject to environmental factors difficult enough to predict, let alone control. Yet we are committed to timely action, and one of the most worthwhile and enabling efforts should be channeled into assessing the need for accurate measurements and strengthening the metrological framework that supports such measurements.

The basis for effective decisions regarding climate change will reside, in part, with the accuracy and uncertainty of measurements the first link in the long chain of environmental action. Sound estimates of uncertainty in data are needed to allow intercomparability of results within and across measurement networks and time, and allow valid propagation of this uncertainty through models dealing with the water, carbon, and nitrogen cycles. In turn, this improves knowledge regarding the relationship between radiatively-significant atmospheric compounds, their biogeochemical cycles, and climate sensitivity. While the specific portfolio of programs is yet to be decided, some things seem certain: the programs will require measurements, and stakeholders will require improved confidence in measurement results.

The U.S. metrological framework is designed to enable measurements of quality sufficient to reach defined goals. Such measurements are performed under conditions having documented traceability to internationally-accepted references, and include the proper use of standard methods, reference materials, calibrations, and reference data. In order to effectively support climate change research, these references must be identified, developed and strengthened as early as possible. A sound metrological framework will provide confidence in the reliability, impartiality, and stated uncertainty of measurement data, which benefits the quality and utility of databases and models supporting informed policy decisions.

3) The following two items could be added in several chapters and sections of the Strategic Plan; it reflects a general deficiency in the Strategic Plan.

RESEARCH NEEDS
Improve confidence in measurement results by strengthening the internationally accepted metrology framework enabling quality measurements based upon documented traceability to accepted references, including standard methods, materials, and data.

PRODUCTS AND PAYOFFS
A sound metrological framework will provide confidence in the reliability, impartiality, and stated uncertainty of measurement data, which benefits the quality and utility of databases and models supporting informed policy decisions.

4) See NIST Measurement and Standards Programs to Support Climate Change Research paper at end of collation.

NOAA-CMDL
We at NOAA/CMDL have read and discussed the CCSP Strategic Plan and have the following comments regarding potential revisions for the next version. This plan does a great job in outlining and promoting extensive research that must be done to understand better the past, present, and future climate variations. The report also does an excellent job in summarizing and acknowledging the current state of knowledge regarding climate

change. Many of our suggestions have to do with making the document more explicit, so as to avoid ambiguity in its interpretation. We also are suggesting some corrections and additions.

****We wish to emphasize that we will do all we can to provide additional text or to augment our suggestions as needed. As we are not sure exactly as to where the document is proceeding, some of our suggestions have had to be more general than we would like. Please contact us for any clarification on any point and we will provide prompt and detailed feedback. This document is important to the research we do and it is important to the future of climate research in general.*

Thank you for your hard work. We appreciate the magnitude of the job facing you and the effort required to make this a viable and workable document.

A. Clear Statements of What we Know for Certain from Past and Present Research. Throughout the document, or preferably in a condensed section at the beginning, we need to identify what is robust today, what we know for certain, *i.e.,* for which there is little or no controversy. A good part of this document and the effort it represents is about uncertainties. We need to make clear to all readers, in bold print if necessary, what is certain, focusing on significant items. Each chapter could contribute some of these. For example, we know that major greenhouse gases are increasing in the atmosphere over the past century at rates higher than any time in the historical record. We know that the increase in atmospheric CO_2 is related mostly to fossil fuel emissions. We know that a diverse group of global models cannot replicate the 20th century increase in temperature without involving the observed increases in greenhouse gases. There are other robust findings already available. Citing these explicitly allows us to provide significant results at the outset of our 2-4 year window. *[Butler 303-497-6898 – Dutton, Hofmann, Ogren, Schnell, Tans; NOAA/CMDL]*

B. Specific Treatment of Uncertainties.

1. For both models and observations, uncertainties need to be stated quantitatively, either specifically or as a range, to make clear that we understand where we need to go from where we are. Scientific research must be done to answer questions. The questions are pretty clear throughout the document, but we need to state what resolution is needed to answer the questions at hand. It may be that we will want to report some variables with even less uncertainty than required, because we anticipate that answers to future questions may require such resolution. However, for the questions given in this document, needed uncertainties must be made clear. This has a particularly important effect on observations, which will be a mainstay of research to come. We want to be sure that the systems in place can provide data that are adequate to answer the questions asked by the modelers and others using the data. *[Butler 303-497-6898 – Dutton, Hofmann, Ogren, Schnell, Tans; NOAA/CMDL]*

2. Similarly, it is important to state what the limitations of the models will be when the desired uncertainties are obtained. We want to be sure that the public and policy makers do not develop expectations that cannot be met by this program. It also would be good to include a greater emphasis on the analysis of societal risk from delaying decisions, given the current acknowledged potential future climate scenarios. This "risk assessment" is crucial for making the decisions that must be made now. Current and future scientific research may take a long time to produce useful results. This also should be presented near the front of the document. *[Dutton 303-497-6660 – Butler, Hofmann, Ogren, Schnell, Tans; NOAA/CMDL]*

C. Explicit Cross-Referencing of Chapters throughout the Document. There is much overlap and apparent redundancy throughout the document, but this is difficult to avoid because the study of climate change is interdependent among disciplines, methods, and agencies. Because of this, it is important that the document extensively cross-reference the contents of the various chapters. Doing this will lead to removal of some redundancy, but most importantly it will strengthen the content of each chapter and underscore the interdependence of the various topics. *[Butler 303-497-6898 – Dutton, Hofmann, Ogren, Schnell, Tans; NOAA/CMDL]*

D. Feasibility of 2-4 year Horizon for Producing Significant Results. We don't want to be in a position of not being able to deliver when the time comes. Credibility and scientific integrity are both lost if that happens. A more realistic approach would be to view the CCSP as a longer-term endeavor, with some answers potentially available in 2-4 years and others requiring longer. The products that come out in 2-4 years most likely are the result of research already being conducted and we should make that emphasis clear throughout the document. We also note that many, if not most, of these products are not ends unto themselves, but rather milestones along a broader stretch of time. *[Ogren 303-497-6210 – Dutton, Hofmann, Butler, Schnell, Tans; NOAA/CMDL]*

1. The document repeatedly notes a 2-4 year focus of the CCRI to yield answers to the scientific aspects of key climate policy issues (e.g., p.2, line 16; p.60, lines 30-31; and many other places). However, there is no discussion about the feasibility of yielding results in such a time frame. Many of the research approaches, especially the experimental ones, typically require years to plan and execute, and additional years for the results to be incorporated into other elements of the Plan. *[Ogren 303-497-6210 – Dutton, Hofmann, Butler, Schnell, Tans; NOAA/CMDL]*

2. The document (p.164, lines 31-32) describes how implementation plans for individual research program elements will be "developed by an interagency working group, reviewed by external scientists, and approved by the CCSP." Along the way, these individual implementation plans will have to be coordinated and balanced within the context of the overall CCSP and funding will have to be allocated. How will these planning, approval, and funding processes be streamlined to yield answers in 2-4 years? *[Ogren 303-497-6210 – Dutton, Hofmann, Butler, Schnell, Tans; NOAA/CMDL]*

3. Some chapters give estimates of the time required to yield "Products and Payoffs" (e.g., Chapter 5) and others do not (e.g., Chapter 4). These time frames should be explicit in all chapters, but they should be carefully reviewed with due consideration for the points noted above, especially the time required to obtain funding and the likelihood of receiving that funding. Payoffs that go beyond the 2-4 year horizon also should be explicitly stated and timeframes assigned accordingly. We certainly do not want to promise delivery times and have to make excuses later. (The mix of products and time scales on p. 74 of Chapter 6 is a good example of how these sections should appear in the document, because the Products and Payoffs there are given for both short and long-term projections.) *[Butler 303-497-6898 – Dutton, Hofmann, Ogren, Schnell, Tans; NOAA/CMDL]*

(NOTE: After comments on remainer of document, these general comments were made, and I am adding them here – Sandy)
Elsewhere, this document provides a prominent example of the danger of relying too strongly on satellite measurements to detect trends. On p.6 (Chapter 1) the discrepancy is mentioned between surface temperature measurements and estimates of mid-tropospheric temperatures from radiances observed by satellites. Several years ago the satellite estimates appeared to show a downward trend of mid-tropospheric temperatures. It was discovered that an additional correction had to be made for orbital drift. When that correction had been carried out, a slight downward trend was reversed into a slight upward trend. There are many other assumptions that have to be made when converting radiances into temperatures. The IPCC considered the apparent satellite temperature trend in its assessment. They had good reasons not to give it as much weight as it has been given in some circles. *[Tans 303-497-6678 – Butler, Dutton, Hofmann, Ogren, Schnell; NOAA/CMDL]*

The gray box on Observational Priorities, which extends from page 134 to page 135, has no mention whatsoever of monitoring global, atmospheric CO_2. The following statement should appear in this box, probably under Carbon Cycle items: "Build an atmospheric observing network of CO_2, CH_4, CO, and related species that enables the continuing measurement of carbon sources and sinks on regional scales." This is the first element of NACP and it should be not only mentioned here, but emphasized. If we included other species into a similar statement, then the question would supercede the hierarchy of Chapters 3, 5, and 9. Perhaps the points in the boxes of this chapter should not fall back directly upon the other chapters, but rather bring them together in such a way that the cross-cutting issues are underscored. *[Tans 303-497-6678 – Butler, Dutton, Hofmann, Ogren, Schnell; NOAA/CMDL]*

There is no mention of inverse models in the gray box stating Modeling Priorities (p. 141-143). Inverse models are the first step toward identifying *significant* carbon fluxes. We suggest the following bullet: "Improved inverse models, coupling the atmosphere and oceans, that will diagnose regional sources and sinks of carbon, based on in-situ observations in the atmosphere and oceans." *[Tans 303-497-6678 – Butler, Dutton, Hofmann, Ogren, Schnell; NOAA/CMDL]*

We need to emphasize the importance of improving instruments and measurements where possible and make funds available for just that. It is likely that the accepted uncertainties will become narrower with time as new questions evolve. Instruments need to be developed to meet these future needs. *[Butler 303-497-6898 – Dutton, Hofmann, Ogren, Schnell, Tans; NOAA/CMDL]*

NORTHWEST RESEARCH ASSOCIATES (NWRA)

Probability Methods in Climate Modeling and Climate Observing Systems

Dr. Ralph Milliff, Dr. Gad Levy, Dr. Joan Oltman-Shay (joan@nwra.com)

II. Overview Comments

The CCSP draft plan can be improved by strengthening connections between climate change issues and probabilistic modelling methodologies and decision theory. Probabilistic methods, based on Bayes Theorem, are well developed in the statistical modelling literature. The theoretical foundation of Bayesian methods provides a framework for quantitative comparisons and characterizations of models, observational datasets, and blended data-model tools that are key to climate change research across the focus areas identified in the CCSP draft plan (atmospheric composition, climate variability, water cycle, carbon cycle, ecosystems and land use changes). Annotated references to specific sections of the CCSP draft plan are provided below (section III).We begin with broader comments to make the case that probabilistic thinking will complement traditional (deterministic methods) in stimulating new advances in climate change research and policy development.

Because of our backgrounds, our comments regarding probabilistic approaches to complement standard deterministic methods in climate research will focus on applications in climate modelling and climate observations. But probabilistic methods are beginning to be applied in other areas of interest to CCSP as well (e.g. see Wikle, 2003). From the policymaker perspective, it is useful to note that a formal decision theory has been developed from these methods (e.g. Berger, 1985), but elaborations of this connection are better left to experts in that area.

A likely first e_ect of an emphasis in CCSP on probabilistic modelling methods will be a change in the tenor of discussions of model and/or observation system philosophies to include estimates for parameters of distributions of interest (e.g. means and variances in measurement error models, conditional distributions for process parameterizations given a specific climate scenario, etc.). Put another way, probabilistic thinking will drive model and data intercomparisons to focus on modes and variances in distributions, in addition to qualitative comparisons of single realizations from a deterministic model response.

Early applications of Bayesian principles in climate science have been concerned less with modelling and more with "detection" and "fingerprint" issues (e.g. Hasselman 1998, Leroy 1998, Forest et al, 2002). But the probabilistic modelling methods do more than provide a framework for comparing analyses and models. They provide a means for simultaneously exploiting more fully the information content in: a) massive earth observation datasets implied by existing and planned in-situ and remote sensing systems; as well as b) the insight gained from process model heritage in atmosphere and ocean sciences. These information sources and their associated estimates of uncertainty can be combined as probability density functions (pdf) in Bayesian Hierarchical Models (BHM; see for example Berliner et al. 2003) that incorporate multi-platform observations and sophisticated model parameterizations. Accordance with Bayes Theorem is achieved in practice via computing methods based in Monte Carlo estimation that port well to modern parallel computing system architectures. The combination of model and data in BHM includes, but also extends beyond, standard practices in data assimilation for deterministic climate models. From the management perspective, output from probabilistic models provide useful bounds on complex systems traditionally treated by deterministic models, and they maximize the return on large public investments in multi-platform climate observing systems.

We emphasize that probabilistic modelling applications in CCSP should be enhanced as a complement to deterministic models that, in atmosphere and ocean sciences, benefit from generations of development and sophistication. Probabilistic concepts in parameterizations are emerging as a means of improving the most sophisticated coupled models for simulating and predicting climate variability and climate change. Extending recent air-sea interaction BHM on the synoptic scale (Berliner et al., 2003) to climate scale problems involves dimension reduction issues that have their counterparts in data assimilation for deterministic models. Ensemble forecast techniques (e.g. Zhu et al, 2002) can be viewed as a means of emulating probability distributions given deterministic model tools. But deterministic ensemble systems do not account for model uncertainty, and its dynamics, as is done in probabilistic models according to Bayes Theorem. Probability distribution output from BHM can be used to guide the development of a few carefully designed deterministic model calculations, and to provide a pdf context for interpreting forecast model results. CCSP support for parallel developments in probabilistic and deterministic climate models will benefit each approach, and lead most e_ciently to an enhanced forecast capability.

Annotated References to Sections of the CCSP Draft

Direct mention of probabilistic modelling methods is rare in the CCSP draft plan. However, there are many references to quantifying uncertainty in contexts that are directly amenable to probabilistic approach. Following Ferson and Ginzburg (1996), we identify two kinds of uncertainty: objective and subjective. Objective uncertainty is the inherent variability of a stochastic system. Limits of predictability issues are concerned with the quantification of objective uncertainty. Subjective uncertainty is a consequence of incomplete knowledge about a system. It can be reduced with increased knowledge, for instance through increased and refocused data collection and research.

Recognizing that reduced uncertainty can occur through increased knowledge, and that increased knowledge comes at a cost, a formal analysis of the cost-benefit of research investments could be approached using Bayesian statistical methods, thus identifying a set of future knowledge needs that provides the greatest reduction in uncertainty for the least research investment (e.g. time, money, infrastructure). Procedures of this kind can be developed to add objectivity to research investment decisions. In the following examples, we make note of both rare references to probabilistic concepts and references to uncertainty management. Additional comments are provided for most references.

• "CCSP analyses should specifically evaluate and report uncertainty" (Chapter 1, page 11).

• "Identify, quantify and systematically reduce uncertainty in climate model projections." (Chapter 4, page 47).

One method suggested in the plan for this purpose is that of climate scenario development. But it is important to note that a single forward model integration under a given climate scenario results in a single realization of the climate change in what is really a distribution for climate change probability given that scenario.

• "A particular need is for full exploitation of the satellite data record." (Chapter 4, page 49, lines 13-14)

While present-day earth-observing satellite data are routinely assimilated into weather and seasonal forecast models via standard methods for forward-model data assimilation, it is widely held that much of the satellite data content is not impacting the forecasts. Incorporating pdf characteristics from the satellite data is a means for improving this situation. BHM test bed experiments indicate that satellite data have a large impact on posterior mean field distributions (Berliner et al, 2003).

• "Sets of ensemble global simulations projecting possible climate change ..." (Chapter 4, page 51).

See comments above regarding ensemble forecasting. Note that this is one of a very few mentions of ensemble methods in the CCSP draft.

• (Chapter 4, page 53, line 14).

This is a rare specific mention of stochastic modelling, and it occurs without su_cient background information.

• (Chapter 6, page 72, lines 1-5).

BHM methods can be used in array design (Berliner et al 1999), and to provide pdf estimates that bound climate change projections (Berliner et al 2000).

• "Provision of probabilistic estimates of regional fluctuations..." (Chapter 6, page 74, line 6).

This specific mention of probabilistic methods in the CCSP requires better background material for interpretation. A probabilistic ENSO model is described in Berliner et al, 2000).

• "Quantitative estimates of the probabilities and risks of abrupt global and regional climate-induced changes...." (Chapter 6, page 75, lines 29-31).

• "Focused regional climate discussions and assessments, including characterization of uncertainties." (Chapter 6, page 78, lines 18-19).

• Specific calls to address uncertainties, reduce errors, and produce consistent analysis of the carbon cycle in both the CCRI and CCSP appear in:

Chapter 2, page 19, lines 31-33; and in

Chapter 9, page 101, lines 22-24.

General Comments

These are supported by citations of deficiencies in current carbon cycle models and measurements, and by calls for developing new models, analytical tools, and methods in:
Chapter 2, page 20, lines 4-6; and in
Chapter 9, page 102, lines 26-27 and 41-43, page 103, line 3, page 105, line 11, page 107 lines 38-39 and 108 lines 1-5.

• Specific calls to address uncertainties in modeling and analyzing water cycle and precipitation processes in both the CCRI and CCSP appear in:
Chapter 2, page 21, line 27; and in
Chapter 7, page 85, line 6.

These are supported by citations of deficiencies in current water cycle observations and models, precipitation forecasting and representation (parameterization in climate models; and by calls for developing new models, analytical tools, and methods in:
Chapter 2, page 21, lines 23-27 and lines 35-39; and in
Chapter 7, page 83, lines 13-15, page 84, lines 14-16, page 85, lines 5-6, and page 87, lines 30-33.

• ”Establishment of a linkage between observations and assimilation technology and between surface and space-borne sensors ...” (Chapter 12, page 132, lines 36-40). BHM methods establish such linkages by combining pdf implied by a) likelihood models based on known measurement error models, and b) priors based on forward model parameterizations and process studies.

• ”Outcomes will span a wide range of options, such as sets of ensemble global simulations projecting possible climate change ... ” (Chapter 12, page 143, lines 16-21). We note that relevant probabilistic model integrations can create pdf for essential model responses. These can guide more expensive forward model, data assimilative integrations so as to control costs. For example, forward model simulations could be designed (”tuned”) to visit prescribed regions of probability space, based on predetermined pdf estimates.

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ORGANIZATION PRESIDENTS (IN LETTER TO JIM MAHONEY)

As Presidents of organizations whose members are actively engaged in climate change research, we congratulate you and your team on the successful Climate Change Science Program Workshop held in December. We offer whatever help we might be able to give to make this Program more successful..

We note in particular our interest in seeing the important issue of vulnerability and resilience research incorporated into the climate Change Science Program. As you noted at the conference, the Program should accelerate the use of information derived from basic research, monitoring, and modeling to help society deal with the difficult issue of resource commitment and the valuation of a response strategy.

Chapter 11 of the CCSP strategic Plan identifies a key questions: What are the current and potential future impacts of global environmental variability and change on human welfare, what Factors influence the capacity of human societies to respond to change, and how can resilience be increased and vulnerability reduced?

To incorporate this question into the broader research agenda, we recommend that a formal program of Vulnerability and Resilience Research should be established to address three issues.

- *Regional Vulnerability Assessments.* Most climate change impacts are (a) regional in nature and (b) often related to extreme events, such as periods of drought, floods, and severe storms. However, private enterprise and local governments most directly at risk frequently lack tools for assessing their vulnerability to such events. Congress can foster resilience at the local level by supporting regional vulnerability assessments based on historical data, current observational data, and predictive models.

- *Preparedness Recommendations*. Building on the vulnerability assessments, the Program should identify short-term and long-term options that can be implemented at the local, state or national levels to reduce vulnerability and enhance resilience with respect to climate change and its associated extremes. The Program should, on a regular basis, review and evaluate progress toward enhancing resilience.

- *Vulnerability and Resilience Research* The Program should also establish and coordinate basic and applied research across numerous disciplines--including physics, meteorology, oceanography, and the other geosciences, and agriculture--to reduce vulnerability and build resilience to climate and weather extremes.

Attention to these elements will improve the nation's ability to prepare for and respond to extreme and changing weather events. It will spur the development of new technologies, provide more options to local planners, and help safeguard the nation's most vulnerable communities. We have submitted these views as part of our formal workshop feedback. In addition, we are developing Congressional interest in this proposal. Our organizations would welcome the opportunity to talk with you personally on these matters at an early date.
Robert Dickinson, President American Geophysical Union; Richard Rosen, President American Meteorological Society; Myriam sarachik, President American Physical Society; Michael J. Singer, President Soil Science Society of America; Robert Hoeft, President American Society of Agronomy; P. Stephen Baenziger, President Crop Science Society of America

OSMOND, COLUMBIA UNIVERSITY, BIOSPHERE 2 CENTER

The division of the CCSP into an acceleration program CCRI and a sustained program is an excellent strategy, likely to secure the synergies needed in the short-term to address the long-term issues of climate change. However in the acceleration process the strategic plan seems to have overlooked that importance of the experimental approach climate change science.

There is an urgent need to bridge the currently predominant approaches in climate science, namely the gulf between observation and modeling through experiments. Harte (2002, Physics Today 55, 29-36) bears witness to the "dysfunctional consequences of this biomodal legacy" noting that "Physicists seek simplicity in universal laws. Ecologists revel in complex interdependencies. A sustainable future for our planet will probably require a look at life from both sides".

Foremost among Harte's ingredients for synthesis of Newtonian and Darwinian traditions in Earth systems science (ESS) is the construction of simple falsifiable, mechanistic models. Hypothesis testing will be much more efficient with simpler models applied in a context where experiments and measurements render them falsifiable.

Most Earth system science models are derived from weather forecasting models. Among other things, these models are anchored in huge data sets of simple physical measurements from over a century of observations of climate systems and in scaling up the principles of fluid dynamics in models of atmospheric and/or ocean circulation to the scale of the whole planet. This focus has led to complex "highly tunable" models that include all plausibly important processes, but the abundance of adjustable parameters makes the models a poor starting point for hypothesis testing-a necessary step in the discovery process.

PIERREHUMBERT, UNIVERSITY OF CHICAGO

1. Although paleoclimate modeling and analysis is mentioned in passing in a few places in the report, it is not given nearly the prominence it should have. It should even be featured in the "short term" strategic plan, as it is an underfunded field where progress relevant to climate change can be accelerated. Paleoclimate studies like the last glacial maximum provide irreplaceable tests of models, with regard to sensitivity to CO2, the carbon cycle, and the methane cycle. They also provide irreplaceable insights as to what the climate system can do, insights we would not obtain from modeling the present alone. Indeed, we wouldn't know about "abrupt change" like thermohaline shutdown if it weren't for paleoclimate studies. --Raymond T. Pierrehumbert, The University of Chicago

2. There is insufficient attention in the strategic plan to the pressing need to accelerate the adoption of modern software engineering strategies into the climate modelling community. The software of climate modellers is mostly stuck in the 1960's (compiled Fortran, the adoption of Fortran 90 notwithstanding). Modular and flexible models, and sharing of code in an "open source" fashion will not happen unless there are major improvements in software engineering. The major centers, GFDL and NCAR, are unfortunately not moving AT ALL in this direction, --Raymond T. Pierrehumbert, The University of Chicago

3. Besides the need for more centralized supercomputing power, there is a need for much more midrange ($200,000 scale) Beowulf clusters at university departments. This would serve the purposes of training the next generation, but also increase the size of the community that can tackle innovated climate science problems. It is cost effective,and dovetails well with modern notions of the Computational Grid. --Raymond T. Pierrehumbert, The University of Chicago

4. The IPCC report is mentioned in passing a few places, but the Strategic Plan (particularly the CCSP as opposed to the GCRP section) almost seems to studiously ignore or even have contempt for the IPCC. The CCSP section does not build on the significant findings of either IPCC or the US. Climate Change Assessment. In all, the report takes too many opportunities to be insulting toward the existing work on climate change, and shows a lot of ignorance about what has already been accomplished. --Raymond T. Pierrehumbert, The University of Chicago

5. Many well known issues are mentioned, such as the need to improve cloud models, but the strategy for accelerating progress is not well or at all articulated. People have been interested in and intensively working on cloud problems for decades, and while more money for research is always welcome, there has not been any convincing effort to articulate whether there are any short term fixes that could speed up progress. --Raymond T. Pierrehumbert, The University of Chicago

6. Climate and disease is treated, but only in the long-term USGCRP section. It is such a pressing issue that it should be given priority in the CCSP section. Will global warming

cause more malaria? More West Nile? --Raymond T. Pierrehumbert, The University of Chicago

7. Overall, there is a large bias in the CCSP section towards "adaptation" strategies rather than mitigation of climate change (i.e. preventing it) . It seems to suggest that climate change is inevitable, so we should just get used to it. There is insufficient attention to exploring "worst case" impacts, since in the face of uncertainties, if the "worst case" is extremely dangerous, it is worth taking mitigating action to head it off, despite a low or unquantifiable probability. Also, in discussing "adaptation," the emphasis is on economic and human systems. There is no attention the the issue of whether there is any realistic way that actions can be taken to help natural ecosystems "adapt" to climate change. The record so far on helping natural systems to survive human degradation of the environment (e.g. salmon and dams in the West) is not especially encouraging. Research needs to be carried out to determine if adaptation is at all a feasible strategy for natural ecosystems.

