STRATEGIC PLAN FOR THE CLIMATE SCIENCE PROGRAM
COLLATION OF COMMENTS – ADDENDUM
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General Comments

Earth Institute, Columbia University

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.
(Neil Ward, Shiv Someshware, Shiv Someshwar, Carolyn Mutter)

My comments are on the natural science aspects of the document. I have read chapters 1-7 and 12.
Overall this is a good document, reflecting current understanding of the climate system and the limits of our knowledge. It lays out an ambitious, comprehensive research program, which, if it could be carried out, would accelerate gains in understanding of climate change. To do so demands a generous increase in the budget allocated to this "War on Global Warming" (on the order of 2% of the cost of the war on Iraq). It may also require an increase in the number of scientists working on these problems. Since neither increase is likely to be forthcoming, the plan may be criticized for not setting the priorities needed to carry out the triage a more realistic budget makes inevitable.

It is worth mentioning that the overall numbers presented as the amount of money the US has spent on Global Change research must be broken down a bit to appreciate what it has meant for research. Most of the money has gone into NASA's earth observing satellites. It would be worthwhile to analyze the "almost $20 billion" claimed as the US "scientific investment" over the past 13 years (p9 line 16ff). What did it actually go to? I suspect much of it is just taking credit for all the usual research in the climate area. Is the spending structure and trajectory much different from what it was before the USGCRP? While this system is an essential part of the suite of climate observations, the allocation going to research and surface observations has been unimpressive. It has not been an adequate response to the potential threat climate change poses to the US and the world.
(Mark A. Cane)

A few caveats. I am affiliated with NASA GISS and the Columbia Earth Institute, but these are my personal opinions not an organizational position. Also, I am giving here only my criticisms of the current plan, so I would like to apologize to the people who prepared it, because there are many excellent statements and sections in the report. Finally, I recognize that the plan is in a sense a product of all of us, who, one way or another, have contributed ideas to it, so I realize that we share the responsibility.

Now, let's ask how well this plan responds to the direction of the President when he announced the Climate Change Research Initiative June 11 last year. The President said "Climate change … is an issue that must be addressed." He said specifically "We will act, learn and act again, adjusting our approaches as science advances and technology evolves." In explanatory statements it was indicated that we knew enough from research in the past decade that "acting now" included energy efficiency, encouraging more renewable energy, and developing technologies for the future. He mentioned 2012 as a time for reassessment, in recognition of both the decadal time scale of the problem and the fact that fundamental advances in our understanding of climate and in our technologies are needed to develop the most effective strategies for dealing with this issue. And Dr. Marburger mentioned that the decadal time scale will not prevent prompt actions in response to discovery-based research and assessments.

So, how well are we, with this plan, achieving the intentions that the President set for us? What the plan seems to have are reports and products in 2 years, 4 years. And we seem to be getting an added level of bureaucracy, which thinks that scientific progress can be "ordered up". The scientific community is already handicapped by excessive report writing and proposal writing for less and less research funding. However, instead of providing increased resources and getting out of the way, the plan seems to be to create more bureaucracy, thus reducing resources for true research and taking from our most valuable resource, the time spent on innovative productive research, which is needed to make substantial progress in our scientific understanding and without which we cannot provide the information that policy makers seek.

There is a naivete in the plan that reminds me of my first proposal for global climate modeling, which I wrote in the mid 1970s to solve the climate impact of ozone and CFCs in 3 years. In rejecting the proposal, Don Hunten said that instead I should indicate some recognition of the decadal time scale of the problem and the need for a broad base of understanding developed from the combination of observations and models, so that we could respond promptly and effectively to decision makers as new issues about stratospheric ozone arose.

In important ways, the climate plan fails to see the forest for the trees. It points out the importance of understanding climate sensitivity, and it attacks this issue by proposing to compare two models, a useful exercise that will reveal some problems in one or both models, but it will do very little to advance our understanding about climate sensitivity of the real world. Besides, climate sensitivity is more yesterday's problem than today's. We know that climate is sensitive from paleoclimate evidence. Whether it is 3C for doubled carbon dioxide or 2C or 4C is worth knowing, and, now that adequate computer power is on the horizon, significant progress in our understanding is possible, provided that we are given the resources not only for computer power but to support the brainpower needed for the research and observations and field work that are inherently expensive.

However, we need to demonstrate that we understand what the large issues are today and tomorrow. For