PORCH, LOS ALAMOS NATIONAL LABORATORY

1. I strongly support the focus of this plan on quantifying climate uncertainties. This seems to me to be a much better approach than providing ìbest guess estimatesî for decision makers, and much less subject to exaggeration of our level of understanding of global climate.
2. I also support the direction of the plan toward developing climate observatories with the necessary infrastructure and calibration such as has been developed by the DOE ARM program, but on a more global scale. However, this development will only allow the decision makers to be able to claim that they made the right decision on a scale of 4 years. It will not aid them in their decisions on how important anthropogenic emissions are to climate change on this short a time scale. However, the improved calibration and algorithm testing of satellite data from this kind of network would help quantify and lower uncertainties on the time scale of 10 to 20 years.
3. The most useful effort to quantify uncertainties in the next 4 years would be to quantify local and regional effects on the surface temperature and water vapor data that we already have (though I agree efforts should continue to resolve temperature trend differences from different satellite analyses). This did not seem to receive the attention it deserves in the strategic plan. Related questions include:
    a. How well are urban heat island effects removed from the temperature record?
    b. How important are regional land use albedo changes at sites that show the greatest changes?
    c. How important are changes in regional water vapor emissions from irrigation farming to temperature and water vapor measurements at key sites? Up to 10% of the boundary layer water vapor observed in the Midwestern United States comes from dry land irrigation in the Western States during the growing season. This is likely to be much more important at many specific sites near farms and reservoirs that were not active 20-50 years ago.
    d. How important are regional aerosol emissions to temperature measurements at key sites? The combination of regional aerosol emissions and increased water vapor from irrigation could result in cloud and haze changes that could be especially

important at night when the relative humidity is highest and might partially explain warming trends at night (and partially explain glacier observations).

RANDALL, THE RAINFOREST REGENERATION INSTITUTE

"Coming from" perspective: Participation in UNFCCC negotiations on development of an appropriate and effective international response action plan to anticipated global climate change. Reviewing IPCC TAR drafts, particularly the Summaries for Policymakers (SPMs) to bring out what presently established scientific knowledge is most salient for intergovernmental policy formulation purposes.

My comments relate to overall balance, realism, and effectiveness of the draft U.S. scientific research plan rather to any specific sections, on which others can offer more meaningful criticisms.

While the draft plan is intended as a strategic plan, it is an open question how the finalized plan will be later operationalized and funded. The draft science plan is obviously a lamination of a new climate change research initiative (CCRI) onto a revisted and updated ongoing decade-old Global Change Research Plan (GCRP). The CCRI, promised by President Bush to provide new resources, is to emphasize "reduction in uncertainty" and near-term "deliverables," while the revitalized GCRP is intended as a fully fledged scientific research program, mostly relating to climate change and its likely effects. The GCRP, as a decade-old ongoing program, is clearly well thought out, with additional suggestions received in the Workshop review conference held December 3-5, 2002.

It was apparent in reviewing the IPCC assessment reports, that the state of knowledge of climate science has improved considerably over the span of the three assessments so far performed, doubtless partly due to the gaps in knowledge that were revealed by the first and second assessments, leading to additional fruitful scientific research activities. It was also apparent that the physical climatology assessed by Working Group 1 is mostly well established scientific knowledge, with some remaining uncertainties, most well identified, mostly arising in the large general circulation climate models, while the vulnerabilities and likely ecosystem (including highly managed ecosystems of agriculture and forestry) responses are largely unelucidated at present. As a result, WG-3 has little knowledge base coming from WG-2 with which to assess potential mitigation options and strategies beyond the basic mitigation strategy of reducing greenhouse gas emissions, the only effective control variable now known. While emissions reduction is likely to remain a major component of any effective intergovernmental response strategy that might be adopted, the present state of scientific ignorance on "ecosystem responses" is limiting with respect to development of effective intergovernmentally negotiated climate mitigation action policies. Thus, I believe there may be a "balance" problem operationalizing this scientific research plan by overemphasizing "reduction of uncertainty" believed to be plaguing the complex computer models rather than reduction of the obviously much greater relative ignorance of the biogeochemical world interacting with the physical climate system, and indeed partially controlling it through known short-term feedbacks, to say nothing of the possible longer-term or emergent feedbacks of which we may be presently completely ignorant.

This skepticism is reinforced by having worked with complex economic forecasting models, which have proved very "uncertain," perhaps to the point of non-utility, though the calculable uncertainty is almost never revealed to the users. While the underlying dynamics of uncertainty propagation may differ in climate models from these economic models, the mathematics are much the same. Even if all parameter uncertainties were reduced by 90 percent (probably a wholly unrealistic target), the interacting multiple equations or postulated relations in complex computer models ensures that the overall uncertainty in model output is not reduced by anything like as much. Indeed, if any of the central parameters is unable of proportionate reduction of uncertainty by scientific research, the effort to reduce uncertainty in parameters more amenable to study may go for nearly nought as far as reducing overall uncertainty in model outputs. Furthermore, there is great temptation to attempt to "reduce uncertainty" by modeling the poorly modeled sectors in greater "more realistic" detail, incorporating more interactions and feedbacks to achieve greater realism. However, this has the effect of increasing the number of equations or relationships, which generally must still be parameterized, so the final output predictive uncertainty may actually be increased by the admittedly "more realistic" model, rather than reduced as might be naively anticipated.

The Climate Change Research Initiative, promising additional U.S. contributions to U.S. sponsored scientific research "to reduce uncertainties" about prospective climate change and human contributions to it, was announced by President Bush at the same time as announcing he would not seek ratification of the Kyoto Protocol, which the United States wrote and shoved down the throats of the rest of the nations who doubted its workability, and hinting at rescinding the U.S. signature of the Kyoto Protocol and even withdrawing from the Framework Convention itself. My concern that the CCRI may be a wasteful use of scarce scientific resources and funding is heightened by the indications at the December 3-5 Workshop that the additional appropriated funding resources are no longer in prospect and that even continuing funding of GCRP may be in some jeopardy because of unrelated national security concerns, particularly relating to terrorism. Consequently, there may be reason to fear that not only won't there be additional resources provided by the Climate Change Research Initiative, but that the CCRI might be used to divert such resources as might be available into unproductive, wheel-spinning, "reduction of uncertainty" in complex computer models to the detriment of building a useful, pertinent scientific knowledge base to inform the development and implementation of internationally agreed and coordinated climate change action response programs practical over the longer term.

Sadly, from my participation as an NGO observer to UNFCCC proceedings, the United States has persistently unilaterally shunted action into modalities and strategies that are unlikely to work, or be implemented by nations who do not accept their utility, workability, fairness, or adequacy that is vital to any action plan that is to be effective in practice. In effect, with the United States behind the "Enronization of Climate Change", this means that the only U.S. contribution to amelioration of the climate change problem may be contributions to the scientific knowledge base. The United States historically has provided a major contribution to the knowledge base, in considerable part through sponsorship of remote sensing of earth and climate variables, providing free access to the resultant data, and by pioneering development of general circulation climate models (in which the U.S. now appears to have fallen behind) and for which further development seems of some questionable value. If adding to the scientific knowledge base is to continue to be the sole significant U.S. contribution to solving the climate problem for the foreseeable future, it would behoove the Administration to ensure that U.S. scientific research funding is directed into the most productive, non-duplicative, activities, rather than wheel-spinning pursuit of chimeras such as "reduction of uncertainty" at the expense of reduction in our ignorance of basic, relevant, biogeochemical processes interacting with climate.

RAPS, INDEPENDENT SCHOLAR

First Overarching Comment: The Plan needs a) to take into full and thorough account in all published documents and internal functioning and planning the entire work of the National Assessment: its processes, its very large network already developed of stakeholders and scholars, and its products; and b) to formulate clear and explicit processes for meeting the Congressional mandate to continue such democratic, participatory exploratory processes, maintain and expand this network, and produce reports of equally high quality and usefulness. The most glaring omission from my perspective is this one, and it pervades the entire plan.

Second Overarching Comment: I encourage strategic planners to reconsider whom is included in the conception of "stakeholder" vs. in the term "citizen," both terms appearing in the Plan but appearing to mean different groups of people. All citizens hold a stake in our as a nation intelligently meeting our climate change responsibilities (to ourselves and other nations) through research, including researching, adapting to and mitigating the effects of climate change which are quite well mapped in published IPCC and USGCRP documents. The task of those in the CCSP as concerns the public (yet another term used to refer to populations overlapping with citizens and stakeholders) is to treat climate change as everyone's concern and to seek out everyone's participation in climate change decisionmaking.

Third Overarching Comment: Yet there is no one group identifiable as "everyone." This task will have to be accomplished community by community, group by group, population by population, depending on solid knowledge of their existing interests. I encourage you to take very seriously in their demanding implications for the whole Plan the comments made by Mike Sprague during the panel on the Plan's "Outreach" section.

Fourth Overarching Comment: Finally, it is appalling to see the Plan's utter failure to make use of IPCC, USGCRP and other prior climate research. It is as if this very expensive, detailed, and very useful research did not exist, or as if Plan authors wish it did not! Yet everyone involved in climate study in any way knows this research exists, and is familiar with it.Beyond being a slap in the face of those having produced and published this research, this approach is costly, foolish, arrogant, and shortsighted. I strongly urge the Plan to integrate this prior research and demonstrably plan for continued integration.

REDMOND, WESTERN REGIONAL CLIMATE CENTER, DESERT RESEARCH INSTITUTE

This report is in general well written, and in my view contains a lot of meat. Much of the writing is quite thoughtful, enough so to bear repeated reads, and most of the major bases are covered. The draft seems to mostly successfully avoid the zoo of acronymns and programs, and yet still carry substance, which is refreshing.

There are, however, a few significant areas where much more attention is needed.

I am looking at report from the standpoint of needs in the western United States, which is my main area of interest and responsibility.

And more specifically, this interest is focussed on how this plan can help overcome numerous present inadequacies and facilitate advances with respect to 1) understanding the physical behavior of climate in this very complex region, and 2) the use of climate information to help resolve societal problems and issues.

The effects of climate variability and change are expressed, and felt, most acutely at the local and regional level. Large scale factors help drive these, of course, but they are largely an abstraction and not directly tangible to the everyday decisions faced by an organism, whether plant or animal, including people. We need to know far more about the whole suite of factors (climate behavior to decision systems) at the local and regional levels.

This is tremendously important in the western United States. The complexity introduced by the presence of nearly a thousand mountain ranges, all of which exhibit fine scale structure in climate and in climate variability, greatly complicates the information picture. These mountains provide much of the resource base on which lower elevation life depends. Their climate behavior is a function of absolute height, relative height (above valleys), orientation, extent, proximity to each other, geographic location, and season of the year. They are the source of most of the region's water, much of its timber, summer grazing, minerals, recreation and tourism base. They occupy about 40 percent of the lower 48 states, provide water to the entire western Mississippi basin, and influence weather all the way to the Atlantic.

The complex climate picture in the mountainous West, coupled with widespread aridity, and the special role of mountains in the development of the American frontier and psyche, have led to an equally complex, and often seemingly bizarre, arcane, and impenetrable jungle of social institutions and arrangements, the foremost of which centers around water law, but including others generally associated with natural resource usage. These factors must be accounted for, and included, if climate information is to be utilized to solve real world problems in this region.

General Comments

A search through the text reveals that the word "mountain" only is mentioned three times in the entire document. This is a major oversight. Doubtless many of the authors understood their importance, but this issue needs explicit recognition and attention.

In addition, mountain climates are much more difficult to measure, and the great majority of western mountain ranges are not monitored for climate. There are several major measurement programs, and a number of smaller ones, that operate in the higher elevations, but most have short records (less than two decades). These networks were deployed in support of agency missions and have been managed around that goal. Unfortunately, very few of these publicly funded programs have the additional specific mandate to also measure for climate purposes, which carries more stringent requirements for quality control, documentation, accuracy, siting, and maintenance than do "weather" measurements. Yet, with the right attention and resource allocations, they could easily and readily do so. We should also utilize, augment, and carefully expand, the small network of high elevation research stations in the West. We cannot afford to have a separate high elevation observing system for climate only, and we must utilize, improve, and supplement existing networks as much as possible so that they can also provide badly needed high elevation information. In our region, the two most prominent candidates are the USDA/NRCS Snotel system (700 sites) for water supplies, and the USDI/USDA (and others) RAWS system (1000 sites) for fire and natural resources management. The strategy I would strongly suggest would 1) identify and build on existing capacity, and 2) add elements to fill voids.

Several other places in this report discuss the discrepancy between surface and upper air variations in climate. In the case of mountains, the surface reaches into the upper air, and surface measurements may help resolve these differences and discrepancies. In either case, it is vital to learn whether climate change arising from human activities, whether greenhouse or land use or other, are different at different elevations. We already know, from physical reasoning and from serendipitous observations, that the tops of mountains do not necessarily have to vary through time in the same way that the adjoining valleys do. We need to get a much better handle on this issue, for its own sake, with additional careful measurements, but in so doing this may help resolve the surface / upper air issues that climate researchers are struggling to understand.

There are ample opportunities, many of them not being realized, for those in the climate community to work with those in the mission-oriented agencies to bring about these much needed capabilities. In many cases the present situation is a matter of restricted vision, perceived limitations on mandate, and real lack of resources, but the entire region is the poorer for this shortsightedness. Coordinating functions need to be in place to insure that larger imperatives are being properly addressed at the top of the organizational structure, and not lost in the shuffle of day to day operational concerns at the field level.

Sensitivity studies showing the effect of temperature changes alone (no change in precipitation) in the western US mountains would have significant and disruptive effects on water supplies to those living everywhere in the region, at all elevations down to sea level. These have proven sobering enough that some western states, such as California, are taking a serious look at the potentially very sizeable economic consequences, and at strategies for adaptation. However, it is telling indeed that we cannot at present definitively state whether California is warming or cooling (regardless of cause), by how much, in the mountains or the valleys, and in which seasons.

The West is not alone in experiencing geographically specific effects of climate change and variability, and analogous arguments can be made for other regions in the nation. However, undeniably, these issues are far more important in the West than elsewhere. If the effects of climate change and variability in mountains, and the necessary monitoring, are not addressed at the fine scales needed (a few miles) we will have insufficient information for decisionmakers to work with.

One more geographic consideration that could also be raised is coastlines. Along the West Coast, there is a large change in climate from the beach to the inland hills, valleys and deserts, often within five or ten miles. Huge populations live in this transition zone, and energy consumption patterns, air quality, wind and precipitation amounts, shipping and transportation decisions, nearshore fisheries practices, oil extraction operations, and others, can react significantly differently to the pronounced local variations in climate. Over longer periods of months to years, coastal and nearshore climates do not necessarily change in synchrony with interior climates even a few miles inland. Again, we need to both 1) better understand the climate differences and causes for such, and 2) develop tools which put this information readily in front of those who need it.

Along these lines, another area that could stand to be highlighted even more is that a set of tools is needed to make it easier for those who wish to incorporate climate information into their decision framework. These tools are needed to access climate information at the level of detail needed by the particular issue of interest, from simple to complex, aggregated to detailed. In the West, again, the range of capabilities spanned by these needs encompasses orders of magnitudes. In our case, the need for intelligent and physically based interpolation and extrapolation methods, and intuitive interfaces to the resulting data and information, all accessible to a wide range of audiences, is very pronounced. To have practical value, these tools and capabilities must be developed in close coordination with those in the climate community who have routine interactions with this very diverse range of societal interests, or with those with specialized research emphasis on the interplay between decision making and climate. This latter point does receive necessary recognition in the draft report, but it is easy to overlook the importance of these bridges between worlds of understanding.

I may have additional things to add in subsequent weeks, but these are what occurred to me right off the bat.

REILLY, MIT

There are some important and useful pieces of proposed scientific research in the document but it is extremely difficult to figure out how this all fits together and how it will be managed. The document now describes 3 Fed. Organizations (in truth, virtual organizations, as the USGCRP at least, and I suspect these others, are really loose confederations of Agency Programs) involved in managing research on climate change. These are the Climate Change Science Program, the Climate Change Research Initiative, and the US Global Change Research Initiative. So the CCSP and CCRI have been layered onto the US GCRP.

Here's the problem. The introductory chapter describes how the CCSP is going to generate public reports and conduct analyses as an aid to the public and decision makers (e.g. see guiding principles of the CCSP). So, this suggests far more than a loose confederation of Agencies—it also suggests a strong central role in determining what is and is not a 'CCSP public report'. But, then we hear nothing more about the CCSP beyond Chapter 1—rather the 'meat' of the plan is simply the CCRI and the USGCRP pasted together with a shell of a CCSP to suggest some coordination. If this were mainly cosmetic, I would not be concerned. For the most part the CCRI and the USGCRP look like the usual—good science campaigns and research with some improved data, and greater data access—but because the CCRI is really something of a hodge-podge of targeted areas of science that need funding attention, it does not provide the sort of integrative and complete view that seems essential for decision making of any kind. And, the CCSP rightly observes that this has been lacking in the USGCRP. But, the CCSP, as described, remains, for good or bad, a very decentralized effort. At the most, one might hope for more coordination across agencies than has occurred in the past but the generation of data and scenarios and forecasts remain in the hands of individual agencies or scientists at universities who have a variety of standards and incentives with regard to who they see as their clients, what data will be released, what it will look like, how it will be documented, etc. This is fine and as it should be for many purposes but, nowhere in this discussion do I see any evidence of a CCSP role in oversight or defining standards, or review, or stamping their name on it. So how does the CCSP go about maintaining the set of standards or principles regarding CCSP public reports that it lays out in the introductory material?

What seems to be intended is that the CCRI and USGCRP are going to continue the science program, with the CCRI helping to target funds in critical areas that have been underfunded, and then the CCSP is going to play the role of synthesizer of new science information. But, where is the manpower, organization, modeling, and analytical capability that is going to bring this information together? The 'decision tools' section of the CCRI reads like the typical NSF program on decision science, where the interest is in funding some new algorithm for solving some complex non-linear, multi-objective decision problem but no willingness to fund the somewhat mundane but essential work of actually synthesizing, incorporating, and simulating somewhat more standard but empirically useful integrated assessment models that would actually be available, and could be simulated in real time, when policy makers had questions.

Will there be a cross-agency research and assessment team of mainly government researchers and modelers who will bring all this together and produce IPCC-like reports focused on the US—how will academic expertise be brought into that process, or are you content to have them developing decision tools or conduct science but not weighing in on how it is used in the integrative studies? Will there be some sort of review panel—or

NRC process of review—that will analyze the integrated summary of the science findings? Since some of this type of analysis and data construction, even when not analyzing policy, can have some pretty strong policy implications, how will this organization create confidence that a firewall exists between those who might want to manipulate the results for political reasons? Agencies like the Bureau of Economic Analysis of DOC, or the Economic Research Service/Office of the Chief Economist in the USDA, or the Bureau of Labor Statistics in Department of Labor, the USGS in Interior, the Congressional Budget Office, or the Energy Information Administration in DOE come to mind as agencies that have been pretty successful in producing forecasts and data for a community of public and/or private decision makers, where the information can be of a politically sensitive nature, while establishing and maintaining their credibility as independent of manipulation by those with political interests. So, there are models of success in government, but I do not see how the CCSP, as vaguely described as it is, plans on addressing this fundamental issue. The repeated phrase of the need for 'completely new relationships between the science community and decision makers' has, at this point, no content.

The attempts by the Federal government to analyze climate issues to support policy has so far been a failure largely because the efforts undertaken have not established credibility as independent of political manipulation. Notable examples include a cross-agency task force to analyze the costs implications of the Kyoto Protocol and the National Assessment effort, both under the Clinton Administration. Clearly, absent an organization with a mandate and responsibility to conduct such analyses, the academic community or research agencies within the Federal departments are not going to rise up and provide the sort of integrative decision relevant material spontaneously in the name of peer-reviewed science. Having attended the CCSP workshop in Washington, I was impressed with the sincerity of the regular civil service and the current group of political appointees to make the science relevant to decision makers and to keep it objective. So there appears to be a will, and maybe this group could be successful with an *ad hoc* organizational response. A legacy this group can leave, however, is an institutional process or organization that assures such objectivity beyond their tenure.

The applied climate modeling initiative is thoughtful and has some useful directions. But, to the extent scenarios are described, the process here is akin to the IPCC, with an implicit time frame of years between when an exercise is started and when some report or analysis is complete. This is out of line with the policy process. Often, the essential questions are not known until the decision is at hand, so answers to questions stakeholders had 2 years earlier may be nearly worthless because by that time quite different questions have arisen. Any one in research (or who makes a decision) knows that one must stop and report, or stop and make a decision, even though there is always room for improving the model/data or searching for more information. What is needed is a Federal organizational response that provides the capability for careful analysis and ongoing response to policy questions. I realize this is particularly tricky because whatever this organizational response is, it must interact with people in both the research and resource management areas of DOE, EPA, USDA, NOAA, NASA, USGS, etc. But, unless a strong integrative capacity is developed in the Federal government, one will continue to see partial analyses that fail to respond in a balanced fashion to questions of decision makers. Much of the research, assessment, and preparedness for climate change that might be needed at regional levels and in the resource management agencies can take place in existing agencies or at local and regional levels. Here the regional assessments begun under the National Assessment may be the way to proceed, recognizing that they are still a research and demonstration activity because it remains an open question as to whether there is much predictive value in current climate forecasts given the decision time scales of 5 years, perhaps 30 at the outside. These need to be fostered to see what information is useful—whether better monitoring of trends, downscaled regional models, or whether the vague idea of vulnerability analysis can be made useful. But, the missing ingredient is the process to support national decision making on overall mitigation policy, and to provide some boundary conditions and climate scenarios that might support a developing impacts/benefits and regional assessment capability. WE HAVE KNOWN FOR NEARLY TWO DECADES THAT THE EXISTING FEDERAL DEPARTMENTAL STRUCTURE IS ILL-SUITED TO DEAL WITH THE CROSS-CUTTING ISSUE OF CLIMATE CHANGE. Well-intentioned and capable people peppered throughout the existing structure is not enough.

I suspect the only way to create a successful organization that is (and is perceived to be) independent of political bias is through a legislative action that creates an organization that reports directly to both the Executive and the legislative branch (as does EIA). It might be useful if there were some sort of requirement that other Federal Agencies with scientific capability provide staffing on detail on some sort of rotating basis. It would also, obviously, be useful to have detailees from the policy/resource management arms of Agencies, on leave from their policy work, but able to help formulate analysis that aids policy making based on their hands on experience. The mandate for this organization should probably also require and have funded slots for academics to visit for periods of say 2 years, and act in some leadership capacity. Some thought would be needed on how to make this inviting to top-notch academic researchers. It could similarly include staff from state government or regional assessment programs, rotating through. Obviously, the program will need some permanent staffing (but this should, I think, be about ½ of the staff at any time) and a budget to attract visiting scientists from academia or other government agencies. Perhaps there could be some 5 or 10 percent bonus pay on top of civil servants salaries in other agencies while detailed in this new agency to encourage top Federal scientists and analysts to take the detail. I think this requirement of drawing across the Federal and academic research and policy establishment is the only way to prevent the organization from becoming too ingrown, and to ensure that it establishes the working relationships across the Federal government, and outside, that are needed. Even with all of this going for it, it will require a lot of skill for someone to make this a successful complement, without being perceived as a threat, to the existing Departmental programs that are necessary and useful for what they do, but not sufficient. Perhaps this provides some concrete way of creating the ‘new working relationships’ that the document calls for.

Such an organization need not, and probably should not, be directly responsible for the longer term research budgets, which should continue to reside in the agencies, but it would have the independent capacity to describe shortfalls, gaps, overlaps, and needs from the standpoint of contributing to its analytical needs which are supporting the decision process. If successful, this should give it a powerful seat at the table in establishing research funding priorities, and an advocate for decision relevant science. If I were at OMB, for example, I would find such an organization extremely useful. It needs budget to draw highly capable scientific staff but it does not need, nor should it have budget clout like NASA, NOAA, or the other science Agencies—its clout should come from it perceived independence and scientific credibility.

Second Overview Comment.

The inconsistency of time frames of the CCRI and the tasks, particular in the science components but also of the idea of decisions support, is so obvious as to suggest that the write up is a farce, meant for the amusement of the reader. It is impossible to imagine that the scientific data collection campaigns, and problems where the uncertainties have resisted resolution for at least 2 decades (or in other cases a ½ century or more), are going to be completed and resolved in 2 to 4 years so that decision makers can then move on with certainty. This continues to portray the decision making under uncertainty problem as one that involves two steps: (1) eliminate the uncertainty (2) then decide under certainty. It fails to recognize that much of the uncertainty may be fundamentally unresolvable (or essentially so if it takes so long that climate change reveals itself to be bad before the science can describe with certainty the particular mechanisms). It further fails to recognize that addressing climate change will be an ongoing process of management, much as management of the economy involves an ongoing set of decisions about fiscal and monetary policy. The science description is at odds with the emphasis elsewhere on decision making under uncertainty. At best, one will only have gotten a little way down the path on some of these problems—such as the role of aerosols—with some tantalizing results and more questions raised. So, the implication that the CCRI goes away after 2 to 4 years, and these things are over seems to be inconsistent with the research and data collection actually described in the document, where there is a correct emphasis on the need for consistent long term observations. As far as I can tell, the CCRI has identified some limited projects to go back and make inconsistent observations consistent by recalibrating and checking with some focused analysis. A short-term project with some backward correction for previous instrumentation problems is okay, but where are we implementing the broader, routine, and ongoing data collection?

Third Overview Comment

A good research plan needs a well-argued set of priorities. In each of the science elements in Chapter 2 and in the observations discussion of (Chapter 3) there are some excellently reasoned white papers, albeit the substance of those background papers are not reflected as well as they might be in the main document. Perhaps the most important idea in this piece is, however, Chapter 4. The key innovative idea and necessary element of this program, and from announcements from the White House a main objective, is to connect the science to the policy and decision making process. But, aside from identifying this, the plan fails miserably to describe how this will happen. Whereas, the other science elements have well-reasoned white papers and references to NRC reports, there is nothing vaguely comparable supporting the decision tools section of Chapter 4. The climate scenarios and 2 climate modeling center strategy is laid out in sufficient detail, and is fine for what it is, but if all one succeeds in doing after 2 to 4 years is creating two climate scenarios from 2 climate modeling centers they will be of little value to decision making, no matter how well they are done. The discussion of decision support is extremely weak, superficial, and full of vague generalities. Perhaps the newness of this idea has not produced the background documentation that exists in the other science areas but there are many people with much experience whose expertise could and should be brought to bear on this topic. While this area is very important in my judgment, I don't see how it could possible compete for scarce budget with such a weak description—it will surely lose out to the well-described science areas unless much attention is directed here.

Fourth Overview Comment : The idea that sensitivity and 'what if' scenarios are useful for decision making is wrong. This idea must be deleted from the document. This silly idea has persisted for some time now among some of the research community, and it is unfortunately indicative of an apparent failure of this part of the community to interact with people who are really making decisions where something is on the line—i.e their livelihood or job depends on making a good decision. (Or else this is a failure by these analysts to understand what is really going on). If we take the National Assessment exercise, for example, and examine the 2 'what if' scenarios for the southern States—one has it becoming hot and dry and the other has it becoming wetter and warmer—it is extremely challenging to imagine how this would really help decisions. One can imagine a set of decisions 'if it became hot and dry' and another set of decisions 'if it became wetter and warmer' and an entirely different set of vulnerabilities in each case—water shortage in one, flooding in the other. One might well abandon agricultural production in one (or develop some mammoth irrigation project with interregional water transfers)—while moving to produce paddy rice in the other. But, this does not help one make a decision about what to do today, since real decisions cannot be conditional. If the 'what if' scenario does not occur (or the other one occurs) then one may have a huge interregional irrigation project and instead one needs a system for controlling the hazards of flooding. And, with only these two scenarios, one has hardly even explored the range of possibilities. There was a nice piece of research by Barry Smit and colleagues some years ago that documented the fact that farmers in Canada observed a few warmer years, apparently listened to some of the climate change hype, and adopted longer maturing corn varieties thinking longer growing seasons were there to stay. Of course, 'normal' weather returned and all their corn froze before it was fully mature. And of course any decent climate researcher knows that the signal of climate change is weak, natural variability is large relative to the signal of climate change over just 5 or 10 years (we can't even get ground based and satellite measurements to agree completely on a signal over 2 decades), and so one must be very cautious about updating one's estimate of mean climate conditions based on a few years of weather. And, then this is an example of the climate scenarios being out of sync with the decision time frame—one plants a crop each season and so a climate forecast for 2030 or 2100 is irrelevant to that decision.

In order for scenarios to be a basis for decision at all, someone must place some likelihood on the result. That likelihood almost certainly is a subjective likelihood and it must be formed by the decision maker, whether the analyst/researcher specifically quantifies or even speaks of likelihood—and whether the analyst/researcher realizes it is happening at all. Much of the inability of the climate research community to get the attention of resource managers is likely due to the fact that when they have gotten these resource managers to listen for a while they often (correctly) subjectively assess the likelihood of the outcomes projected as so low, or so vague, as to have no real information content for the decisions they face. Where there are some successes it is often because decision makers interact closely with the analyst/researchers, questioning the validity of the scenarios, understanding what's behind them, and eventually forming a judgment that there is a high level of confidence in some element of the forecast. Some of the apparently useful 'scenario analysis' in the National Assessment involved earlier snowmelt in the west and its consequent effect on water management. But, realize that this was not simply 'a scenario' or two. It was a finding of many scenarios, and a finding that seemed consistent with reasoning and with observation. So all of the surrounding questioning and analysis associated with 'a scenario' apparently led to a fairly high degree of confidence that earlier snowmelt is happening now, and is a trend that is likely to continue. It is no longer just a scenario but a prediction with a relatively high degree of confidence.

Even with all of that, I'm not sure exactly how or whether this result has yet led to a different investment or management strategy. Many of the hard decisions require more than just the idea that snowmelt will be earlier, but rather how much earlier and how soon, as well as the extent to which the actual quantity of water in snowpack will change. So, here, I suspect there remains considerable uncertainty. Never-the-less, I'm willing to accept the idea that because some water managers have found this information useful (even if one cannot trace the information to a specific change in investment or management approach) that this is a success and evidence that climate forecasts can be useful to decision makers as they try to adapt to climate change. The point is, however, that this information is only useful because there was a substantial investigation that led decision makers/resource managers to believe that this projection was much more than just 'a scenario analysis'.

The only other way that some of this scenario analysis might affect decision is that it might increase uncertainty about the future. There is a variety of empirical and theoretical analysis in economics that shows that an increase in uncertainty should (and does) cause decision makers to forestall large investment or to choose investments that are less irreversible. One area where this appears to have been important was in power plant construction. When demand forecasting proved unreliable after the 1970's and many companies got stuck with excess capacity, the reaction appears to have been to avoid committing to building large new plants. Instead utilities built smaller gas turbine capacity or kept old capacity running. Unfortunately there are confounding factors here such as the NSR regulations that also favored keeping old plants running, and the increasing difficulty of siting new plants. Even for scenario analysis to be useful in warning people about large irreversible investments, however, there must be some confidence that the increased uncertainty suggested by the scenario analysis is real—and not just 'a scenario'. So again, one does not escape the conclusion that 'scenario analysis' cannot be a basis for decision. Scenario analysis can be useful as a first step toward understanding how climate might affect decisions but it would be an extreme error if this program proceeded on the basis that scenario analysis can be sufficient. I think that those in the trenches and working with decision makers realize this implicitly, if not explicitly, but it does no one any good to have this superficial (and wrong) discussion of scenario analysis in this document.

Fifth Overview Comment: This document describes research that if carried out as generally described, would seem to me to require on the order of a $2 billion increase in funding. E.g. the one concrete area where I have some knowledge is in the carbon cycle plan, and here I recently heard rough estimates that this could be done cheaply—but by the time people got done estimating $2 million here, $10 million there…they were up to at least $60 million just for one component of the carbon cycle initiative—and this was taking advantage of a lot of existing programs and infrastructure. Since my understanding is that the total new funding for everything will be more on the order of $40 million, if that, seriously reviewing most of this document is a waste of time because obviously nothing will come of it. Again, many of the climate science areas are well-described, but potentially huge budget items. If you can get a reasonable description and a more reasoned plan for the decision support exercise, I think it is the single highest priority. I would think that it should grow over the course of 2 to 4 years to about $20 million a year—this is apart from the 2 climate center modeling strategy but I see funding for that as separate. So, the new organization might include both a national decision support component and a central and supporting role for an ongoing regional assessment activity, although funding for regional assessment would again be a separate item. So, if in the $40 million one funds the applied climate modeling, regional assessment, and the critical decision support operation there is nothing left for any of the science priorities. The programs are worthy and well-described science research programs, but, let's face it, spreading $40 million lightly across them will hardly make a dent in the needed research. So, there will be virtually no real advance relevant to decision making in any relevant time frame. Suppose for example we actually resolved the difference between satellite and ground based temperature observation, or that we actually had a full array of towers to measure carbon flux from terrestrial systems in the US, over the next 2 to 4 years. How would that really affect our estimates of future climate change. It may take 10, 20 or more years of consistent observation to really make a dent in understanding climate sensitivity, cloud processes, and the like. And, since it appears that no one is really prepared to put up the $2 billion a year really needed to do this well, we probably should invest most of the new funds in finding ways to best utilize our limited knowledge—what we know now, including the limits of what we know—to assist decision making. So that is why I allocate my imagined $40 million to the decision support, applied climate modeling, and regional assessment. With success perhaps then, as the limits of current predictive capacity, and potential implications of climate change and efforts to mitigate it, become better known and are accepted because they are supported by a credible science-based analytical process, the need for the better science and improved observation will be seen more clearly by those who must open the purse strings and come up with $2 billion or so that is needed to do a credible job of the science and observation.

RICE, KANSAS STATE UNIVERSITY

First Overview Comment: Greater linkage should be made between the chapters. The individual chapters are fine but a more detailed document such as that provided with the carbon cycle chapter should be referenced.

Second Overview Comment: I assume there is not an infinite budget. The program should identify the questions that have the greatest need with the available budget. A process should be identified how and who will do the prioritization.

ROCK, UNIVERSITY OF NEW HAMPSHIRE

1. Since the audience for the Strategic Plan is both scientists and stakeholders, two versions of the plan should be made available: a more detailed and fully referenced version (primary sources, summary documents, etc.) for the scientists and a shorter, more general version for the stakeholder. The National and Regional Assessments used this format to good effect, producing a *Foundation Document* for scientists and an *Overview Document* for stakeholders.

2. The impact of poor air quality on carbon cycle function (that is, the biological effects on photosynthetic activities) is a very important issue that needs to be addressed. In the present document it is not included in either of the two logical chapters (Chapter 5 ˆ Atmospheric Composition, or Chapter 9 ˆ The Carbon Cycle). The issue of the impact of poor air quality on the carbon cycle represents a key linkage between the two chapters, and in fact, should also be considered as an important factor for ecosystems as well (Chapter 10).

3. An airborne aerosol monitoring capability is needed as part of an obsrvation network that will identify and characterize atmospheric aerosol sizes, types, and composition. Airborne polarimetry, coupled with hyperspectral reflectance remote sensing, will provide such a capability. Research and development in this emerging field offers a great operational observation tool.

4. An essential component of the "Human Dimension" Chapter (11) will be the communication of key findings to the general public, policy makers, NGOs, etc. This will require an outreach capability that provides lucid and useful information in a timely fashion. Don,t assume that this will be handled by the Outreach Chapter (13). Outreach is a key linkage with all of the chapters, but is especially critical in dealing with the human dimension.

5. The chapter on Reporting and Outreach (13) is vague and lacks substance. In addition, it is too heavily focused on Agency efforts and fails to recognize the fact that the most effective outreach occurs through public/private collaborative efforts. All outreach efforts must be local or regional to be effective. Making use of existing K-12 outreach networks (national, such as *GLOBE,* and regional, such as *Forest Watch*) will allow effective dissemination of climate change information. Care must be taken to ensure that data is converted into information of use to the target audience. It has been said that asking a scientist to speak plain English is like asking a cat to bark. The CCSP effort must identify and utilize "barking cats."

6. It is essential that direct and unbiased treatment of the National Assessment and the accompanying Regional Assessments, funded through the USGCRP, be included in the

CCSP Strategic Plan. To fail to do so represents an unprofessional, unscientific, and unacceptable treatment of a fully valid and extensive outreach effort. How can a document designed for scientists allow such a blatant political re-write of the facts? To ignore this very open and substantive on-going effort undermines all that is central to the scientific process. To pretend it never happened is ridiculous.

ROSSOW, NASA GODDARD INSTITUTE FOR SPACE STUDIES

The plan as currently structured seems to be based on a flawed conception of the climate system: except for brief mention in the Water Cycle chapter, there is no discussion, representation, issue, question or action regarding the Energy Cycle. The climate is an **Energy Engine** (even the biosphere is a system for tapping the energy flows in the system), so that nothing about its state, variability, response to changed forcing or predictability can be understood without understanding the fundamental energy exchange processes. In fact, the requirement for information about the Energy Cycle is scattered throughout all the other chapters. Although I agree that there is a need for a Water Cycle chapter to focus attention on water supply, the Water Cycle cannot be considered as separate from the Energy Cycle. The failure to even mention the Energy Cycle is a profound deficiency that must be rectified if the plan is to be scientifically credible. The Energy Cycle, including the water part of it, is central to the whole climate. Below is some additional text offered to begin the needed revision.

A climate research program aims to achieve sufficient understanding of the climate to be able to predict (as much as is possible) its future, especially in response to human-induced perturbations. The essential logic behind the research activities is that the development of understanding, embodied in models, is to be based on analyses of global observations of the correlated time variations of climate forcing and the climate state (response) and that testing of the fidelity of the model simulations is to be accomplished by comparison of model behavior with the observational analysis results. However, knowing the forcing and the state of the climate as matched functions of time is not sufficient because the climate is not in static equilibrium: there are also unforced variations occurring that can mask or exaggerate the forced changes. Hence there can be climate variability without climate change. Moreover, the occurrence of such unforced variations may indicate the existence of multiple climate states that can be "in balance" with current forcing. In addition there are slower components of the climate system that can greatly delay the complete response or introduce new feedback relationships at a later stage of the climate change. Thus, the analysis of the observations and climate models has to go much further than characterizing the properties of the climate to elucidate the differential and integral relationships among the many climate state variables and explain how these relationships vary with the state of the climate. The analysis of observations must diagnose the global distributions of mass, energy (in its several forms) and momentum as functions of time to **determine the coupling of the "different-speed" components of the climate system** and do this in sufficient detail to separate the forced variations from the unforced variations.

The fundamental Energy Cycle of the climate system begins with absorption of solar radiation, **mostly at the surface**. All other exchanges or transformations of energy (especially involving water) are part of the internal workings of the climate system. In particular, the non-uniform distribution of solar heating at the surface causes motions in the ocean and atmosphere that transport energy (heat and water). All of the energy exchanges and transports ultimately complete the energy cycle by determining the loss of an equal amount of energy by terrestrial radiation, **mostly from the atmosphere**. The surface heating and atmospheric cooling are connected by water: evaporation of water cools the surface and precipitation of water heats the atmosphere. These faster atmospheric processes and transports mediate all of the internal energy and water exchanges among the slower climate components: land/hydrosphere, biosphere, ocean and cryosphere, essentially acting as their forcing on shorter time scales. Thus, **the (joint) atmospheric Energy and Water cycle (including the surface exchanges) is at the center of the climate system's response to any forcing change, providing the main feedbacks, and should be the strategic focus of the current climate research program** before other issues can be dealt with completely.

Since the atmospheric circulation rapidly integrates local forcing differences and couples the local responses of all the other climate components into a single global response, it is essential to diagnose climate variations globally; but the variations of the climate response have the space-time-scale characteristics of the weather. Thus, to diagnose the energy and water exchanges constituting the climate response, observations must have a combination of high space-time resolution and global, long-term coverage that can only be provided by satellite observations. The former is required to resolve exchange variations at the weather-process-level accurately and the latter is required to provide enough examples of the different possible configurations of the climate system to understand the range of multi-variate relationships that are produced by the processes. However, since satellite observations do not provide a complete description of the climate, the only practical approach is to supplement the satellite observations with focused *in situ* observations to improve interpretations and anchor the satellite record with surface-based long-term monitoring. **In other words, the observations from many systems have to be coordinated to form an observing system and the analysis of collection of observations has to integrated to provide the needed detail and long-term, global coverage.**

Significant effort is underway (and should continue) to assemble quantitative measures of climate forcing changes for the past several and the coming decades. Observed variations of the top-of-atmosphere and surface radiation budget for the past 20 years exhibit a number of episodic variations, associated with ENSO and volcanic eruption events, as well as possible decadal scale changes. Therefore, it also makes sense to diagnose in depth the variations (responses) of the climate's energy and water cycles over a similar time period. Such an analysis has to go beyond mere characterization of the climate state variations to diagnosing the energy and water **exchanges** so as to separate forced from unforced variations. Thus, **a <u>coordinated</u> program for the comprehensive analysis of the observed climate variations over the satellite observation period is needed to explain their causes**.

General Comments

Overall, this Plan like all Climate Plans before, does not say anything concrete or specific about how the "climate feedbacks" will be addressed, despite characterizing this as one of the most important issues for the program. I believe that this failure to suggest a concrete course of action results from the fact that the climate research community does not actually have valid methods for analyzing feedbacks, either in observations or in climate models. Hence, little progress has been made on quantifying them or verifying their representation in climate models, even though this has been listed as the highest priority for over 25 years. This subject should be a very high priority item on which CCRI could focus resources: specifically, there is a need to examine the whole paradigm of climate feedbacks in light of progress made in other research fields to understand non-linear dynamical systems. There is a need to invest in developing new, advanced observation and model analysis methods that can handle a large number of variables involved in very non-linear relationships to quantify these relationships and their space-time variations. **Note also that all of the high-priority feedbacks that are mentioned in the current draft are elements of the Energy and Water Cycle.**

T**here is no definition of what a Climate Observing System INCLUDES in this Plan.** Currently, climate research draws on data sources obtained from observing systems designed for other purposes. The Plan suggests that the existing system needs to be **improved**. Many speakers at the recent workshop suggested that the existing system needs to be **replaced** by one designed specifically for climate. I believe that a climate system can be created by **improving** the existing system **BUT ONLY** if the content of the climate observing system is defined in focused way. If the climate system is defined to include EVERYTHING, it is not practical and will never be implemented. The discussion above argues for the central importance at this time of the global Energy and Water Cycle to understanding the climate. Based on this argument, the climate observing system could be defined in three stages: (1) improve the current weather observing system by basing it more completely on satellites and bringing its quality up to that needed for climate studies, (2) add those missing measurements that are needed to monitor the complete Energy and Water Cycle, and (3) then add additional elements to address the issues raised by the atmospheric composition (chemistry) and biosphere questions. Since some elements of all three stages are already in place, these three stages really represent a strategy: emphasis now on improving the quality and completing the measurements of the Energy and Water Cycle, later emphasis on completing the observation system for chemistry and biology (some elements of which are long-lead items that should begin development now).

SARACHIK, UNIVERSITY OF WASHINGTON

1. Uncertainty in the CCRI
2. Other Time Scales in Science and Decision Support
3. Regionality
4. The CCRI
    a. Aerosols
    b. Feedbacks
    c. What actually could be done in four years.
5. The Observing System

6. The Bottom Line

1. Uncertainty in the CCRI

While it is true that policymakers need to know the uncertainty attached to climate information, it is also true that reducing uncertainty is a poor focus for a science program, especially one like the CCRI . Focusing on reducing uncertainty means that the priorities chosen for emphasis are those problems that are most uncertain, in particular aerosols (in particular the indirect effect) and climate feedbacks. The reason these are most uncertain despite having been studied for a long time is that these problems are especially intractable. Therefore to put these particular problems at the heart of the program and expect progress in 4 years is to set up the science program for failure. These topics are more properly a subject for the USGCRP.

When policymakers can define how much certainty they need to make a decision, then perhaps a rational program can be devised around reducing the uncertainty, no matter how long it takes--this is *not* the emphasis of the CCRI. If policymakers cannot quantify the uncertainty they need, it is unreasonable to design a program around the reduction of uncertainty--this simply is not how science is done. In fact is isn't even how policymakers think--policymakers need to know the uncertainties involved in the information they use to make decisions. They also need to know why this uncertainty exists. Policymakers make decisions under these conditions all the time.

Our Secretary of Defense, Don Rumsfeld, in the summer of 2001, stated that because we don't know who our future enemies are going to be nor what the nature of future warfare is going to be, the military budget should be increased by $40 Billion a year. Compare that to the same administration saying that we can't do anything about global warming because the science is uncertain. Clearly uncertainty can be used to justify pre-existing predilections.

2. Other time scales in Science and Decision Support

Scientifically, the year to year variability of the climate is far larger than the subtle trends that would exist over 50 year time scales even if the predictions by the most sensitive models are believed. Society by adapting to these year to year variations (and to decadal variations) will certainly adapt to the longer time scales except for some very special circumstances that only exist on the long scale--eg sea level rise and the slow creep of forests and species.

Further, the natural time scale of decision making is one year: budgets are made each year on every level of government, industry, and private economies, and it would be foolish to design the year to year budget only in terms of 50 year goals. All over the world, the conditions for the next year are paramount, and longer time scales enter less and less as the time scales increase. For the CCRI and the USGCRP to concentrate on long time scales when information on the shorter seasonal-to-interannual time and decadal scales is more relevant to the way societies and economies actually work makes little sense to me, for a program that is supposed to use climate information for decision support. . It makes especially little sense to have the CCRI (with its emphasis on short term results) try to produce information on the longest time scales.

Recognizing that all decisions are made one year at a time means a different approach to decision support. We should not have different decision support systems for each time scale---they should be integrated into a single decision support system that can be informed by climate information on a range of time scales. Scientifically, information on the longest times scales is built up of information collected and analyzed daily---the long time is simply the integration of the shorter time scales. That some forcing is acting on a fifty year time scale has to be reflected in the recognition that the response is expressed as changes of variability on shorter time scales. Vulnerability and adaptation on shorter time scales is the key to understanding vulnerability and adaptation on the longer time scales.

3. Regionality

Only the President and the Congress make decisions involving the entire country. Almost all other decision making takes place on much smaller space scales. Even industries that span the nation devise strategies on more local levels.

Recognizing the regional quality of most decision making, any program that seeks to devise decision support systems using climate information should therefore concentrate on regional climate information on seasonal to interannual to decadal time scales, but should not neglect the longer global warming (50 year) time scale again at regional space scales.

The entire document is sadly deficient at any of the shorter time scales and at the regional space scale. This ignores a decade of what we have learned during the USGCRP about the importance of these space and time scales and ignores the recommendations of a large number of National Research Council documents (especially NRC, 2001a).

4. The CCRI

The CCRI has three major components: reducing uncertainties primarily for aerosols and feedbacks; establishing a long term monitoring system; and doing research on the interactions between environment and society. Of these, only the third can be expected to yield results over the next 4 years but I remain unconvinced that there is a serious effort to actually make progress on this. The RISA program within NOAA is devoted to just such issues and it is poorly funded with no apparent efforts on the part of NOAA or the USGCRP to increase the funding.

a. Aerosols are especially difficult to study because they have short space and time scales. Therefore to fully characterize them would require a large *in situ* observing system combined with remote observations. No one has designed such a system and even if designed, would only contribute to the direct effects of aerosols, not the more uncertain indirect effects which would come under the rubric of feedbacks. Perhaps the most useful thing that could be done over the next four years is for the aerosol community to design such a system and thereby show its feasibility.

b. No-one knows how to study interacting feedbacks except by continued observations and comparisons and interpretations by models. This is a slow and painstaking process that depends on the quality of the continuing observations and the quality of the modeling: both have been found wanting in recent NRC reports (NRC, 1999b, 1999d, 2001b). Not unless we improve both can we expect major increases in understanding (and concomitant decreases in uncertainty)--this is a slow painstaking process and not suitable for the short term goals of CCRI.

While it is possible to monitor specific aspects of the climate, monitoring is not observing---throughout the document there is a confusion about this. Each rawinsonde monitors a specific region of space but it takes the combinations of rawinsondes, surface obs, air reps, satellites, *and models and their products* to make an observing system. There is no way to implement a climate observing system in 4 years--one could , however, *design* it in four years. Starting to implement specific observations without having a design in mind subverts the path to the optimum climate observing system and makes decisions about it arbitrarily. More of this below.

c. So what could be done in four years for the CCRI that would demonstrate the use of climate information for decision support and would serve as proof of concept? I contend that on the seasonal-to-interannual time scale, almost all of the components of such a decision support system exist and only need to be put together. The IRI and NCEP makes forecasts on a regular and systematic basis. The ENSO Observing system exists but needs to be organizationally integrated with the modeling and prediction efforts. The one piece that doesn't quite exist is the delivery system that makes climate information available and interpretable in a useful manner--this could be demonstrated (but not sustained) by the RISAs---ultimately a set of regional climate information centers would be required (NRC, 1999c).

5. The Observing System

We make climate observations by sub-systems designed for other purposes. We do have a weather observing system, but we do not have a climate observing *system*. Why is this so? Because the Weather Service delivers products that customers find useful and are therefore willing to support. The weather observing system grew because two powerful customers, the military and the airlines, needed the products that the weather observing system, combined with the models and model products (as analyses and predictions), produced.

I do not believe that a climate observing system, no matter how many scientists or the National Research Council call for it, can be sustained without being part of a system that delivers *useful* climate products. The research community can design and implement such a system but cannot sustain a system for long because the maintenance of the system is not research. We currently do not have the operational infrastructure that can maintain a climate observing system----indeed the weather observing system does not satisfy the 10 commandments of climate observations and therefore can not even be considered a component of the climate observing system. Until we do have such an operational organization for climate, attempts to sustain an climate observing system within the research community are doomed to failure, both by culture and by the ultimate lack of support for the observing system due the failure to deliver useful products, again impossible to sustain in the research community.

I also do not believe that, even if such a useful product delivery system were to exist, that more than one sustained climate observing system can exist. We will not have an ocean observing system for ocean uses and an ocean observing system for climate uses. We will not have an observing system for global warming and one for seasonal-to-interannual prediction. The implication is that we should be designing and implementing the grand climate observing system as part of a useful product delivery system, not an ocean observing system divorced from climate. Nor should we be implementing pieces of the climate observing system piecemeal since how we start a climate observing system will determine what sort of observing system we have--it will not be the one we would have designed. Nor should we be designing the grand climate observing system independent of the climate models that produce the products, nor independent of the climate delivery system and the customers.

6. The Bottom Line

The parts of the document on the USGCRP are OK but the Pathways report was far more comprehensive, better thought out, and more important, to a large extent unimplemented (NRC, 1998, 1999a).

But I don't believe that the authors of the document have thought carefully enough about what a decision support system informed by climate information really means. In particular, the climate organization within government is primarily a research organization while much of what needs to go into a decision support system is operational. Therefore there are not feds on the ground who have the expertise to design this system. Only the NOAA Climate and Global Change Program has had the design of a Climate Service as its mission and its contributions to this document are relatively invisible.

A decision support system is (or should be) a Climate Service. It should be thought of as permanent—it is not a one shot deal. It must include an integrated observing and modeling system for climate and must have a delivery system for decision makers. Its relationship to research is essential and must be carefully designed. It will not be cheap (I estimate 1-1.5 Billion dollars/year). It can not be done piecemeal—the observing system and modeling system must be carefully integrated both scientifically and organizationally and neither will have the public support to provide the funding unless it delivers useful climate information products—the more varied on all space and time scales the better. It should not cannibalize the USGCRP—research will always be needed.

What CCRI can do is *demonstrate* the operating concepts of such a Climate Service using elements that are already in existence. The USGCRP part of the CCSP should be improved along the lines already suggested by the Pathways document (NRC, 1998, 1999a). If both of these are done, we will have made real progress.

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SAGARIN, UNIVERSITY OF CALIFORNIA, SANTA BARBARA

I am pleased to have the opportunity to comment on the President's Climate Change Science Program (CCSP). I view the program in its current form as a "smoke and mirrors" tactic that squanders the opportunity to meaningfully take action to prevent future climate change. There are three primary areas in which I feel the program is insufficient:

1. The budget for new science programs is woefully inadequate. 2. The focus of the program is on future technologies and adaptation strategies rather than on current opportunities to mitigate against future climate change. 3. The leadership of the program has tainted its scientific credibility by acting in a highly politicized, rather than scientific, manner at high level planning workshops. My comments are based on both the Strategic Plan for the Climate Change Science Program and the Climate Change Science Program Planning Workshop for Scientists and Stakeholders (3-5 December, 2002).

Budget

The CCSP is a bold and welcome scientific plan for studying climate change that is negligently under-funded. Recently, the Intergovernmental Panel on Climate Change and the U.S. National Academy of Sciences concluded that while the balance of evidence suggests that human activities are contributing to climate change, large uncertainties remain in our overall understanding. Accordingly, the CCSP calls for ambitious new programs and analyses that will build on the current research base, with the goal of reducing these uncertainties. Unfortunately, the administration has provided almost no new funding for such a research program. Since Fiscal Year 1995 when funding for the U.S. Global Change Research Program reached a peak of $1.72 billion, funding has remained flat or fallen to a FY '03 request of $1.71 billion. Even with the $40 million budgeted in FY '03 for the President's new Climate Change Research Initiative (CCRI), total funding for climate change research is less than 2 % higher than FY '95 levels. If the administration's primary strategy with climate change is to study the issue further while it refuses to properly fund such study, it can only be concluded that the administration has no climate change policy. Reviewer's name, affiliation: Raphael Sagarin, University of California, Santa Barbara

Focus

I believe that the program focuses on the wrong questions first. This is because the entire program is predicated on the untested assumption that mitigating for climate change now will be too expensive for the economy. There is almost no emphasis on currently available technologies to reduce greenhouse gases (GHGs), nor is there any call to identify the current economic, social and political barriers that limit our ability to mitigate for future climate change. In short, until we fully explore what is keeping us from reducing GHG emissions in the present, there is no reason to make huge investments in uncertain strategies for the future.

Thus, the very first study of the CCSP should be a proper accounting of the total costs and benefits of our current Carbon intensive economy relative to a less Carbon intensive economy. No such study is called for in the present draft. There are at least six large categories of costs from our Carbon-based fuels dominated economy that are currently not factored into electricity bills or pump price of gasoline and that are borne by American taxpayers:

1. Health costs due to asthma and other lung diseases, especially when those health effects are disproportionately felt by poor populations who are less likely to pay for private health care. 2. Degeneration of air and water quality which translates into additional costs for water treatment and reduced value of views for property owners and visitors to National Parks. 3. Effects of toxic chemicals and waste water heating of power plants on wildlife populations and habitats. 4. Dependence on foreign oil from countries that support terrorism and global instability leads to increased expenditure for terrorism prevention and response as well as the cost of waging war in these countries. 5. Economic cost of climate change including increased insurance claims due to sea level rise and increased climatic instability, loss of crop production due to range shifts of crops or their pollinators, reduced output of hydro power due to prolonged drought conditions and potential for catastrophic damages due to sudden climatic shifts. 6. Direct taxpayer subsidies to fossil fuel energy producers. Direct subsidies through tax breaks and direct funding for coal, oil, and gas industries amounts to nearly $3 billion per year. These amounts may go even higher if the House and Senate reintroduce and pass provisions of the comprehensive energy bill that were introduced in the last Congress.

Repeatedly Administration officials at the workshop stated that we need to grow our economy to find ways to reduce GHG at some point in the future. A similar theme pervades the entire CCSP Draft Strategic Plan. It is scientifically and economically groundless to pursue unknown future benefits of technology without fully exploring today's options for reducing GHGs.

Notably, there is no mention in the entire Draft Strategic Plan of "no regrets" scenarios. These scenarios would focus on strategies beneficial for our economy and environment regardless of the ultimate effects of climate change. "No regrets" strategies include promoting energy efficiency programs that pay for themselves through energy savings and programs that reduce health hazards of pollutants while also reduce GHG emissions. Such strategies should be given first priority in any discussion of climate change.
Reviewer's name, affiliation: Raphael Sagarin, University of California, Santa Barbara

Leadership
I am deeply concerned about Secretary of Energy Abraham's ability to impartially lead a Climate Change Science Initiative. In his keynote address at the Workshop for Scientists and Stakeholders (3 December, 2002), Secretary Abraham primarily promoted President Bush's voluntary proposal to reduce GHG intensity by 18%. The Secretary claimed that

this proposal would reduce carbon emissions by roughly the same amount as adoption of the Kyoto Protocol principles would. This is a scientifically indefensible statement that taints the entire CCSI program from the start by using misleading statements and highly questionable assumptions. There are three main reasons why the President's plan reduction of GHG intensity will not reduce GHG emissions: Ö GHG intensity was already reduced 17.4% in the U.S. during the 1990's. During this time GHG emissions rose 14% due to economic growth. Under economic growth scenarios forwarded by the Bush Administration, the plan to increase GHG intensity 17.5% by 2012 will result in another 14% increase in GHG emissions. This would mean a total of 28% increase in emissions by 2012, a far cry from the 6% decrease called for in the Kyoto protocol. Ö Part of a reduction in GHG intensity for any one country really means a shift of the most GHG intensive industries to another country (e.g., manufacturing shifting from the U.S. to Asia). Because climate change is a global issue, these shifts do nothing to protect the U.S. or any other country from the effects of continued GHG emissions. Ö The plan sets no firm compliance targets or penalties for failure to meet the goal. Thus, there is no guarantee that even the inadequate targets set in the program will be met.

There was no reason for Secretary Abraham to promote this policy at a workshop aimed to get scientific input on the CCSP. That he did so suggests a continuing disdain for the scientific community and for stakeholders that seek to promote actions that will lessen the potential impacts of climate change on our economy, environment and society.

SAWIN'S, JANET AND FREYR - WORLDWATCH INSTITUTE AND INDEPENDENT ENERGY CONSULTANT

First Overview Comment:

1) The premise of this Strategic Plan and the subsequent Science Conference are fundamentally flawed because they ignore at least one-half of the topics that are an integral part of what otherwise would be a sound Climate Change Research Program.

Only a half of the pie, if that, has been offered for discussion: remaining scientific uncertainty on climate change. The Strategic Plan neglects the need to examine and discuss *mitigation* options in the areas of *policy* and *technology*. Equal thought, effort, and openness must be given to the debate on research and implementation of mitigation as is being given to the science of global change. Policy and technology should have a forum of discussion similar to the Science Conference and those topics should be treated as equals to the topic of scientific research on climate change in the context of public discourse. The Administration calls on scientists to provide "sound science" but it presumes its own "sound policy". The truth is that the U.S. government has a poor record on picking winners among different technologies.

The United States needs a full and open discussion regarding not only which technologies we should pursue and invest in as possible solutions to the climate problem, but also the best means for developing and diffusing these technologies. This includes a thorough study of the climate and energy policies of other countries around the world, particularly Japan and members of the EU, and examination of which policies have been most successful to date.

2) The Strategic Plan and the accompanying Science Conference are intellectually dishonest in their construct because they serve a political purpose before they serve the science.
To hold a Science Conference without a similar opportunity to discuss policy and technology solutions serves a political purpose: It frames the "debate" on climate change exclusively in the context of climate science, and carries the message to the American People that climate change is solely an issue of continuing scientific uncertainty. After all, scientists and science conferences, by definition, deal with remaining issues of uncertainty, and not issues of established fact. This gives the citizen the impression that there is no room or need for discussion on what constitute appropriate policy responses and technological solutions to the well-established threat of global climate change.

Similarly, the Science Conference took political advantage of the 1,200 or so scientists who flocked to the feeding troth of Federal science funding, playing into the hands of a political leadership that wishes to obscure the critical policy issues under the shroud of remaining scientific uncertainty.

3) The Administration's climate strategy exposes an ideological bias when it examines the costs and risks of mitigation while ignoring the costs of climate change impacts and adaptation. A thorough study and understanding of the market and non-market costs of potential impacts and adaptation must be included in the climate science strategy.
The research program must include a thorough study of the potential costs of adaptation, and of the costs of changes and events to which humans, societies and ecosystems cannot adapt. This should include worst-case scenarios (as do many estimates of mitigation costs that are cited by the Administration). It must also include non-market costs such as loss of biodiversity, and the human dimensions of climate change – impacts on health, water availability and quality, agriculture, and even the potential of climate change impacts to increase conflict worldwide.

It must also take into account the level of ability to adapt – not only in the United States, but also elsewhere around the world, and particularly in the developing world where many people require a stable climate for their very survival, and thus may not be able to adapt at all.

4) The Strategic Plan should not oversell the ability to reduce or eliminate uncertainties.
We won't know the full impacts of climate change until we are forced to live with them.

5) The Strategic Plan should recognize that despite remaining uncertainties in the area of climate change science, such uncertainty is not a reason to delay mitigation efforts and discussion on appropriate policy options.
All policy decisions are made in the face of uncertainty. The U.S. government must *at the very least* begin to take "no-regret" steps to reduce emissions.

6) The Strategic Plan should recognize the significance of path dependence and the risk of delayed action on mitigation. What will be the cost of delaying action for 10 years?

The sooner we begin to reduce emissions, the easier and less costly it will be. Similarly, the longer we continue forward with business-as-usual, the more we will lock ourselves, and others around the world, onto an unsustainable energy path dependent on fossil fuels and nuclear power, while possibly locking ourselves out of potential options and solutions. The plan should include an examination of these risks, to be completed within the first one to two years at most.

7) The Strategic Plan should recognize that U.S. energy policy is an integral part of the discussion on global climate change and that the topic of climate science should not be artificially divorced from policy considerations.

The U.S. government is investing billions of taxpayer dollars into research and subsidies for energy technologies annually, without any discourse or "public comment period" on how the technological initiative funds are being spent. All U.S. energy and climate experts have the right and a duty to play a role in the decision-making process regarding how such resources are allocated and which potential energy technology options are pursued.

8) The Adminitration's Plan is unjustifiably dismissive of non-conventional energy sources that arguably hold the key to the problem of climate change.

Many in the Administration have spoken about the high costs of renewable energy and the long time period required for these technologies to enter the market place. Yet although the majority of government subsidies and R&D funding has consistently gone to fossil fuels and nuclear power over the past several decades, and despite the fact that regulatory structures have erected as many barriers to renewables as some have tried to break down, in the United States and elsewhere, renewables are the fastest growing energy technologies in the world. Even without including external costs, some are now cost-competitive or nearly so with some conventional energy technologies.

There is also talk about their inability to provide the energy needs of industrialized societies. But renewables now provide more than 20 percent of Denmark's electricity; wind provides 4 percent of Germany's electricity, and 2 percent of Spain's.

In September of 2002, the German government presented a scenario for the nation's energy future which suggests that Germany can reduce its CO_2 emissions by as much as 80 percent (relatively to 1990 levels) by 2050 with efficiency improvements, renewables, and combined heat and power generation. If the government of Germany, the world's third largest economy, can envision such a future, the U.S. government should be able to consider targets that are at least half as ambitious.

In addition to studying the future use of fossil fuels, the plan should look seriously at the potential role for renewable energy in achieving a stabilization of atmospheric GHG concentrations.

9) The same effort applied to overcoming uncertainties in the science of climate change should be applied to overcoming uncertainties regarding potential solutions. It is essential to look at the full potential costs and consequences of all possible options – potential health implications, safety issues, and other non-market and market costs.

The U.S. government is going all-out with efforts to promote “clean coal” linked with carbon sequestration, and nuclear power as the favored solutions to climate change. This is despite the fact that other options – energy efficiency and renewable energy – are available today. These technologies do not threaten the global climate, and they produce minimal to no air, soil or water pollution, do not damage human health, do not rely on imported fuels, but instead they can increase U.S. energy and national security, and create new jobs and attract investment. Some of these technologies are now cost-competitive with conventional energy technologies, even excluding external costs of conventional fuels and the extremely high cost of carbon sequestration.

Sequestration is very expensive, in the words of David Garman, and it is uncertain whether it is ever going to be technologically or economically viable. Studies must include the impacts of leakage of sequestered carbon, because even “minor” leaks of 1-2 percent of total sequestered carbon would have significant and severe impacts on the global climate if we were to rely heavily on this option. Nuclear power brings its own environmental, social and security threats.

The Administration’s policy to pursue an astronomical rate of cost-reduction for carbon sequestration, while goals for renewable energy remain far less ambitious, is misguided. Considering the potential of renewable energy to solve the problem of climate change, and its many other benefits, the current focus and spending on sequestration is wasteful and indefensible.

10) The Strategic Plan must build on work of the National Assessment and the IPCC findings and reports, not only work of the NRC.

First Additional Comment:

1) We recommend that Dr.s John Christy and Frank Wentz be encouraged to exchange data and calculations to find the source of disparities in their conclusions regarding tropospheric temperature records.

2) We object to misleading, trivial, and erroneous information included in the summary of the conference breakout session on “Stabilizing Greenhouse Gases in Earth’s Atmosphere: Opportunities for Technology and Innovations.”

The summary mentioned that thousands of gigawatts of wind capacity could result in regional climate impacts by capturing the energy of the wind. The entire electricity generating capacity of the United States does not amount to even one thousand gigawatts. More importantly, the speculation that wind turbines would ever significantly change regional climate is silly and trivializes the discussion.

3) To say that President Bush is committed to the UN Framework Convention on Climate Change, and that the Bush Administration plan results will be comparable to what others achieve under the Kyoto Protocol, is to belittle a very serious global problem and to trivialize the serious efforts of other countries to reduce their GHG emissions.

SCHOEBERL, NASA

The plan as presented by Dr. Mahoney at the fall AGU did not talk about the incorporation of new scientific results into the assessment/policy/planning activity. It seems to me that "climate science" still has a number of uncertainties (such as the role of black carbon aerosols in heating) which will need to be sorted out in the next few years. This information, when available, may have a large impact on both the assessment and subsequent policy. How will the plan incorporate "updates?" It seems to me that - like the IPCC - the science should be evaluated on an annual or bi-annual basis and that new information should be fed into the planning documents and recommendations. The plan has to be a "living document." All too often, when policy begins, science stops (e.g. acid rain research). We should avoid that pitfall.

SCHWARTZ, BROOKHAVEN NAT'L LAB

The US Climate Change Science Program is to be commended for undertaking to prepare this Strategic Plan and to open the process to wide input from the scientific community. It is to be hoped that what will emerge out of this process is a plan that will meet the requirements for scientific input into the many decisions that need to be made regarding climate change policy.

In my judgment the document as presented is NOT a strategic plan. A strategic plan must state specific target goals and objectives and then set out a path to meet them. This document does neither.

The document is unacceptably non-specific in both the requirements of and activities of the CCSP. Examples from the high-level section entitled "The Research Program" (Introduction, beginning page 8):

- "Research carried out under the auspices of the CCSP addresses a diverse set of topics."(Page 10). The statement is diffuse and not objective driven.

- In the list of research immediately following, first bullet: "Improving the understanding of driving forces of climate and global change, including natural forces such as solar variability and human forces such as changes in land cover and emissions of greenhouse gases and aerosols." Note the diffuseness: "Improving". without specification of the required improvement; "Including". generally indicative of not identifying the crucial issues. "Such as" (twice), the same.

- Next item; "The atmosphere and its role in integrating climate forcing factors, including the roles of emissions of different atmospheric constituents." Note again lack of specificity, lack of quantitative requirements. Rather a vague study of the atmosphere and its role, again with the non specific "including".

The document is replete with such diffuse, non-specific statements. These are just a few examples of many.

General Comments

The Strategic Plan needs to be driven by requirements. The overall objective must be specified. The Strategic Plan is then a statement of what must be done to achieve that objective and how it will be done.

Need for reporting uncertainties

Page 11: "CCSP analyses should specifically evaluate and report uncertainty." This is a strong plus in that it implies a quantitative product. What is missing is a specification in the quantity or quantities for which the uncertainty should be evaluated or specified.

Contrast this plan with the NRC *Climate Change Science* Report

The Draft Plan justifies particular research components in large part on the NRC *Climate Change Science* Report (2001). That report provides strong justification for understanding and developing predictive capability for atmospheric composition:

> Predictions of global climate change will require major advances in understanding and modeling of the factors that determine atmospheric concentrations of greenhouse gases and aerosols: a) future usage of fossil fuels, b) future emissions of methane, c) the fraction of the future fossil fuel carbon that will remain in the atmosphere and provide radiative forcing versus exchange with the oceans or net exchange with the land biosphere.

Note use in the NRC report of the strong verb "require", in contrast to the weak and diffuse language of the Draft CCSP plan as cited above and throughout.

In view of the deficiencies noted above the following *Statement of Objectives of a Strategic Plan* and the basis for them is proposed for consideration.

Concentrations of CO_2 and other long-lived greenhouse gases have increased substantially over the industrial period, and are expected to continue to increase as a consequence of fossil fuel combustion and other activities. There is substantial model-based indication and some observational evidence of global warming and other climate changes associated with these increases in greenhouse gases. The present understanding of the sensitivity of climate to changes in GHG concentrations is rather uncertain precluding confident projection of the response of future climate to continued increases of GHGs. This situation is unsatisfactory for the nations of the world to make decisions regarding either approaches to reduce greenhouse gas emissions with resultant implications on the energy economy of the world, or approaches to adapting to climate change as it occurs.

In order to plan for dealing with climate change by one or the other of these approaches it is necessary to know (with quantified confidence) what the expected climate change will be. This requires knowledge of:

1. Expected forcing of climate change in the future, i.e. changes in radiative flux components responsible for driving climate change. This requires knowledge of concentrations of atmospheric constituents that are responsible for driving climate change, which in turn depend on the emissions of these constituents (which must be assumed, subject to many constraints), their subsequent residence in the atmosphere, and the specific forcing associated with unit amount of material in the atmosphere; and

2 Climate change per unit forcing. The first-order measure of climate change is the global mean temperature. The climate change per unit forcing is denoted the climate sensitivity. Climate sensitivity includes effects of feedbacks. There is some ambiguity in the implicit time frame for climate change. There are time lags of climate response to forcing. The implicit time frame here is over "relevant time scales" which are decade to century.

Present knowledge of the climate sensitivity is based almost entirely on calculations with climate models, mainly models that have attempted to represent climate change over the industrial period, for example by ramping atmospheric concentration of CO_2 and other constituents or otherwise representing the time dependent forcings of these substances. It is these models that yield the factor of three range in estimated climate sensitivity. As noted, confidence in the models can be gained only by comparison with observation. It is claimed (by IPCC and others) that comparison which has been conducted is sufficient to place confidence in climate model predictions. However information presented elsewhere by IPCC shows that present global forcing relative to preindustrial times is sufficiently uncertain to preclude determination of climate sensitivity from temperature change over the industrial period with anywhere near the requisite accuracy to permit decision making and planning by the nations of the world with a high level of confidence. Therefore a key requirement of the Climate Change Science Program must be to

Determine the climate sensitivity with sufficient accuracy and confidence to permit decision making and planning to limit emissions of greenhouse gases and/or to develop strategies to adapt to future climate change.

An estimate of the requisite uncertainty is developed below. As for confidence, this can be gained only from observation. Specifically, to the extent that climate models are used as the basis of determining this sensitivity, then confidence can be gained only through evaluation of these models by comparison with observations.

Possible ***approaches*** are as follows.

1. **Development of the perfect climate model**. Such a model will represent all elements of the climate system with sufficient accuracy that the results of this model can be used with complete confidence. Such a model is an abstraction, to be approached but never realized, certainly not realizable on time scales of interest here. In principle, as the model would be perfect, no evaluation would be required, but prudence would require evaluation even of such a perfect model.

2. **Development and evaluation of imperfect, incomplete climate models**. Development of such models will be guided by the desire to represent the essential elements of climate and climate change. Confidence in the use of such models will be gained by the accuracy with which they represent past and current climate and climate change. Model evaluation should examine all aspects of the climate system for which predictive capability is desired, such as local climate change, local hydrology, and the like to gain perspective in the accuracy of the model for such application.

3. **Considerations of forcing and response**. The forcing and response concept has been useful in considerations of global-scale climate change, key being global mean temperature. Forcing is total change in all energy fluxes generally calculated at the top of the tropopause, and response is change in global mean temperature, though other global-average response indices might be developed. Virtually all climate model studies to date show equivalent or near equivalent global mean

temperature change for a given global-mean forcing, irrespective of the nature or geographical distribution of the forcing. The climate change sensitivity □ is defined as the response per unit forcing. This sensitivity may be obtained as an output from climate models, in which case the accuracy must be evaluated by comparison with observations; alternatively it may be wholly empirical; but in any case it cannot be derived entirely from models available now or in the foreseeable future. Application of the Forcing-Response approach requires assumption of a linear relation between forcing and response. This is a concern given the complex nonlinearity of the climate system and the known properties of nonlinear systems to exhibit highly nonlinear response to perturbations, so at best this approach is appropriate only for small perturbations. Confidence in the Forcing-Response approach can be gained only by examination of its accuracy in representing past climate change, especially climate change over the industrial period.

It must be stressed that application of any these approaches requires accurate knowledge of the forcing of climate change over the industrial period.

It is recommended that the Strategic Plan explicitly take cognizance of these approaches; perhaps others may be identified and added to the list. It is further recommended that Approach 1 be viewed as unattainable for the foreseeable future and therefore that the Plan specify proceeding along Approaches 2 and 3.

Accuracy requirements for predictions of future climate change permit specification of required research objectives

Present "commonly accepted" (IPCC, 2001, p 13) estimates of the sensitivity for global temperature change per doubling of CO_2 (conventionally taken as 4 W m^{-2}) range from 1.5 to 4.5 K; that is, the uncertainty range $R = 3$. Virtually all who are concerned with climate change would concur that this range is unacceptably large to be useful for planning mitigation or adaptation policy. It makes a great difference whether the temperature in a doubled-CO_2 world will be 1.5 K or 4.5 K greater than preindustrial, given the fact that the change in global mean temperature from ice age to interglacial is 5 K.

This model-based uncertainty may be reduced either through identifying a subset of the models whose results for the industrial period agree with observations within uncertainties, or by empirical determination of sensitivity coefficient λ. By either approach it is necessary to know the forcing over the industrial period F to much better accuracy than is known at present (see below).

Consider the empirical approach to determining λ, here taken in units K per (W m^{-2}) and evaluated as $\lambda = \Delta T/F$ where ΔT is the temperature increase over the industrial period or some other calibration period. Under assumption that the uncertainties in ΔT and F are uncorrelated, the fractional uncertainty in λ is

$$\frac{\delta\lambda}{\lambda} = \sqrt{\left(\frac{\delta\Delta T}{\Delta T}\right)^2 + \left(\frac{\delta F}{F}\right)^2}$$

According to IPCC (2001, p. 2) the increase in global mean temperature over the industrial period is 0.6 ± 0.2 K; *i.e.*, the estimated fractional uncertainty is $\lambda\Delta T/\Delta T = 0.33$.

General Comments

What is a reasonable target for fractional uncertainty in sensitivity, $\delta\lambda/\lambda$? This can be addressed by noting that a fractional uncertainty $\delta\lambda/\lambda$ results in an uncertainty range *R* for a given future forcing

$$R = \frac{\Delta T_{\max}}{\Delta T_{\min}} = \frac{1 + \dfrac{\delta\lambda}{\lambda}}{1 - \dfrac{\delta\lambda}{\lambda}}$$

This can be inverted to yield the requisite fractional uncertainty in sensitivity required for a specified uncertainty range in temperature change, *R*:

$$\frac{\delta\lambda}{\lambda} = \frac{R-1}{R+1}$$

For the value of $R = 3$ claimed by IPCC, $\delta\lambda/\lambda = 0.5$, and for $\delta\Delta T/\Delta T = 0.33$, the implied fractional uncertainty in forcing is $\delta F/F = 0.37$. As we shall see this is far less than the uncertainty in physically based estimates of forcing,

Now consider the required uncertainty in climate sensitivity. For uncertainty range $R = 2$, which may still be on the high end of the range that is useful for policymaking purposes the requisite fractional uncertainty in climate sensitivity is $\delta\lambda/\lambda = 0.33$, and for $R = 1.5$, which may be closer to the required uncertainty range, the requisite $\delta\lambda/\lambda = 0.2$.

How does this translate into required uncertainty in forcing over the calibration period. Assume that by future research the fractional uncertainty in both temperature change and forcing over the calibration period can be reduced to the same value *f*. Then the required fractional uncertainty in both □*T* and *F* over the calibration period is

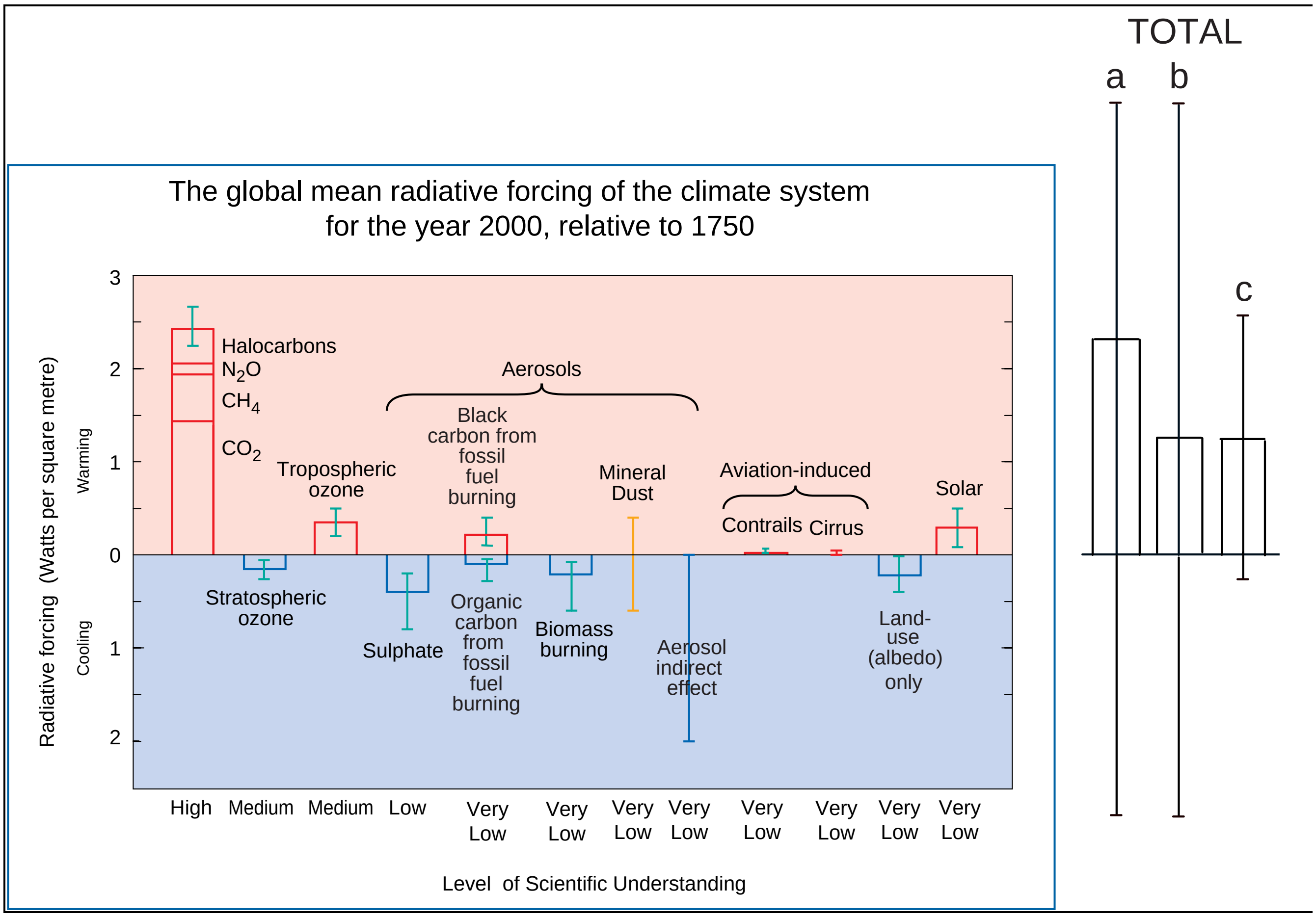
$$f = \frac{1}{\sqrt{2}}\frac{\delta\lambda}{\lambda} = \frac{1}{\sqrt{2}}\frac{R-1}{R+1}$$

Hence for an uncertainty range in $\lambda\Delta T$ corresponding to a doubling of CO_2 $R = 2$, the fractional uncertainty in both forcing and temperature change over the calibration period *f* must be 0.23, and for R = 1.5, $f = 0.14$. There can be slight trade-offs; a slightly lower uncertainty in ΔT would allow a slightly higher uncertainty in *F* and vice versa.

The purpose of the present analysis is not to specify the requisite value of *R*; that must be done by other groups contributing to this *Strategic Plan* taking into account the amount uncertainty *R* that would permit useful policymaking. Rather the present analysis is a means of translating a required value of *R* to a required uncertainty in forcing that must be achieved by research on atmospheric composition and atmospheric physics relating composition to forcing.

The value for the present fractional uncertainty in forcing over the industrial period $\delta F/F$ 0.37 inferred above and the lower values that might be required in the future should be compared with the values in current physically based estimates of these forcings. Here reference is made to the figure below, adapted from IPCC (2001). The figure shows the estimates, from IPCC (2001) of the several components of radiative forcing over the industrial period [indicated by the distance of the top (positive forcing, warming), or bottom (negative forcing, cooling), of the bar from the zero line] and the uncertainties associated with those estimates (I-beams). Note that for the long-lived greenhouse gases the fractional uncertainties are rather small, whereas for tropospheric ozone and even more so for the several aerosol components these fractional uncertainties are much greater. In some instances the IPCC declined even to provide a best estimate of the forcing, offering only an uncertainty range, somewhere within which the actual forcing lies. At the bottom of the figure is the IPCC working group's assessment of the level of scientific understanding associated with each of the estimates, a qualitative measure of the confidence that can be placed in the estimate. It is evident from the figure that the several aerosol forcings are individually and collectively the greatest contributions to the uncertainty in the total forcing.

At the right of the figure are several estimates of the total forcing and of the uncertainty in it; again the bars denote total forcing and the I-beams the associated uncertainty. In (a) only the forcings for which the IPCC gave actual estimates are used in calculating the total forcing, which is obtained as the algebraic sum of the several component forcings; in (b) and (c), for those forcings in which only a range was given, the central value of the range is used. In (a) and (b) the uncertainty in the total forcing is estimated from the algebraic sums of the maxima and minima of the I-beams for each of the component forcings; in (c) the uncertainty in the total forcing is estimated from the square root of the sums of the squares of the departures of the maxima and minima of the I-beams from the estimates of the several component forcings. Several features should be noted. First, the estimate of the total forcing over the industrial period is greatly reduced (from 3.3 W m^{-2} to 1.2 W m^{-2}) if the central values of the estimated ranges of mineral dust forcing and aerosol indirect forcing are included in the estimate of the total forcing instead of being taken as zero. Second, no matter what the approach to the estimate, the uncertainty in the total forcing is greater than 100%; that is the maximum of the uncertainty range is more than twice as great as the estimated total forcing, and the minimum of the uncertainty range is less than zero (i.e., net cooling forcing).



Components of radiative forcing over the industrial period and associated uncertainty ranges as estimated by IPCC (2001) (in box) and estimates of the total forcing (evaluated as algebraic sum of component forcings) and associated uncertainty (at right of figure). *a*) Forcings for which no value is given by IPCC are set equal to zero; uncertainty range is estimated as algebraic sum of minimum and maximum for each component forcing. *b*) Forcings for which no value is given by IPCC are set equal to midpoint of uncertainty range; uncertainty range as in *a*. *c*) ; Forcings as in *b*; uncertainty range evaluated as square root of sum of squares of difference between maximum (or minimum) of uncertainty range and estimated forcing, summed over the several component forcings.

The uncertainty in forcing represented in the Figure may be compared with the value for this uncertainty of 37% implicit in the IPCC analysis presented above. The uncertainty in physically based estimates of this forcing greatly exceed those inferred from IPCC statements regarding climate sensitivity and uncertainty in temperature change over the industrial period.

Comparison of the present situation represented in the Figure with the potential target objectives of 23% or 14% uncertainty in forcing over a calibration period shows that there is a long way to go to achieving such objectives. It is clear that much effort will be required to narrow this uncertainty. The figure does, however, point to some important directions, namely to focus on reducing the greatest sources of uncertainty, those associated with the aerosols generally, especially the aerosol indirect effect and direct forcing by mineral dust. Therefore research efforts should be targeted to improved quantification of these forcings.

Suffice it to say that no analysis of this sort appears in the Draft Strategic Plan, but that such an analysis must be included in the Plan if it is to be used in any meaningful way to guide research to improving confidence in prediction of future climate change.

The foregoing focuses on global mean temperature as the first-order index of climate change. In principle a similar analysis should be carried out for other climate change indices of concern, e.g., change in global average precipitation, possible regional changes, and the like, but such analyses would be more difficult. Perhaps a focus on change in global mean temperature might be enough to define initial requirements, with additional requirements added along the way as progress is made toward developing the needed predictive capability for global mean temperature.

DIAN SEIDEL, NOAA AIR RESOURCES LABORATORY (R/ARL)

Comment 1: I attended parts of the workshop and was pleased to see that many of my own general feelings about the workshop and the draft plan were heard and were echoed in the moderators, reports and in the final session by the panelists and Dr. Mahoney. I greed with the view that the workshop was exceptionally well run and gave the impression of being truly an opportunity to glean input from the entire community. I appreciated the opportunity to participate and provide comments on the plan. Key among my concerns are: (1) the need for more specificity in the plan; (2) the apparent (or assumed) lack of significant new resources to address a fairly significant set of observational needs and research problems; and (3) the focus on observational needs and understanding uncertainty, which, while important, tend to suggest that we know and understand less that we actually already do. However, I applaud the serious language devoted to observations, which currently are quite inadequate for monitoring long-term climate changes. Addressing this deficiency requires a sustained long-term investment, though, and that means a commitment of resources well beyond what is currently allocated.

Comment 2: One particular concern of mine was the focus on the tropospheric vs surface temperature trend disparity issue. This one science issue was given special treatment in the plan and at the workshop, which otherwise addressed very broad programmatic and thematic topics. This is an area in which I have some background, and, while I think it is still an interesting area for fruitful research, I don,t believe it merits the kind of attention given by the plan. It is no longer a wildly controversial topic. Indeed it never was wildly controversial among the scientific community but was manipulated by journalists and by people with political agendas to appear more controversial than it was. It troubles me that the framers of the CCSP continue to promulgate the notion that this single open question in climate science is so overriding. There are other climate science questions that present and equal or greater intellectual challenge, and by focusing on this one, we give the others short shrift.

SEVERINGHAUS, SCRIPPS INSTITUTE

My major comment on the draft plan is that it should emphasize a glaring problem with the current world-class climate models: failure to simulate abrupt climate change. These models without exception fail to reproduce the magnitude and areal extent of abrupt climate changes, that are known to have occurred from the paleoclimatic record. Robust decadal-resolution temperature information has been obtained from the Greenland ice core records over the past 100,000 years (Alley et al., 1993, Nature; Cuffey et al., 1995, Science; Severinghaus et al., 1998, Nature; Severinghaus and Brook, 1999, Science). These records show that central Greenland warmed 10 ±3 C on multiple occasions. The duration of these changes was less than 10 years in many cases, a conclusion with high confidence based on the identification of annual layers in the ice core record. Similar records of somewhat lower confidence have been obtained from ocean sediment cores and tropical ice cores, and these show that the same abrupt changes occurred over most of the globe (Clark et al., Nature 415, 863-869 (2002)). Models in contrast produce changes of only several degrees in Greenland and much less elsewhere.

The fact that the models do not reproduce the correct magnitude, nor the near-global extent of the changes, implies that significant physics is missing from the models. Specifically, positive feedbacks that have an amplifying effect and a globalizing effect on abrupt changes are probably missing from the models. This is not an academic issue, because the future response of the climate system to anthropogenic forcing will certainly involve the same physics. To give an illustrative example of what such "missing physics" might be (without implying that this is the correct answer), let us consider the effect of wind on sea surface temperatures (SST). Abrupt climate change is accompanied by an abrupt drop in wind speed, as seen from particle sizes in ice cores. When winds over the low latitude ocean suddenly die, the upper tens of meters of the sea surface stratifies and thus warms, especially in ocean upwelling regions, feeding back to produce further climatic warming (Agustsdottir et al., *Geophys. Res. Lett.* **26**, 1333-1336 (1999)). However, global climate models are unable to resolve the upper tens of meters of the ocean because of computer power limitations.

The research agenda should focus on identifying and understanding at a fundamental level the physical processes that may constitute the "missing physics". Ultimately, better parameterizations of these processes should enable the models to correctly simulate the paleoclimatic record of abrupt change, and by logical extension, more accurately forecast the warming expected in the next century due to human perturbation of the climate system.

An excellent National Academy of Sciences study of the abrupt change issue has recently been published and is a must for citation in the Climate Research Plan: "Abrupt Climate Change: Inevitable Surprises", National Research Council, National Academy Press, Washington, D.C., ISBN 0-309-07434-7, (2002). This study contains many excellent recommendations for future research that the Climate Research Plan would do well to include.

SHEA, EAST-WEST CENTER, HAWAII

1. The draft Strategic Plan for the U.S. Climate Change Science Program represents an important step forward in the continuing evolution of national scientific efforts to understand and respond to the consequences of climate variability and change for the Nation and the world. I would like to start by complimenting and thanking the Administration officials, agency staff and members of the scientific community responsible for the preparation of the draft Strategic Plan and the convening of the

General Comments

December 2002 Workshop for Scientists and Stakeholders held in Washington, DC. Your leadership, vision and hard work are greatly appreciated and have produced a solid draft Plan. The following comments are offered in the spirit of strengthening the Plan overall, addressing some specific issues in selected chapters, and ensuring that we build on past experience as we move toward the future as a joint community of scientists and decision makers – both stakeholders in the future of the Program and the Nation. I look forward to continuing to contribute to the evolution of the USCCSP and will be happy to help in any way that I can.

2. One way of providing some of the program-wide "integration" called for during the December 2002 Workshop would be to **consider a program framework focused on the establishment and support of a climate information system** for the U.S. and the world. While not explicitly called-out in the Plan, this framework was alluded to throughout the Workshop and offers a clear statement of how and why funding the USCCSP benefits government agencies, private-sector decision-makers and the public at large. This framework points to the end-product for the Nation – a *climate information system* that provides scientists and decision makers with information on the nature and consequences of climate variability and change. Such a programmatic framework can help guide the integration of observations, research, modeling, assessment, education and outreach activities as inter-dependent functional elements of a USCCSP. This conceptual approach would also provide an integrated context for discussing the interdependencies and relationships among the individual thematic elements of the Strategic Plan (water, land use/cover change/etc.).

An overarching focus on an information system would help clarify what was meant by references to supporting a "transition to applications" during the December Workshop – an extremely important programmatic objective that is not yet well-articulated in the Plan. This could, in turn, help encourage/facilitate partnerships with mission agencies at Federal, state and local agencies as well as public and private-sector interests in key sectors.

3. Incorporation of the concept of providing information to support decision-making as an explicit focus for the USCCSP has a number of implications for the development and implementation of the program and the revision of the Strategic Plan. Most notable among these (from my perspective) are:

- The need to address a **continuum of timescales** that incorporates climate variability and change as part of a holistic climate (and information) system. Most decision makers are interested in information on that full continuum (from extreme events through seasonal-to-interannual and decadal variations and long-term climate changes influenced by human activities;

- A related issue involves a commitment to a scientific program that helps **address today's problems** (e.g., extreme events associated with year-to-year variability like ENSO) **while planning for the future**. My own experience at the regional level suggests that this focus on helping public and private-sector decision-makers in the near-term helps keep them engaged in thinking about/addressing the longer-term consequences of climate variability and change;

- The importance of providing information to identify and evaluate consequences and response options – including **both adaptation and mitigation**;

- A recognition that most decision-makers will be **addressing climate considerations in the context of other, inter-related stresses and opportunities** facing the environment, economy/businesses and public welfare. In other words, there are very few (if any) decisions that will be made solely on the basis of climate information and a scientific program designed to provide the climate input for those decisions must recognize the broader context for those decisions. This requires a scientific program that **understands and explores decision-making processes** as well as physical-chemical-biological-geological climate processes;

- All of the above leads to recognition of the importance of engaging scientists and decision makers in a collaborative program of shared learning and joint problem-solving. This, in turn, requires not only focused research programs but a long-term commitment to a **sustained, interactive dialogue with decision makers** at local, state, regional, national and international levels. This involves more than just traditional "education and outreach" programs and represents a fundamental shift in the paradigm that has governed the relationship of science and society in the past. Addressing the consequences of climate variability and change for the Nation and the world offers an exciting opportunity to demonstrate just how this new science-society partnership might evolve in the future. The draft Strategic Plan recognizes the need for such a new partnership but I would like to see it more explicitly addressed in the programmatic approach outlined in the Plan.

4. In this context, I would strongly recommend that the Strategic Plan **recognize the significant progress that has already been made in establishing this sustained dialogue** between scientists and decision-makers. In particular, I believe the Plan should explicitly **acknowledge and support the stakeholder networks, education and outreach programs** that have been initiated in the context of both the IPCC and national and regional assessment programs that have previously been supported by the USGCRP. Acknowledging my own bias as the lead for one such regional assessment programs, I think it's important to acknowledge the fact that the "*credibility and legitimacy*" of the results of the USCCSP requires more than a traditional measure of scientific quality. Building and sustaining trust among scientist and decision makers is and will continue to be a critical factor in the development and application of useful and usable climate information products derived from investments in the USCCSP. The USGCRP agencies have already invested in the creation of these science-society partnerships and the USCCSP can take advantage of and build on those initial investments. This point was highlighted often during the December 2002 Workshop so I won't belabor it further here.
5.

6. In continuing to evolve the USCCSP's approach to the important partnership among scientists and decision-makers, recognize the **important role that trusted "knowledge brokers"** play in both establishing the credibility and legitimacy of USCCSP scientific results and encouraging the use of USCCSP information products to support decision-making. In this context, you might want to explicitly acknowledge and engage individuals and organizations that currently serve in the role of information brokers including (but not limited to): the National Weather Service, state climatologists and regional climate centers; extension agents (agricultural and marine/coastal); Federal and state resource management agencies; other Federal and state mission agencies engaged in climate risk management (e.g., FEMA and state civil defense/emergency management agencies); universities and private-sector research institutions; non-governmental organizations (NGOs); sectoral and/or industry associations; and community leaders.

7. Consider the identification of some **Program-wide "integrating theme(s)"** that can help clarify the inter-dependencies and interactions among the various program elements (water, land use/land cover, atmospheric chemistry, etc.). One such integrating theme might be a commitment to explore and address the climate-related **"vulnerability"** U.S. businesses, communities and ecosystems. Some of the advantages of using vulnerability as an integrating theme include:

    - Integration of considerations of exposure, sensitivity and resilience – factors that relate to the human systems, natural systems and the climate system – and the inclusion of a focus on "resilience" requires the consideration of response options as well as information on current and changing conditions;
    - Providing a focus for development, evaluation and application of integrated models and decision support tools;
    - A requirement for interaction among experts from a variety of disciplines as well as decision-makers in government and business that will: (1) deepen understanding of the nature and consequences of climate variability and change; (2) establish trust and credibility; and (3) strengthen lines of communication; and
    - Providing information to reduce vulnerability/enhance resilience is a powerful tool to engage decision-makers and focus USCCSP products.

8. I would also encourage consideration of **climate-related extreme events** as another possible integrating theme for at least some sections of the Plan (e.g., water and climate most notably). The broader context of "**climate risk management**" might be even more appropriate and would incorporate the considerations of vulnerability described briefly above.

9. Another possible integrating theme for the USCCSP is, of course, **the water cycle** itself. "Water is gold" as a participant in the Pacific Islands regional assessment activity once said and the consequences of climate variability and change for water resources has direct and cascading influences on the viability of communities,

businesses and ecosystems throughout the country and around the world. This was discussed at length during the December Workshop so I will not comment further at this point.

10. I would encourage the authors to consider **incorporating a single chapter that describes a/the functional elements of the USCCSP (observations, process research, modeling, assessment, data and information services)** and how the program strives to integrate these program elements in addressing each/all of the problem areas described in the Plan. This would avoid duplication throughout the text and provide the reader with an important sense of how the program seeks to integrate these previously independent activities.

11. I think it's important to recognize that **some of the near-term deliverables from the USCCSP in the next two-to-four years will, in fact, come from ongoing work being conducted in the context of the USGCRP**. This not only strengthens the rationale for continuing those important investments but also clarifies the interdependence between the existing programs of the USGCRP agencies and the targeted, new activities proposed as part of the President's Climate Change Science Initiative (CCSI). This suggestion was highlighted by a number of presentations on the last day of the December 2002 Workshop and I simply wanted to reinforce it in these written comments.

12. **More fully integrate considerations of the "human dimensions"** in all of the chapters of the Strategic Plan. Chapter 8 (land use/land cover) does a very good job of integrating the human dimensions of the problem and could serve as a model for the other chapters.

My apologies for not being able to provide more detailed, page-by-page comments on the draft document but I know that many of the editorial and specific suggestions I might offer have already been submitted by others. I hope that these more general comments have been useful and, as I said at the start, I look forward to continuing to work with you to develop the USCCSP in the future. As has been the case with the USGCRP before it, this program is vital to the future of the Nation and I share your commitment to its development and implementation. If you have any questions or would like to discuss any of these points further, please do not hesitate to contact me.

SLINGO, NCAS CENTRE FOR GLOBAL ATMOSPHERIC MODELLING

While the document provides a comprehensive assessment of the science needs in various aspects of the earth system, the cross-cutting infrastructure required to develop and deliver the best possible predictions along with assessments of model accuracy does not come through clearly.

Chapter 3 reviews the status of climate observations and identifies future needs. It would be worthwhile having a similar chapter on the status of climate modelling and the possible strategies that could be developed to address model systematic error, a huge barrier to prediction on all timescales. At the end of my talk, I identified some key infrastructures that we consider necessary to advance climate model development. These are not necessarily dealt with in Chapter 12.

Modelling hierarchy covering range of space/time scales – process studies, non-linear scale interactions, exploring parameter space, paleoclimates – but there must be traceability
Advanced computational methods for resolving key processes, exploiting high resolution EO data – nested grids, adaptive mesh refinement (AMR), super-parametrizations
Maintaining model diversity and developing model complexity – modular framework (e.g. EU PRISM, ESMF)
Linking with operational activities - NWP, seasonal to interannual prediction
Confronting models with observations – developing innovative diagnostics
Integrated assessment – allowing feedback from climate impacts

SMITH, L., RETIRED

While this draft Plan has some decided strengths in its discussion and treatment of particular aspects of Earth System science, it is fundamentally flawed as a strategy for constructing a policy-relevant research program. The risks of global climate change are far too important to humans and the supporting Earth System as a whole to treat them, as this draft Plan does, primarily as the subject of a curiosity-driven academic research exercise of moderate priority and interest. Human-forced global climate change is a problem of steadily growing importance that calls for responsible action now. There is so much momentum inherent in the several components of the Earth System that respond to greenhouse gas forcing, and so much momentum inherent in the socioeconomic systems that are responsible for steadily increasing greenhouse gas emissions, that there is no room for the luxury of another decade of scientific studies to finely tune our understanding of accumulating past, present and future global climate changes before response strategies are seriously considered on an equal and interconnected footing.

Surely, in almost every field of human endeavor where the risk of major adverse consequences is palpable, responsible societies that are also knowledgeable initiate measures to reduce the risk, even though the understanding of how far to push risk-reduction measures may be incomplete. This draft Plan assumes that our understanding of global climate change today is so flawed and incomplete that no action is prudent or feasible before several or many more years of additional research pass by. A reasonable person must reject this view, and conclude that this draft Plan should be recast so as to support a policy stance of adaptive decision making and management, recognizing that meaningful steps to reduce greenhouse gas emissions are needed now, and that increased understanding over the coming years should impel frequent reevaluation of policies to combat this major problem of the 21st century. Our knowledge of global climate change will never be complete. Uncertainties will always exist.

The number of senior level Administration spokespersons at the workshop confirms the inherent policy nature of this discussion. They each encouraged the workshop participants to evaluate the science contained in this draft Plan, but it is the policy context that is most in need of reexamination. While I can certainly find some flawed biases in the description of scientific knowledge in the draft Plan, for example the statement on page 6, lines 21-22, "measurements taken from satellite observations of the lower- to mid-troposphere∑show no significant warming trends in the last two decades of the 20th century." This statement is contrary to the published work of Frank Wentz, Kevin Trenberth, Ben Santer and others. Such biasing of the reported science needs to be swept from the draft Plan in working towards the next revision. This is the easy part. The more challenging job is for Administration senior executives to recognize that the entire policy framework of this draft Plan is fatally flawed. It is time to recognize that global climate change is a real problem, one that will not go away on its own, and that a responsible American government must take responsibility for exercising leadership in changing the public discourse and its policy stance towards finding and implementing real solutions. (Lowell Smith, unaffiliated)

In this light, my principal concerns about the draft Plan are:

1. CRITERIA FOR POLICY-RELEVANT ACTION ARE NEEDED The plan discusses uncertainty, but fails to lay out any criteria for deciding when mitigation or adaptation actions would or might be required. It does not articulate prospective policy actions that could be considered, nor what level of increased scientific confidence would be necessary to trigger such action. Thus, there is no objective basis for deciding which uncertainties, at what level, are impediments to decision making and which uncertainties might be less relevant to the decision making process. Informed policy making requires this degree of specificity. This fatal flaw, the draft Plan's silence on these matters, makes it nothing more than a call for more research of indeterminate scope, intensity and duration. This is plainly a disservice to the citizens of the United States and the peoples of the world. REQUIRED ACTION: The revised Plan must clearly articulate explicitly the degree of confidence required for information inputs to policy making on climate change at the senior executive level within the Administration. (Lowell Smith, unaffiliated)

2. ROLE OF ASSESSMENTS NEEDS CLARIFICATION The assessment process has two invaluable purposes in a policy-relevant research program: 1) to assist in informing on an ongoing basis policy-making activities ranging from localities to the national level; and 2) to determine analytically priorities for addressing the myriad of scientific uncertainties that may or may not be germane to critical policy issues. The draft Plan skirts discussing the critical nature of assessments in policy-relevant research programs and outlining how assessments will be used in the Climate Change Science Program. This could lead to a lack of desirable support to resource managers, inappropriate design or placement of long-lived infrastructure investments, a dearth of balanced information to educate the public, as well as inattention elected officials to a growing problem of high consequence. "The interface between science and public policy and the important role that assessments play in this interface are important issues and challenges."---Ari Patrinos, What is it in the words, "∑shall prepare and submit to the President and the Congress an assessment which ∑integrates, evaluates, interprets∑analyzes∑not less frequently than every four years" that those responsible for this draft Plan do not understand? REQUIRED ACTION: The revised Plan must

recognize the essential roles that assessments play and clarify how they will be incorporated into the research program. (Lowell Smith, unaffiliated)

3. NEED TO SUSTAIN AND BUILD ON THE RECENT NATIONAL ASSESSMENT FOCUS ON REGIONAL VULNERABILITIES AND CONSEQUENCES The recent National Assessment served the very useful role of focusing at a regional level on the vulnerabilities and consequences of climate change. This critical work is most efficiently done by entraining and funding science researchers at universities located throughout the affected 50 states. The draft Plan is silent on the need to continue and expand this recent research effort that was focused on local and regional vulnerabilities and consequences. REQUIRED ACTION: The revised Plan should commit to nurturing and expanding the research and assessment activities initiated during the National Assessment that foster a fuller understanding of climate change at the local and regional levels. (Lowell Smith, unaffiliated)

4. UNREALITY OF COMMITTED RESOURCES It is not credible to expect to achieve the objectives of the draft Plan plus the many worthy suggested augmentations reported by the breakout groups at the workshop within the current budget ceiling. As there is little apparent fat to cut, simply rearranging priorities will be ineffective in reallocating the budget so as to achieve all the worthy and necessary research objectives in a timely manner. The overwhelming impediments to shifting a fixed amount of funds among agencies make it even more improbable that these research objectives can be met expeditiously at the current funding level. The lack of cross-cutting assessments makes it even more improbable that all critical policy-relevant information needs will be recognized and met by adequately funded research efforts. Plans for research programs without the assignment of adequate resources are misleading at best, and irresponsibly disingenuous at worst. REQUIRED ACTION: The revised Plan should honestly acknowledge the requirement for adequate resources based on a realistic appraisal of the true cost of achieving its objectives, and the Administration must commit to requesting of Congress a research and assessment budget adequate to the importance and complexity of the research and assessment tasks. (Lowell Smith, unaffiliated)

5. UNREALITY OF CCRI REMOVING MAJOR SCIENTIFIC UNCERTAINTIES IN A 3-5 YEAR TIME FRAME While the scientific uncertainty questions selected to focus the efforts of the CCRI are interesting and important, scientific progress in answering these and related questions can be expected to be incremental over the next several decades (not years), with low expectations for a threshold breakthrough over the next few years. In this circumstance, raising expectations for a major increase in understanding in these areas in a relatively short period of time is misguided. On the other hand, placing immediate priority for augmented funding for developing and implementing a suite of global climate observing systems and advancing the art and practice of assessments would be both realistic and beneficial. REQUIRED ACTION: The revised Plan should assign these long-term research efforts to their proper place within the USGCRP, and should put a strong focus on creating and implementing robust global climate observing systems and producing useful assessments. (Lowell Smith, unaffiliated)

6. PROVIDING SUFFICIENT TECHNICAL AND INSTITUTIONAL SUPPORT FOR REGIONAL CONSEQUENCES RESEARCH AND ASSESSMENTS The scientific study and assessment of regional climate change vulnerabilities and consequences would be greatly facilitated by providing to the several relevant communities of ecological, hydrological, agricultural and socioeconomic researchers physically consistent climate change scenarios with adequate spatial resolution and statistical representation of probable weather extremes. This would require dedicated resources of high capacity computer time, and dedicated scientific and technical groups to produce and interpret such scenarios. A useful model of the mobilization of effort required is that developed by the UK in its LINK programme at the Climatic Research Unit at the University of East Anglia. Link serves as an exceptionally useful and functional interface between the modelers at the Hadley Centre and the impacts research community throughout Europe and the IPCC. The understanding of climate change consequences in the US could be greatly accelerated by building a comparable partnership in this country. REQUIRED ACTION: The revised Plan should give special attention to how to accelerate regionally focused climate research and assessments by providing sufficient technical and institutional support. (Lowell Smith, unaffiliated)

7. TECHNOLOGICAL RESEARCH, DEVELOPMENT AND DEMONSTRATIONS ARE AN INTEGRAL PART OF A INTEGRATED, COMPREHENSIVE PLAN Of the 24 breakout sessions at the workshop, only one was devoted to technological solutions, perhaps signaling the disinterest or disbelief of the Administration's executive leadership in the need for mitigative actions at any time in the foreseeable future. The literature is full of analyses on how responding now to global climate change can provide a net benefit to the economy. Moreover, new energy supply industries based at home would cut back on the massive export of funds now being used to pay for imported energy from insecure regions of the world. Reductions in environmental disbenefits would offset many of the residual costs. There has been a debate over energy efficiency for many years. The unambiguous conclusion is that employing more efficient technologies saves money and reduces environmental impacts. REQUIRED ACTION: The draft Plan needs to be comprehensive in furthering jointly an understanding of the scope of the scientific and societal problem and its technological and institutional solutions. Moreover, in this regard it needs to recognize the opportunities to increase our national security and environmental security, while at the same time strengthening the nation's economy and protecting its environment by accelerating the Nation's technological capabilities and the economic attractiveness of becoming substantially more energy efficient and transitioning to a renewable energy resource infrastructure.

SOIL SCIENCE SOCIETY, BRONDIZIO

The “Strategic plan for the Climate Change Science Program” uses the 2001 Climate Change Research Initiative (CCRI) as a baseline to argue for a “distinct focus” to the US Global Change program. However, the very broad nature of the document makes it difficulty to comment on the specific direction for research on global environmental change in general and climate-related change in particular. While recognizing a 13-year history (and US$ 20 billion budget) of US research on global change, the plan does not take full advantage of enormous developments made during the past decade on areas such as: biosphere-atmosphere interactions, land use and cover change, human dimensions, etc. In this sense, while aiming at a "distinct focus," the document re-states several goals and research questions already in place, for instance, as stated in references such as NRC 1999a, and NRC 2001a cited in the plan. [E.Brondizio, Indiana University]

- The main character and message of the document is centered on the issue of "uncertainties." While recognition of "uncertainties" should open new opportunities for increasing funding to global change research and continue support for existing and successful research programs (e.g., at NASA, NOAA, NSF,.), the document does not offer specific guidelines/policies on the availability and direction of funds for research. [E.Brondizio, Indiana University]

-While aiming at a 'distinct focus' I found the sectorial organization of the plan (that is, atmosphere, land use and cover, water, etc..) offers little innovation towards the 'frontiers' of knowledge on global environmental change, that is, on "integrated science" and assessments. One of the lessons of global change research during the past decade is the need to move beyond compartmentalized research (sectors). <u>"Uncertainty," after all, in global change research lies exactly on the lack of integrated understanding of processes connecting human-terrestrial ecosystems-water-atmosphere processes</u> (feedbacks, thresholds, mechanisms underlying change). I elaborate on some needs for 'integrated science' for chapter 8 (LUCC) below.

SOIL SCIENCE, GLASENER

The plan could be called the "Carbon" plan. The role of nitrogen is mostly missed. N2O is a major GHG and is not really covered, Chapter 8 focuses on the carbon cycle (CO2 and CH4) and N2O is not considered yet nitrogen fertilization is one of the driving forces and it is a major GHG. It should be included.

The authors of chapter 9 tend to be heavy to management types, really could be said about most of the chapters, the input for hard scientists is limited and this is reflected in the very general nature of the document.

Much of the driving force of the document is modeling and modeling is needed but the hard science to support model development seems to be missing.

No one seems to want to address the role of the large increase in the world's population 3 X from about 1940. In many developing countries it is at a much higher rate. This is a very political sensitive issue but still needs to be addressed, or should be.

There is a complete lack of references, early drafts of chapter 9 had over 50 references, and this becomes a non-scientific document as presented. And is why I say non-scientists wrote it. Statements are made without any back up through out the whole document and taken as scientific truths.

Land use change is used in much of the document, what we are really talking about is "land use and land use change". Also some time it is called land use and land cover change (page 93 line 17 for example). Land use change is more that land cover change. This needs to be addressed.

Over all focus is on regional and national scales and that is the way GHG will be reported for international accounting but process models need to be developed at the farm or even field scale and then scaled up for regional and national scales. This is top down not bottom up!

SOLOMON , NOAA AERONOMY LAB

First I want to express my sincere appreciation to all who obviously worked very hard on the workshop and on the document. I thought it was truly outstanding. My comments below are offered with the goal of assisting in strengthening this already-very-fine work.

Chapter 1

The discussion of the goals of the program would benefit from some better distinctions between short-term and long-term foci. In particularly, evaluation of the limitations of current understanding and modelling (esp. regional models) should be a short-term focus. In the short-term, the program should also focus on getting a better understanding of the 'baseline' climate and beginning to assess its vulnerability to change. A detailed understanding of future change, the predictability of the climate system, and the regional structure of climate change, will have to be a long-term focus. The program should commit to both long-term and short-term foci, not just short-term foci; this should help the policymakers to understand the problems will not all be solved rapidly.

SPRIGG, UNIVERSITY OF ARIZONA

Critical components of a program to understand climate are included in the Strategic Plan. However, critical information needed to organize to address these components is lacking, such as an inventory of ongoing programs and resources. Then, how much of the ongoing resource can be relied upon to continue? On which of the components will new resources be given? Thus, we have a shopping list of things to do, but we have no idea which ones will be given priority. Deciding where to put one's effort depends heavily on where the first steps are taken. Telegraphing where you WILL go is more important than telegraphing where you HOPE to go.

There are several places where an author's bias creeps in, such as only thinking of "black carbon" or soot when giving an example of aerosol. What about particulates such as mineral dust? the effect on climate of dust storms? the feedbacks among land use, desertification due to natural and human influences, climate, dust storms, and actions to control dust sources? The entire document needs to be reviewed to catch similar biases.

The contributions of other countries should be expanded. Someone is bound to point out that this is a National plan and the emphasis should be on the U.S. strategy, but in climate research we depend on collaboration with other countries. There are too few references to these contributions in comparison to, say, references to the IPCC, which is the equivalent of an international think tank. IPCC does no research, conducts no measurement, and manages no data.

If you are going to mention Presidential support, Chapter 14 is embarrassing in that it only mentions George W Bush and George H. W. Bush. The chapter fails to mention the significant support of presidents Jimmy Carter, who signed into law the National Climate Program Act in 1978, and William Jefferson Clinton, who gave National and international attention to climate change and the responsibilities of governments world wide in addressing it, and under whose leadership considerable progress was made in understanding climate.

This leads to my last comment. This cannot be a science strategy without mentioning the volume of work that has gone before. Science progresses "on the shoulders of giants," and we have always been careful to acknowledge that. This plan ignores big studies and important influences on where we are today, including the body of knowledge attained through the U.S. assessment, "Preparing for a Changing Climate: The Potential Consequences of Climate Variability and Change," the impacts of the 1st and 2nd World Climate Conferences, and the roles of U.S. agencies, the U.S. Congress, the National Research Council, and the U.S. Domestic Council in the 1970's and 80's in forging U.S. strategies to address climate variability and change. This is important because it tells all readers that focussed study of climate processes, developing observation technology, reducing uncertainty in measurements and models, forging international cooperation to improve data access, and other key factors in understanding climate has been going on for some time ... time enough to test many hypotheses and observational techniques to warrant our estimates of how confident we are when assessing the state-of-the-science.

STOUFFER, GFDL/NOAA

Overall, I agree that the research effort of climate change in the United States needs to be more organized. In that regard, the CCSP effort is a good thing. However, I have many problems with the focus of the document as in now stands. These problems are outlined below.

1 . I tend to agree with Jim Hansen's criticism of this document in that it puts another layer of bureaucracy on the scientists without adding much to their ability to perform the science. There is much we do not understand in this field. The focus should be on reducing that understanding, not on providing information to policymakers. In my opinion, there are enough mechanisms already in place to accomplish that goal (the IPCC and US Assessment progresses). This plan barely acknowledges those efforts and does not indicate what is wrong with them. If the pathways for information flow are broken, that is what needs fixed. The solution of "scenarios on demand" is a catchy phrase, but it is scientifically very doubtful that they would be of any practical value.

2. Throughout the document, there is a lack of realization of past efforts. There have been 3 full IPCC reports and a national assessment in the past decade or so. Results from these and other efforts need to be cited as part of the background in many discussions. In general, the document does a very poor job of providing this background as it stands.

3. There is confusion in use of term "climate sensitivity". This term is well defined in climate community: global surface air temperature change for doubling of CO2, giving the ocean an infinite time to come into equilibrium with radiative forcing. In many places in the document, this term is used (miss-used) to describe time dependent changes. In the last IPCC, the term Transient Climate Response (TCR) is used to describe transient response which includes ocean heat uptake.

STRUBLE, BOISE, IDAHO

I have read through the draft strategic plan on climate change posted on your web site. I have several general comments about the document.

I support the overall thrust of this effort, to improve monitoring, measurement, and analysis of climate change. I also support the "Guiding Principles" in Chapter 1, Section 3 in that the focus should be on producing the best science and best data possible, not making policy.

I am concerned that almost all the authors are government officials. Only a tiny handful are affiliated with universities, according to the author credits. This being the case, it is hard to see how this program can keep politics out and focus exclusively on the science. I would like to see more involvement by leading university researchers as this program moves forward. This is the only way to give the program credibility, and avoid the appearance that the entire program is designed to give the aura of scientific credibility to the Bush administration's policy of doing little or nothing to curb climate change.

THOMPSON, NOAA

There seems to be little science devoted to the importance of oceans as a carbon sink and even less importance paid to fisheries or the marine ecosystem in general. The document is very "terrestrialcentric" with the marine environment (which occupies nearly 80% of the surface area) being inadequately considered, except in cases where ocean temperature changes may impact regional climatic conditions.

In "Chapter 10: Ecosystems," the feedback between ecosystems and drivers of environmental change is emphasized. The definition of "Feedback" provided, makes a clear distinction between environmental factors and the ecosystems they impact. With an ecosystem approach, how does one separate environmental factors from the ecosystems they impact? Isn't the environment part of the ecosystem, particularly when investigating climate change?

The illustrative research questions presented in Chapter 10, on investigating the linkages between ecosystems and global environmental changes, speak of the impact of environmental changes on ecosystems. It might be more appropriate to frame ecosystem-based research questions to focus on the INTERACTIONS between environmental factors and other components of the ecosystem, rather than presuming that impacts are unidirectional.

TITUS, EPA

Overview Comments on the Title of the Report

First Overview Comment: The most fundamental problem with *"Strategic Plan for the Climate Change Science Program*" is that it is not a strategic plan. The report leaves the reader wondering whether the government knows what a strategic plan is, or simply used that term because it sounded good.

Although the definition of a strategic plan varies, in general the elements include (1) analysis of the current situation and baseline projection; (2) specification of ultimate objectives and criteria for measuring success; (3) analysis of barriers to success; (4) specification and evaluation of alternative options ; (5) articulation of measureable goals; (6) action plan; and (7) evaluation.

The document discusses at a very broad level the current situation and some of the questions that ought to be answered. Unfortunately, it offers virtually no reflection on the objectives of the research program, the current success and failures at attaining those objectives, reasons for falling short of the objectives, options for improving results, or criteria for choosing between alternative options; nor does it offer a recommendation on which options to follow and which to reject. For all practical purposes, this "strategic plan" skips the strategic planning step and simply recommends a continuation of the status quo, without any analysis of what we should be doing, whether we are doing it the right way, or what might lead us to change course.

A more accurate title would be "Some Research Questions for the Climate Change Science Program" CCSPO must either (1) change the title of the document to reflect the fact that it is not a strategic plan; (2) undertake the much more laborious effort necessary to actually produce a strategic plan, or (3) put out a report with an inaccurate title.

Second Overview Comment: A small problem with the title is that several of the chapters focus on *global* change rather than *climate* change.

- One option would be to change the title to "global change", revise Chapter 1 to explain what global change is and indicate why some chapters have the narrower focus on climate change, and edit all the chapters to indicate whether they are focussed on climate change or global change. A drawback is that the organization itself is the *Climate* Change Science Program Office.
- Another option would be to keep the title and revise all chapters so that they focus on climate change, deleting material that is only relevant to other types of global change.
- The need to resolve this discrepancy depends on whether this document is meant to be a strategic plan or simply a discussion of various research issues. If the latter, it may be sufficient to simply provide explanations in the chapters that we are describing autonomous research programs, within the structures through which the key research officials see their programs. On the other hand, if CCSPO wants a strategic plan—or at least the illusion of one—the chapters should only focus on those issues that directly contribute to achieving the stated objectives (with the linkage stated).

[duplicate comment deleted in space below]

General Comments

[duplicate comment deleted in space below]

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[duplicate comment deleted in space below]

[p 139, p 11]

TOWNSHEND, UNIVERSITY OF MARYLAND

I found many aspects of the organization of the plan confusing. For example why are observations monitoring and data management dealt with primarily under the CCRI? They are just as relevant to the USGCRP but appear there only as part of the Grand Challenges chapter. Similarly it is not clear how and why the research areas discussed in chapter 2 were chosen rather than any of the many other potential candidates. They also map awkwardly onto the materials found in Part 11. All in all the document appears to be two separate documents bolted together with some cross-referencing. A much closer integration of the materials would be very beneficial.

U.S. CLIVAR SCIENTIFIC STEERING COMMITTEE

The Climate Variability and Predictability (CLIVAR) program is an internationally coordinated World Climate Research Program (WCRP) project aimed at improving scientists' understanding of and skill in predicting physical climate variability, from time scales of seasons and longer. U.S. CLIVAR is the scientific program designed and implemented by U.S. scientists and agencies to carry out the parts of the international CLIVAR plan that are most important to the United States and to which we can contribute the most. A Scientific Steering Committee (SSC) guides the U.S. CLIVAR program. This committee has established eight sub-committees to help implement and coordinate activities. Some of these sub-committees may submit their own comments. The comments here reflect a consensus from the SSC.

INCONSISTENCY: THROUGHOUT THE DOCUMENT THE REFERENCE TO MODES OF CLIMATE VARIABILITY THAT ARE ASSOCIATED WITH CHANGES IN THE SUBPOLAR-SUBTROPICAL ATMOSPHERIC PRESSURE GRADIENTS SHOULD BE DEALT WITH CONSISTENTLY. A POLL WITH THE COMMUNITY HAS RESULTED IN THE NOTION THAT FOR THE NORTHERN HEMISPHERE WE NOW USE: NAO/NAM (NORTH ATLANTIC OSCILLATION / NORTHERN ANNULAR MODE) WHILE IN THE SOUTHERN HEMISPHERE THE RELATED MODE IS NOW REFERRED TO AS: SAM (SOUTHERN ANNULAR MODE). THE USE OF ARCTIC OSCILLATION (AO) AND ANTARCTIC OSCILLATION (AAO) SHOULD BE AVOIDED IN THE DOCUMENT. IN THE FOLLOWING WE

WILL LIST ALL THE PLACES IN THE DOCUMENT THAT NEED TO BE MODIFIED:

- -P 50, line 39 … (NAO/NAM) North Atlantic Oscillation / Northern Annular Mode …
- -P 72, line 38 … replace NAO by (NAO/NAM) North Atlantic Oscillation /Northern Annular Mode
- -P 73, line 1 … add Southern Annular Mode (SAM)
- -P 73, line 27 replace AO by NAO/NAM
- -P 74, line 8 replace AO by NAO/NAM
- -P6, line 5 replace AO/NAO by NAO/NAM
- -P 167, line 10 remove
- -P 169, line 18 add NAO Northern Atlantic Oscillation
- -P 169, line 18 add NAM Northern Annular Mode
- -P 170, line 8 add SAM Southern Annular Mode

USGCRP GLOBAL WATER CYCLE SCIENCE STEERING GROUP

While our understanding of the water cycle has steadily progressed over the past decades, it is the opinion of the SSG members that integrated, well-coordinated field campaigns focusing on "closing" the water budget at the basin scale is essential to achieve further significant progress in our theories, models, and applications. Such intensive field campaigns need to be supplemented by long-term monitoring of various atmospheric and hydrologic variables in these basins. Our inability to close the water budget at this fundamental scale emphasizes the gap in our understanding of the various components of the water cycle, which cannot be fully understood unless all its components are simultaneously observed at the single storm scale. On the land, this requires measuring precipitation, evapotranspiration, runoff, soil storage and percolation to the water table. In the atmosphere, measuring water in its different phases as well as aerosols that serve as condensation nuclei is also essential. Measuring convective cloud properties simultaneously with the environment that forces them remains a very high priority. Such observations would provide needed datasets to improve our theories and modeling capability, which directly affect the quality of our applications. Both a rapid (2-3 years) and a long-term payoff would be anticipated from such activity.

There are other issues related to the water cycle that it is essential to keep in mind. First, the global water cycle is largely driven and considerably affected by the oceans. While various programs/agencies may chose to focus on various aspects of the water cycle and decide subjectively to separate the continental from the oceanic components of the cycle, it is very important to realize that the water cycle is highly integrated and interconnected, and it is impossible to understand its continental component without taking into account its oceanic counterpart. This cycle is also strongly interconnected, through many direct and indirect interactive and feedback mechanisms, to the various biogeochemical cycles. In particular, carbon and its impact on climate cannot be disconnected from the water cycle.

Finally, current models used to study the global water cycle need to be considerably improved at many levels. This includes improving the description of sources and sinks, as well as the parameterization of subgrid-scale processes of various elements. Among

others, clouds and precipitation remain a central aspect of the water cycle that is still not properly simulated.
Using the above comments as a general guideline to review Chapter 7, it appears that most of the issues found to be important by the SSG are noted at one place or another in the chapter. However, what is found to be missing in it is the proper emphasis of the most significant points. While specific comments on the wording of the text is provided in the comments of individual members below, we recommend that the introduction and conclusion of the chapter be used to emphasize the general comments made above.
Specifically, we recommend that the introduction emphasizes the role of the oceans on the water cycle with a statement that this chapter intentionally focuses on the continental aspect of the cycle, if this has indeed been deliberately intended. The introduction should also emphasize the connections to the other cycle, especially emphasizing the impossibility to separate the carbon cycle and its impact on the climate system from the water cycle because of the multiple interactions and feedback existing between these cycles. The conclusion should emphasize the need for integrated observations and the continuing development and improvement of models.

MARTIN VISBECK, COLUMBIA UNIVERSITY

Throughout the document the reference to modes of climate variability that are associated with changes in the subpolar-subtropical atmospheric pressure gradients should be dealt with consistently. A poll with the community has resulted in the notion that for the northern hemisphere we now use: NAO/NAM (North Atlantic Oscillation / Northern Annular Mode) while in the southern hemisphere the related mode is now referred to as: SAM (Southern Annular Mode). The use of Arctic Oscillation (AO) and Antarctic Oscillation (AAO) should be avoided in the document. In the following we will list all the places in the document that need to be modified:

WARRILOW, WILKINS – UK DEPT OF ENVIRONMENT, FOOD AND RURAL AFFAIRS

We congratulate the authors for putting together a good first draft of the programme for what is a very important but complex and broad issue. We also appreciated the opportunity to participate in the workshop and enjoyed the stimulating discussions. We were impressed by the intense interest shown and hope that our comments will help provide a useful perspective. David Warrilow is responsible for the UK's policy relevant climate change research programme, which can be viewed at www.defra.gov.uk. We will soon publish our own new science strategy and will make it available when it is completed. We would be pleased to encourage opportunities for collaborative work on areas of mutual interest.

We now turn to a few general comments on the structure and content of the plan.

a) Firstly we would like to address the overall balance of the programme. A clear idea of the likely balance of programme spend in different in areas would be helpful. This could help ensure that new and useful areas such as the work on handling risk are adequately resourced. We strongly support the aim expressed on p15 identifying specific deliverables from the programme that could provide a robust evidence basis to inform future policy and scientific decisions, and suggest these be made as specific as possible. While many elements are included which we would expect to see, we feel that there is an over emphasis on the basic climate science and insufficient on the challenges associated with the assessment of impacts and adaptation as well as the economic and social aspects of climate change.

b) There appears moreover to be an over-emphasis on the uncertainties associated with causes of climate change, which in our view have been well researched. We would not deny that there is value in revisiting such work, particularly as better estimates of aerosol forcing comes available, but we would not expect it to yield significantly different conclusions about the growing component of climate change due to greenhouse gas forcing.

c) We would agree that priority should be given to improving our understanding of components of the climate system that have proved remarkably resistant to attempts to reduce uncertainty – particularly the assessment of cloud – radiation feedback and the overall sensitivity of the climate system.

d) We would point out that from a policy point of view a major scientific challenge is to assess the overall impacts of climate change at various levels of stabilisation as a pre-requisite for tackling the political question associated of how to define "dangerous levels" of greenhouse gases in the atmosphere. The IPCC 3rd Assessment Report shows clearly that there is considerable uncertainty associated with determining the amount of climate change and impacts associated with different levels of greenhouse gas forcing. This presents a formidable array of complex and integrated scientific challenges, which we suggest should be given high priority. We would also suggest that such questions need to be addressed as part of a co-ordinated international scientific effort, which we will be keen to support and which would benefit greatly from full participation of US scientists.

e) An allied question to the above would be consideration of the emission scenarios which might lead to different levels of stabilisation, how they might be achieved both technically and economically. Some of the socio-political aspects might also be considered as an aspect of human dimensions.

WEBB, NOAA

Although attempts have been made to make the CCSP Strategic Plan as comprehensive as possible, I find an omission of a comprehensive discussion of the research needed to educate and inform the public on what is happening and why in terms of current and evolving climate to a decade from now, and to develop a capacity to produce medium range climate projections. The convergence of global monitoring, climate modeling, and a growing demand for information on regional climate variation and change provides the research community with an opportunity to develop a capability to forecast our way through climate change, whether it is due to natural or to anthropogenic forcing. The research community needs to be mobilized to provide the public with routine, credible diagnosis of current and evolving climate conditions, climate extremes, and their association with regional climate change, and to provide credible attribution for the processes driving climate variation and change and its likely trajectory over the next decade to meet the needs of the public and decision-makers.

Providing credible attribution for climate variability and change at decadal time scales will fill a gap in the CCSP Strategic Plan framework for climate monitoring, seasonal forecasting, and climate change projection. Attribution, by offering an understanding of the origin of climate impacts, will lead to improved predictions, and yield a better appraisal of the uncertainty in climate change projections. The regular and systematic attribution of current and evolving climate conditions and associated assessments of climate forecasts and outlooks will also improve credibility of the climate information products by helping external users of climate information understand the limits and strengths of these products. The activity to determine the origins of regional climate change and improve climate change projections at the regional scale involves an immediate and ongoing transfer of research to operations

The second gap in the CCSP Strategic Plan framework exists in between activities to improve seasonal climate forecasts and climate change projection efforts to the year 2100 under various greenhouse gas scenarios. Predictions of the path of the transient climate state, including abrupt changes, on time increments of years to a decade are as valuable, if not more, for decision makers as climate change projects for 2100. To provide these credible medium range climate change projects research will be required to identify the existence and causality for regional climate trends and multiyear to decadal climate variability, to quantifying the uncertainty of regional climate change projections based on an improved understanding of multiyear to decadal climate, and to develop the capacity to produce annually updated regional outlooks of climate patterns up to 10 years into the future.

This omission in the CCSP Strategic Plan crosscuts a number of chapters because the proposed capability provide attribution for current and evolving climate conditions and medium range climate change projections (a few years to a decades) is critical to improve assessments of the uncertainty in climate change projections and associated impacts. I would insert sections in Chapter 4 (Decision Support Resources) describing the use and need for these capabilities and in Chapter 6 (Climate Variability and Change) describing the scientific research needed to provide these capabilities.

WELLER, ET AL., UNIVERSITY OF ALASKA FAIRBANKS

The Strategic Plan for the Climate Change Science Program is very comprehensive but we are concerned that the polar regions, which play such a crucial role in global climate change (early indicators of change, polar climate amplification, feedback processes on the global climate, roles as climate change triggers including effects on the great ocean circulation belt, and global effects such as sea level changes due to melting ice) have received little attention in the strategic plan. We have therefore suggested some additions to the document, as listed below. The Europeans have taken the lead in at least three key climate research areas related to the polar regions: in better GCM modeling of ice processes (the UK Hadley and Max Planck ECHAM GCMs are leaders) and in the development and use of regional models, in ice core research (the US may be as much as 10 years behind Europe), and in high-resolution satellite observations (e. g. SAR). We believe that the polar regions should be addressed more substantively in the strategic plan.

A glaring omission in the "Strategic Plan" is any mention of the comprehensive US National Assessment (NAST, 2000), which has addressed impacts due to climate change in 15 regions of the United States and on sectors including agriculture, water, human health, coastal areas and marine resources, and forests, among others. This omission should be rectified by referring to the NAST Report.

Reference

NAST, 2000. Climate Change Impacts on the United States: the Potential Consequences of Climate Variability and Change. National Assessment Synthesis Team, US Global Change Research Program, Washington, DC. Cambridge University Press.

Reviewers: UA Group

WESTERN GOVERNORS' ASSOCIATION

On behalf of the Western Governors' Association, I would like to submit the following general comments regarding the draft *Strategic Plan for the Climate Change Science Program*.

The Western Governors' Association is an independent, nonprofit organization located in Denver, Colorado and Washington, D.C. WGA represents the governors of 18 states and three U.S.-Flag Pacific islands. Through WGA, these governors identify and address key policy and governance issues in natural resources, the environment, human services, economic development, international relations and public management.

<u>Adaptation</u>

The Governors recognize that climate prediction is complex with many uncertainties. The governors therefore recommend that policies related to long-term climate not be based on particular predictions, but instead focus on policy alternatives that make sense for a wide range of plausible climatic conditions regardless of future climate.

To implement this recommendation, the governors would urge you to form partnerships with the states, particularly as you consider the research questions in the draft strategy such as Question 5 in Chapter 6, "Climate Variability and Change," which reads:

> *How can interactions between producers and users of climate variability and change information be optimally structured to ensure essential information needed for formulating adaptive management strategies is identified and provided to decisionmakers and policymakers?*

<u>Good Governance</u>

In many places, the draft strategic plan discusses the importance of linking scientific research to natural resource management. For example, in Chapter 3 it states:

> *A climate observing system must go beyond climate observations themselves to include the processing and support system that leads to reliable and useful products. To be most effective it must also provide critical data for decision support and policymakers in areas such as climate and weather forecasting,*

human health, energy, environmental monitoring, and natural resource management.

Chapter 4 states:
Research will provide a continually stronger foundation to help decionmakers evaluate the suite of alternative policy options and operational strategies.

Chapter 6 asks the question:
How can emerging scientific findings on climate variability and change be further developed and communicated to most effectively meet the needs of policymakers and public and private sector decisionmakers, in order to enhance human well-being, strengthen the economy, and reduce risks and vulnerability of climate-sensitive activities and resources?

The Western Governors strongly concur with the emphasis in the draft strategy on linking the research contemplated in the strategy to the needs of policy makers. The governors recognize the importance of good science to decision-making. Under the Enlibra Principles (WGA resolution 02-07, "Principles for Environmental Management in the West"), the governors include a principle which states: "Science for Facts, Process for Priorities."

An example of where the governors have attempted to implement this principle that is relevant to the draft strategy involves our effort to have a national policy on drought enacted by Congress. During the 107th Congress, bi-partisan legislation was introduced in the House and Senate which would have established a national drought policy. The legislation was based on recommendations by the National Drought Policy Commission in its May 2000 report. A key provision in the bill directed the National Drought Council (which would be established by the bill) to "coordinate and prioritize specific activities that will improve the National Drought Monitoring Network."

The National Drought Preparedness Act of 2002 defines National Drought Monitoring Network as:
"...a comprehensive network that collects and integrates information on the key indicators of drought, including stream flow, ground water levels, reservoir levels, soil moisture, snow pack, climate (including precipitation and temperature), and forecasts, in order to make usable, reliable, and timely assessments of drought, including the severity of drought."

The need for the Network was identified by the National Drought Policy Commission in its report, concluding that better coordination of governments and private entities in international drought monitoring, prediction, research, education, water conservation, and technology transfer is essential. The Commission recommended to Congress that they "authorize and fund a viable plan to maintain, modernize, expand, and coordinate a system of observation networks that meets the needs of the public at large."

General Comments

With other natural disasters such as hurricanes, tornadoes, and floods, it is readily apparent when the disaster has occurred, thus making it easy for the federal government to respond accordingly. The onset of droughts, on the other hand, are much more difficult to gauge. We therefore need to build a drought monitoring network using improved and real-time data sets that together provide a holistic view of drought, on which policy makers can reliably base decisions. With other disasters it is obvious when they occur. With droughts, we need a solid foundation of science to trigger mitigation and response programs accordingly and at the appropriate level.

The governors recommend that the draft strategy keep its focus on ensuring that the CCSP research facilitate good governance through improved decision-making. The governors further recommend that the research strategy include the development of the National Drought Monitoring Network, as contemplated in the National Drought Preparedness Act of 2002, through a collaborative partnership with the states.

Regional Focus

Chapter 4 contemplates "Decision Support Resources for Regional Resource Management." As you know, the climate in the West is highly variable from watershed to watershed, and can be dramatically different than the eastern United States. To ensure that the CCSP research is sensitive to the conditions and needs of the West, it should invest in science programs in Western states which are able to pursue scientific questions relevant to the West.

Partnership with States

Chapter 4 and Chapter 13 define "decisionmakers" as: *"those who are actively involved in policy at the national and regional level and those who are making operational decisions for natural resources based on climate information."* This definition does not specifically mention state governments. Likewise, the draft strategy in general neglects to mention the important role of states in managing many of the programs that are impacted by climate change. We would therefore urge you to amend the draft strategy to more accurately reflect the important role of the states. More importantly, we would urge you to form partnerships with the states as you go forward with the implementation of the research strategy. For one, the Western Governors= Association would offer to work collaboratively with you on issues pertinent to the Western region.

In conclusion, the Western Governors thank you for allowing us an opportunity to provide comments on the draft "Strategic Plan for the Climate Change Science Program." We believe the research strategy should 1) emphasize adaptation, 2) facilitate good governance through improved decision-making, and 3) include a regional focus by conducting research in Western states ensuring that the unique needs of the West are met. Additionally, the governors would like to partner with you in the implementation of the strategy.

General Comments

WIELICKI, NASA LANGLEY

Major Omission: there is no Energy Cycle in the document. Yet changes in energetics from radiative forcing to cloud feedback are fundamental to why climate is changing and how much it will change. The document is fundamentally flawed without a chapter on the Energy Cycle. comments 2 and 3 are a result of this missing chapter. While the water cycle is also very important: its changes are a RESPONSE to changes in the Energy Cycle. In this sense the energy cycle is more fundamental to understanding global climate change than even the water cycle. Yes, they are tied through water vapor and clouds, but the major factor in the water cycle is precipitating clouds. These clouds are a very small fraction of all clouds. The non-precipitating clouds are the majority, are the major uncertainty in climate sensitivity, and play almost NO role in the water cycle. Average column cloud liquid water for non-precipitating clouds is only $1/400^{th}$ of the column water vapor amount: a quarter of 1%. These clouds, while critical to the energy budget, are an insignificant part of the water cycle (less than 0.25%). But they are the critical uncertainty in the Energy Cycle. The attempt to force these into one water cycle picture leads to a lack of focus in the document. An Energy Cycle section must be added to the document.

Major Omission: no discussion of measuring surface Sensible heat, Latent heat, or Radiative fluxes, yet these are key to atmosphere/ocean interaction, are poorly known, and are one of the largest errors in coupling ocean/atmosphere systems. There are new techniques now being developed and improved to provide satellite based regional and global radiative fluxes, and tropical to subtropical sensible and latent heat fluxes. This is a rapidly emerging key capability and does not require new instruments as much as continued algorithm development and validation: something the CCRI could show as a new climate action without spending a lot.

Major Omission: no discussion of measuring top of atmosphere solar and thermal infrared radiative fluxes which are the boundary condition for driving the climate system. This is currently one of the most accurate climate data sets available, and climate modelers use it as a fundamental measure of success in whether they are handling clouds and atmosphere/ocean transport correctly. This is the most fundamental constraint on atmosphere + ocean equator to pole transport. Changes in the top of atmosphere radiation continue to quantify major shortcomings in climate model cloud feedback (e.g. Cess et al., GRL, Dec 2001, Lin et al, Journal of Climate, Jan. 2002, and Wielicki et al., Science, Feb 2002. This omission is especially strange given that NASA is transitioning this key climate measurement with its 23-year satellite record to date over to the NPOESS system. What CCRI could contribute is to assure that the 50% probability of a gap in the record between the end of the NASA Aqua mission (2008) and start of NPOESS mission (2011) could be relatively simply fixed by adding the last NASA instrument copy (currently in storage) to the joint NOAA/NASA satellite mission going up in 2006/7. This could be another relatively inexpensive CCRI initiative to repair the likely gap in a key climate record especially pertinent to cloud/radiation feedback in the climate system. This sort of problem in our climate observations is symptomatic of climate records falling between the cracks of NASA research focus and NOAA weather/operations focus. A true multi-agency climate effort with solid coordination would not have these records broken. A break in the record degrades both the 30 years of data up to 2008, as well as all the data starting in 2011: because they cannot be tied together as a single long climate record: requires overlapping intercalibration to achieve climate accuracy.

The current plan is not a strategic plan, but a general science plan. It does a good job of explaining WHY we want to measure or model climate, but never explains the strategy of HOW to measure and model climate, except in very vague terms. Need to add specifics or change the name to a Science Plan that proposes as Step 1 do develop a Strategic Implementation Plan.

With no significant new sources of funding: this plan will fail: it proposes in general terms an approach that cannot be met with current funding levels. See comments on Chapter 3 as an example. We have put our heads in the sand and claim we can produce a climate observing system with the current resources: I have led development of satellite climate data products for about 20 years: we are short by a factor of 2 to 3 what it would take to build the climate observing system envisioned in this document. I did this estimate 3 years ago as part of a NASA 25 year vision exercise. We should admit up front in the document, that we cannot afford it, but will do the best with what we have, accepting large risks that the climate data system will not be adequate for the task. Note also, that when the U.S. public sees enough climate change to demand an answer (what I call the Climate Epiphany happens) then money will the thrown at the problem for a Apollo or Manhattan like project. We can buy computer power in 1-2 years. We can retrain science talent in 2-5 years. But the decadal climate records needed to verify confidence in all the new models will take decades to collect: you cannot test against data of insufficient accuracy, stability, and full of gaps to make policy decisions with high confidence.

WIENER, INDIVIDUAL COMMENTATOR

1. A programmatic comment on the plan as a whole: There have been a number of efforts in a variety of prominent research programs to achieve centralized information management and comprehensive synthesis of reports from disparate sources. Two outputs have been notable. First, there have been substantially generalized "over-arching" syntheses which are not apparently fruitful beyond the academic and policy-descriptive exercise. Place-specific and problem-specific work fostered by such programs (e.g. IGBP, and others) has had substantial value and significant applicability. In a gross sense, however, making the detailed and empirical studies that are done into grist for a more generalized synthesis is "going the extra mile" to produce something which is so abstract that in order to use the work, one must promptly reverse course, "and march right back again" to sufficiently contextualize and specify the policy prescription. Generating large-scale comparisons of many cases with long checklists of all relevant causal variables is not useless, but it is less useful than applicable prescriptions. Therefore, I fear devotion of limited resources of scientific talent, experience, and funding to the less useful products.

General Comments

The second kind of mega-synthesis work which has appeared is the scientific equivalent of a somewhat democratic process in which cautious and conservative syntheses are produced through the work and disputation of large, cumbersome, and diverse committees, with a slow, expensive, and laborious process. The IPCC is the leading example. These documents are "behind the curve", but they are as close as we have come to fully-debated "science court" statements of what has been solidly established. The burden of proof is on those who would overturn such findings. The purpose served is not advancement of the front-lines of science, but settling the rear guard issues, and the IPCC has done this well. Re-doing this is not worth the diversion of talent and time. Private interests are always free to fund policy-seeking research; the tobacco and cancer situation demonstrates this. The public, however, should not be funding our best researchers and institutions in efforts to show that smoking is beneficial or that gravity is a political claim. Public science demands non-partisan professionalism. The slow and contested path of scientific argument produces credibility which can only be diminished by misuse.

Therefore, in my view, the CCSP should be directed away from grand synthesis at levels of abstraction which preclude direct application, and more explicitly directed at regional and problem or sector-specific examinations which are case, place, sector and policy relevant.

2. A second programmatic comment on the plan in part. In the late 1970s and early 1980s, there was a great deal of disappointment with efforts to model both "the world resources problem", from both unitary efforts (Club of Rome's Forrester model and successive versions), and linked syntheses of different models (e.g. Global 2000 report to the President, by a federal multi-agency committee). Most unfortunately, however, there were several claims of modeling energy needs and use, which were exposed as actually rather elaborate partial efforts subject to manipulation; these were discredited and served in the end to embarrass rather than inform. (See Commoner, B., 1979, The Politics of Energy, Knopf, regarding interest group distortion of federal modeling efforts, and Schwarz, M. and M. Thompson, 1990, Divided We Stand: Redefining Politics, Technology and Social Choice; U. of Pennsylvania Press, regarding IIASA energy modeling.)

Efforts to centralize and manage science and data may be a mixed blessing and curse. The complexities and uncertainties in very large modeling and interpretation of complex data sets are a great temptation for those certain of the correct outcome, for whatever reason. The scientific, and in fact American political, traditional answer has been transparency of data and manipulations. The climate science community is fractious, divided, raucous and argumentative about almost everything – and entirely admirable for the public discourse and explicit approach of their arguments and discourse. In my opinion, this should be celebrated and appreciated. I have been privileged to observe some of this, and like our political processes, the diversity and complexity are a source of strength and certainty.

Therefore, I urge that the management of data by the CCSP and federal agencies in general be carefully divided into two distinct realms. The important discussion of national security-related secrecy must go on, and the research into other topics should be handled in custodial and transparent fashion. Because no one is free of some hope for outcomes and policies, all must be able to work from data with a clear history and pedigree; replicability of observations is not available, but full explanation is very important. And, the policy and interest relevant outcomes should be plain and also fully exposed.

We will not persuade the vast majority of the world that our interests are not only self-interests if we are not willing to show our work fully and freely, subject only to the national security needs. There is no credibility in proceeding while disregarding the inevitable claim that all of this is driven by the oil companies; we will be accused of that no matter what comes from our work. We should not be naïve about this, and we should not risk losing the value of our work to such critiques. I am quite concerned that the appearance of political manipulation will discredit work that should help everyone. This means, to me, that the need for maximum openness and full explanation of all interpretations and judgement is even greater than normal.

WILBANKS, OAK RIDGE NATIONAL LABORATORY

This is a massive, sprawling program proposal, excellent in many ways, with a little bit for just about everybody. One central issue, however, is whether the proposed deliverables are reasonable given the resources available, when the strategic plan is not accompanied by a proposed budget. If, in fact, resources will not be sufficient to support all of the proposed activities, then what are the highest priorities? What are the most critical elements in the program, in terms of either critical-path importance for the research enterprise or urgency for decision-making? In particular, what will be the relative emphasis on the nearer-term CCRI vs. the longer-term GCRP?

Regarding critical issues and grand challenges, much more attention should be paid to the impacts of different greenhouse gas stabilization levels on people and ecosystems. This is the key issue for climate change policy, and it can be informed by research both in the near term and the longer term.

Clearly, relationships between CCSP and the Climate Change Technology Program need to be addressed and clarified. The current brief mention in Chapter 15 of a high-level coordinating body is not sufficient. The issues are fundamentally conceptual, methodological, and otherwise research-related as well as administrative/bureaucratic.

WILK, INDIANA UNIVERSITY

Overview Comments on Chapter 11: Human Contributions and Responses to Environmental Change

I was very surprised and disappointed that this section of the report made no reference to the NRC report on Environmentally Significant Consumption (Stern et. al.).

This report makes clear that there are many human activities that have a direct and important impact on greenhouse gas emissions, which are not included in the present draft of this chapter, or indeed anywhere in the Strategic Plan.

Indeed in every section this plan discusses human impacts on the climate at such a level of abstraction, which one would think that issues like 'land use' are totally unrelated to the actual reasons people want to use the land in the first place. That is to produce agricultural and extractive goods that will be processed for human consumption.

Using language like "human driving forces" or "living standards" obscures the fact that social scientists working on consumption know a great deal about why human levels of energy and material consumption are rising all over the planet. This is not some kind of physical "driving force," but is instead a social, political, economic and cultural issue of great immediate importance. Indeed, further research on this issue is absolutely central to understanding the growing human impact on the global environment.

We know that culture makes a difference in levels of consumption and emission of greenhouse gasses; it is not simply a matter of income or "living standards." At the same levels of income, people of different cultures have different amounts of impact on global climate. Automobile use, for instance, has distinctive characteristics in different countries. Yet, we do not yet know at what levels automobile use will be 'saturated' and will level off - all we can predict is that this level will be vary widely from place to place depending on a whole series of variables that have not yet been identified, much less quantified. Is this not a key question for modeling the future emissions of greenhouse gasses from automobiles? Present projections are based on extremely weak and untested assumptions, which generally come down to extending present trends indefinitely into the future.

It is almost incomprehensible to me that the social dynamics of consumption are almost completely absent from the research program outlined in the present plan. There are literally thousands of social scientists around the world working on consumer culture, and the directions it is taking on a global basis. Most of this work is uncoordinated, underfunded, and lacking in common standards and language; but it has great potential, and should be included in any research program. Both OECD and the European Science Foundation have started major initiatives in the last four years to study what they are calling 'sustainable consumption.' This is the study of how the future environmental impacts of human consumer of goods and materials can be reduced, without lowering 'living standards.' It seems strange to me that there is not even a glimmer of a parallel research effort in this country.

DEREK WINSTANLEY, ILLINOIS STATE WATER SURVEY

First Overview Comment: The science of climate and global change is a major component in policymaking in the US and other nations. It is also a major component in policy making by states and businesses. I am pleased to see that the Federal government is making progress in producing an appropriate strategic plan and that there is some emphasis in the plan on decision support for decision makers. The other major group involved in setting the research agenda is scientists. I find that there is ambiguity and inconsistency in the draft plan as to the extent to which these two groups - decision makers and scientists - are driving the research agenda for CCSP (CCRI and USGCRP), and a lack of clarity as to the intended audience(s).

In the Foreword, it states that the CCSP pursues "accelerated development of answers to the scientific aspects of key climate policy issues" while continuing to make scientific advances. On Page 9 it states that "This focus is defined by a set of uncertainties about the global climate change system that have been identified by policymakers ....", but I do not see anywhere in the report where the scientific needs of policymakers are identified explicitly. Indeed, it states on Page 11 that "The scientific analyses conducted by the CCSP are policy relevant but not policy driven." However, on Page 40 it states that "The CCRI **will initiate** [my emphasis] a process to identify policy decisions that should influence the focus of climate change research programs." On Page 40 it is stated that a major product **will be** [my emphasis] the “Selection of a set of potential policy questions ....” These statements indicate that policy questions and/or decisions have not yet been identified. Hence, it is apparent that the research needs and products identified in the draft plan have not been developed to support clearly articulated policy questions or decisions. On line 29 of Page 11, it states that the research strategy is to identify “Primary research questions that focus on broad **science** [my emphasis] issues....”, not policy issues. Although it is encouraging that Chapter 4 is dedicated to identifying "Decision Support Resources", this comes **after** the CCRI research and data management agendas have been set in Chapters 2 and 3. As a description of the USGCRP programs follows later in Part II, and these programs are not obviously driven by specified decision support needs, there seems to be further disconnect in providing explicit decision support. Indeed, the CCSP mechanisms for management of the USGCRP listed on Page 163 include "scientific guidance" but not “decision support”.

That the research agenda appears presently to be set largely by scientists is apparent in the characterization of the products that are to be produced. Typically, the products are to “improve understanding”", “reduce uncertainty", and “increase confidence". While such scientific products may be useful to decision makers, there is little expression of the level of confidence or uncertainty, or the level of understanding required by decision makers to make decisions. Scientists always welcome an opportunity to set research agendas to reduce uncertainty, but quite often research increases uncertainty. On the other hand, decision makers have been known to say that it is scientific uncertainty that inhibits them from making key decisions. The plan appropriately recognizes that large uncertainties always will exist in making long-term climate projections, even with a perfect climate-system model, because of, for example, the uncertainties in projecting population and economic growth. Perhaps it will be part of the decision support efforts, but I recommend that decision makers be pinned down in identifying explicitly what their scientific needs are for decision making, including target accuracies and acceptable levels of uncertainty. By the very nature of decision making, decision makers are used to making decisions in the face of uncertainty, so what is it that decision makers need to know about climate change that will allow them to make decisions in the face of uncertainty? Who are the decision makers?

General Comments

The Administration’s position is that it acknowledges that human activities are contributing to climate change and that strategies are needed that will result in non-Draconian response strategies with minimal adverse economic impact. In June, 2002, The President announced “the fundamental principles to guide a scientifically sound and effective global effort to reduce the buildup of greenhouse gases in the atmosphere” He said that his Administration’s climate change policy will be science-based, encourage research breakthroughs that lead to technological innovation, and take advantage of the power of markets (http://www.whitehouse.gov/news/releases/2001/07/20010713-2.html). On June 11, 2001, The President had stated that “Our approach must be consistent with the long-term goal of stabilizing greenhouse gas concentrations in the atmosphere.” “The policy challenge is to act in a serious and sensible way, given the limits of our knowledge. While scientific uncertainties remain, we can begin now to address the factors that contribute to climate change. There are only two ways to stabilize concentration of greenhouse gases. One is to avoid emitting them in the first place; the other is to try to capture them after they're created. And there are problems with both approaches. We're making great progress through technology, but have not yet developed cost-effective ways to capture carbon emissions at their source; although there is some promising work that is being done.” The President continued, “My administration will establish the U.S. Climate Change Research Initiative to study areas of uncertainty and identify priority areas where investments can make a difference” (http://www.whitehouse.gov/news/releases/2001/06/20010611-2.html).

President Bush recognizes the complexity of the science and that scientific uncertainties remain (for example, “Yet, the Academy's report tells us that we do not know how much effect natural fluctuations in climate may have had on warming. We do not know how much our climate could, or will change in the future. We do not know how fast change will occur, or even how some of our actions could impact it” (http://www.whitehouse.gov/news/releases/2001/06/20010611-2.html)). Nevertheless, The President states that “The policy challenge is to act in a serious and sensible way, given the limits of our knowledge. While scientific uncertainties remain, we can begin now to address the factors that contribute to climate change” (http://www.whitehouse.gov/news/releases/2001/06/20010611-2.html). President Bush acknowledges that “the National Academy of Sciences indicate that the increase [in greenhouse gas concentrations] is due in large part to human activity” (http://www.whitehouse.gov/news/releases/2001/06/20010611-2.html) and has reaffirmed “America's commitment to the United Nations Framework Convention and it's central goal, to stabilize atmospheric greenhouse gas concentrations at a level that will prevent dangerous human interference with the climate”. “My administration is committed to cutting our nation's greenhouse gas intensity -- how much we emit per unit of economic activity -- by 18 percent over the next 10 years. This will set America on a path to slow the growth of our greenhouse gas emissions and, as science justifies, to stop and then reverse the growth of emissions.” “If, however, by 2012, our progress is not sufficient and sound science justifies further action, the United States will respond with additional measures that may include broad-based market programs as well as additional incentives and voluntary measures designed to accelerate technology development and deployment” (http://www.whitehouse.gov/news/releases/2002/02/20020214-5.html).

Thus, it can be argued that the main emphasis of a Federal Executive Branch climate change science program should be to support the Presidential call to action to identify effective, least-cost strategies to stabilize the concentration of greenhouse gas concentrations in the atmosphere, as well as reducing scientific uncertainties. However, The President also created the National Climate Change Technology Initiative (NCCTI) to strengthen research at universities and national labs, to enhance partnerships in applied research, to develop improved technology for measuring and monitoring gross and net greenhouse gas emissions, and to fund demonstration projects for cutting-edge technologies, such as bioreactors and fuel cells" (http://www.whitehouse.gov/news/releases/2001/06/20010611-2.html). As both programs presumably address mitigation and adaptation, the scope of the CCSP and the NCCTI need to be clarified in order to prevent unnecessary duplication and gaps, and an explicit role for CCSP in addressing stabilization of greenhouse gases be included in the strategic plan.

If a key justification for the research programs is decision support for Federal decision makers, then the policy positions and questions of the Executive and Legislative Branches of Federal government should be articulated in the science plan as a basis for designing a focused and prioritized science agenda. Clearly, the current level of scientific understanding of climate change is deemed sufficient by many senior officials to warrant slowing the growth of greenhouse gas emissions in order to reduce the adverse effects and/or risk of climate change. A remaining key policy question, then, is "What is the level at which the concentration of greenhouse gas concentrations needs to be stabilized and the growth of emissions reversed to prevent dangerous human interference with the climate?" The President recognizes that "no one can say with any certainty what constitutes a dangerous level of warming, and therefore what level must be avoided" (http://www.whitehouse.gov/news/releases/2001/06/20010611-2.html). So how will the level that must be avoided ever be defined? CCSP could tackle the scientific aspects of defining "dangerous interference with the climate" and "the level of greenhouse gas concentrations that must be avoided", while also articulating that social, ethical, economic and other factors also play major roles in deciding what levels of greenhouse gas concentrations and interference with climate are judged to be acceptable. Science alone will never be able to define the benefits and costs that are acceptable to society and determine what level of warming must be avoided. Scientists can articulate the role of science in a value-laden risk-assessment and risk-management framework, and should seek to temper the often over-stated policy position that decisions rest mainly on scientific justification.

In his January 8, 2003, testimony to Congress, Dr. Mahoney stated on Page 11 that "Maintaining a vigorous, ongoing program of basic research, funded and managed independently of the climate assessment activity, will be crucial for narrowing these uncertainties" (http://www.climatescience.gov/). I recommend that the plan more clearly define the extent to which the various components of CCRI and the USGCRP are intended to meet the needs of decision makers - especially policy makers - versus provide for continued funding for scientific investigation. Also, the plan should identify the basic research components and articulate how these will be funded and managed independently of the assessment activities. I recommend that decision support be accorded much earlier, greater, and consistent emphasis in the plan. Key decision makers, decisions, decision-making needs, and policies should be identified explicitly. Criteria for setting research priorities should be included in the strategic plan, not left to development of implementation plans for individual programs necessary to "meet the key science objectives and the roles of each participating agency" (Page 164).

Second Overview Comment: There is ambiguity as to the scope of the plan, particularly whether it is intended to be a US Federal Executive Branch plan, or a broader US plan. For example, it states in the Foreword that the CCSP "coordinates and directs the US research efforts in the areas of climate and global change", but the overwhelming dominance of Federal agency employees in the preparation of the plan clearly points to this being predominantly a Federal Executive Branch plan, rather than a US plan. Indeed, the Global Climate Change Executive Summary states that "The Secretary of Commerce and Secretary of Energy have completed their review of the **federal** government's science and technology research portfolios and recommend a path forward" (http://www.usda.gov/oce/gcpo/Feb14storybook.pdf).

Apparent exclusion of anybody but government officials from making policy or managing resources is further evident on Page 11 where it states that "Policy and resource management decisions are the responsibility of government officials ...." On Page 38 it states that decision makers are defined as those who engage in the development of national policy; there are only resource managers in regions and sectors! Federal Executive Branch agencies clearly have a major role to play in identifying decision issues, but the Federal Legislative Branch, states, industry, and other stakeholders have roles to play and need to be involved in identifying issues at both the national and regional levels (Page 41), if it is to be a comprehensive national plan. Although on Page 41 it identifies the need to address decision support resources for regional resource management, states also have their own climate change policies, either separate or embedded in energy and environmental policies. Some states also support scientific research and monitoring related to regional and global climate change. For example, the Illinois State Water Survey is a leader in developing, testing, and applying a regional climate model linked to a regional air quality model, but the CCRI modeling agenda seems to focus exclusively on global models. I recommend greater participation in preparing, approving, and executing the plan by the US Congress, state governments, industry, non-governmental organizations and others for it to be a truly US plan. Alternatively, the plan could be called a Federal Executive Branch plan.

Although the plan is stated to be for a climate change science program, some major elements are included whose realm is climate variability rather than climate change, for example, ENSO forecasting, which primarily is in the realm of seasonal and interannual climate variability rather than climate change. I suggest that either I) the title of the plan be changed to something like a strategic plan for a climate science program, in which case seasonal and interannual climate variability can be included, or ii) the program elements whose prime domain is not climate change be deleted, or iii) only the relevant climate change portions of basically non-climate change programs be included in a climate change plan.

Another part of this ambiguity is whether the plan is intended to be restricted to climate change (as in the title of the plan (Climate Change Science Program) and The Climate Change Research Initiative), or to include climate and global change (as in the

Foreword, Introduction, and US Global Change Research Program). These ambiguity should be resolved.

Third Overview Comment: Direction of the CCRI and the USGCRP receives little attention and is not clear. On Page 162 it states that the CCSP oversees and coordinates the CCRI and the USGCRP. But the very next sentence states that the Subcommittee on Global Change Research, of which the CCSP is a member, coordinates the USGCRP. As noted above, it states in the Foreword that the CCSP coordinates **and directs** [my emphasis] the US research efforts in the areas of climate and global change. There is a very definite need for direction of these programs, as well as coordination, to ensure that they meet the needs of decision makers in particular. Clarification should be provided as to how the CCRI and USGCRP are directed, by whom, and how.

Fourth Overview Comment: The President has recognized that "Global and regional scale climate modeling increases our understanding of, and our ability to assess, changes in climate variability and their impact on the environment" (http://www.whitehouse.gov/news/releases/2001/07/20010719-6.html). However, the section in the CCRI on Applied Climate Modeling on Page 47 is restricted to global modeling at two centers and seems, therefore, to be inconsistent with The President's position. The section should be made consistent with The President's position on regional scale climate modeling by including recognition of the importance of regional scale climate modeling, progress already made in regional climate modeling, and the need for further development, testing, and application of region climate models. Also, progress already made in regional air quality modeling, and the need for further development, testing, and application of regional air quality models linked to regional climate models should be included.

The "two center strategy" is too narrow for a US plan and should be broadened. On Page 141 it states that the USGCRP needs to improve climate forecast capabilities for regional applications and risk reduction, and on Page 156 that regional-scale cooperative research needs to be planned and implemented (in the international arena). Discussion of reporting and outreach to local/regional governments, businesses, and NGOs on Page 151 places heavy emphasis on the Federal government providing data and information to them, with little emphasis on these important constituents providing data and information to the Federal government. Again, the states in the US should be accorded more recognition and attention from both scientific and policy perspectives.

Fifth Overview Comment: The plan is stated to be a draft strategic plan and identifies research needs and products, but omits the central component of most strategic plans - to identify strategies. There is one sentence on Page 11 which sates that "The strategy for each major area of the program is described more fully in an accompanying set of white papers, which address the issues in greater depth." The reader has to reach Page 163 before finding out that the Scientific Steering Committees will be established to develop detailed science plans. However, the white papers that are available, while providing more detail, still do not contain explicit strategies. The basic format for the white papers is the same as for the draft strategic plan, which is to identify research needs and then to jump to products. I recommend that strategy sections be included in the strategic plan, or, at a minimum better explain the nature of this strategic plan in the Introduction and/or Foreword and then include strategies in the white papers.

An example of an apparent inconsistency in program scope and a lack of strategies is evident in descriptions of the CCRI. On Page 9 it states that “The CCRI provides a distinct focus to the overall research program. This focus is defined by a set of uncertainties about the global climate system ....”On Page 41 it states that “The CCRI will devote attention to the type of institutional changes necessary to forge effective interactions between research processes and policy development.” If CCRI is to devote attention to institutional changes as well as climate change research, then the goals and objectives of the program should be stated more clearly up front. If CCRI is to address institutional change, what are the strategies for doing this?

Without priorities and strategies being identified and related to resource availability and resource needs, it is not clear whether all the products can or should be produced with a continuation of existing programs at level funding, whether some existing programs would be reprogrammed and/or terminated and new programs started with level funding, or whether additional resources are needed to meet the research needs. Also, without strategies, it is not clear whether, for example, research supported by states and industry would be included in what is characterized as a US plan.

In chapters 2 and 3, specific questions lead to "research needs" which lead in turn to "products and payoffs". In chapters 5-12, on the other hand, there are only "illustrative research questions." However, there are still products and payoffs with 2-4-year payoff times. That seems unrealistic when the research topics aren't even known yet.

On page 86, Research Needs is the sentence "Overall, there is a basic need to develop an integrated research vision (complete with hypotheses) for addressing multiple-process (hydrological, physical, chemical, and ecological) interactions between water and other Earth systems." This seems to be an admission that scientists have not yet developed an appropriate vision, yet there are concrete products and payoffs with relatively short delivery times.

Sixth Overview Comment: Performance evaluation is an important component of implementing strategic plans, but the plan does not state how performance will be evaluated. Perhaps this will be stated in a companion management plan, but as program management is included in Chapter 15, I recommend that performance evaluation and accountability be addressed in Chapter 15. Also, the plan also should state who evaluates CCSP for decision-support relevance, to complement the NRC evaluating USGCRP for scientific merit (Page 10).

Seventh Overview Comment: It is stated on the first line of the Decision Support Resources chapter that the CCRI will synthesize the results of the research conducted by the CCSP (which includes USGCRP) to present critical information to decision makers and resource managers both within and outside the US Government. While synthesizing CCSP research results is important and necessary, it is also important and necessary that some group in the US synthesizes all relevant research results (CCSP, US non-Federal research, and research in other countries relevant to US interests) and make these assessment available to decision makers, resource managers and the public in the US. For example, researchers in other countries use different models to project climate and the results of these models can be different from the US models. As these model results also are used in global, national, and regional assessments of climate change, it is important that the US should diagnose and evaluate the performance of these models as well as US models. Such diagnosis and evaluation will provide the US with a broader scientific basis for commenting on the policy positions of other nations that may be based on or influenced by these model results, and a broader scientific basis for developing US policy positions.

1. The challenge for society is stated to be discerning whether human activities are causing observed temperature changes and impacts (Page 5). While temperature undoubtedly is a major climatic variable of concern, precipitation and other climatic variables are of equal, or perhaps greater importance in many regions. The importance of other climatic variables should be given more recognition.

2. One of the three research foci in Chapter 2 is carbon sources and sinks in North America. Certainly, management decisions in North America can affect carbon sources and sinks, but as greenhouse gases are long lasting and thoroughly mixed in the global atmosphere and international carbon trading could become important, it would seem to be more appropriate to address global sources and sinks of carbon. Chapter 7 on the water cycle addresses the global water cycle, not just the water cycle in North America.

3. Little mention is made of groundwater-level and groundwater-quality monitoring, while much emphasis is placed on precipitation, soil moisture, runoff, and stream-flow. There should be more discussion of the importance of potential climate-induced changes in groundwater levels and groundwater quality and the need to monitor groundwater levels and groundwater quality.

4. A couple of comments buried in Part II belong in Part I. "In order to understand the impacts of land use and land cover change, *there must be ongoing close cooperation with other CCSP research elements* that will improve understanding of the interrelationships and dynamic feedbacks ... ." (p 96) " "Determining the most important and societally relevant ecosystem responses to global change *will require collaboration among the physical, biological, and social science communities* ... ." (p 116) With a little rewording these could be added to the guiding principles of the whole program on Page 11.

WISCOMBE, NASA GODDARD

Omissions

The word "balloon" is used several times in the draft plan, but, unlike the other platforms mentioned, there is no plan to actually develop balloons for Earth Science in any federal agency. One might think NASA is working on this, since balloons are "airborne", but they are not. NASA only develops gigantic balloons, capable of lifting two tons, for space science applications. No NASA funds whatsoever are being devoted to developing advanced balloons customized for Earth Science observations, where several hundred pounds would be considered a reasonable payload. And no other federal agencies have programs in the balloon area of any consequence.

General Comments

There are of course many kinds of balloons. One developed by the French skims the ocean surface. Others can float at mid-levels in the atmosphere, say 5-10 km altitude. Others make quasi-vertical ascents (radiosondes). My concern here is mainly with stratospheric balloons that fly in the quiescent air at 35 km altitude (although technologies developed for such balloons would have important spinoffs for other balloons as well). This 35-km altitude is basically in space since there is an insignificant amount of atmosphere above the balloon; from 35 km, one sees the black of space at all times of day (many satellite scientists would find this capability useful for testing and prototyping their instruments and technology).

Why are such balloons a concern? Because they offer a key capability now missing from the airborne part of the global change program: low cost coupled with long time on station. Ocean buoys have this capability, and look how useful they have proved! And at least two technologies now exist to give balloons some steering authority, enough to keep them well spaced from other balloons, avoid cities, and avoid being trapped in vortices. As to cost, even the most expensive large balloon would not cost more than roughly $0.25M; compare this with $10-20M for an aircraft, and $100M for a satellite. A properly-sized Earth Science balloon would cost no more the $100K. If you look at bang for the buck, balloons offer at least a factor of 10 more data per dollar than any other airborne platform -- and this is a very conservative estimate.

As to time on station, balloons now on the drawing boards could remain aloft for over 100 days, and, with further development, up to a year; compare this with conventional research aircraft which at present can remain aloft for no more than 8-10 hours, exotic unpiloted aircraft (UAV's) which can remain aloft for a week, and satellites which work for 5-10 years. Eventually UAV's will be able to remain aloft for months, but at what price in terms of cost, ground support, and repair?

Any scientist who now wants to take observations for a period longer than a week (which encompasses most climate observations) is now forced to consider a satellite flight. Unfortunately, getting a satellite flight is harder than squeezing the proverbial camel through the eye of a needle. Funds for new satellites are scarce, and getting scarcer. A vigorous balloon program customized for Earth Science needs would provide a capability to bridge across this "Temporal Valley of Death" and provide observations on seasonal to yearly scales. And bringing down a balloon, recovering and refitting its payload, then relaunching it, is so easy that multi-year flights are well within the realm of possibility. Satellites and aircraft offer the advantage of greater speed, but the truth is, many global change phenomena do not require "racing to the scene of the crime"; they can perfectly well be studied from a slow-moving platform with some albeit limited steering authority.

Single balloons will be much less useful than constellations of balloons. One can imagine a constellation of 400 balloons offering global instantaneous coverage for about the cost of a cheap satellite. Other topologies, such as the "String of Pearls", could provide a constant presence over a hurricane. Constellation topologies are limited only by one's imagination. And much can be learned from autonomously managing balloon constellations, that would transfer directly to formation flying satellites in space.

Finally, one should not overlook the dropsonde application. Preliminary ideas for replacing the decaying world radiosonde network with dropsondes, or providing "targeted observations" with dropsondes, have been put forward by a NOAA scientist. This would be a revolutionary advance; however, the proposed platform is the Global Hawk, a $20M aircraft. Buying a fleet of Global Hawks would be cost-prohibitive. By contrast, a constellation of stratospheric balloons, each loaded with 1000 dropsondes, could provide a similar capability at less than 1% of the cost -- quite affordable even under the current global change budget.

WYNDHAM, CITIZEN

This issue (global warming) has been studied to death. It's time to act. Stop stalling and start listening to the scientific reports already compiled.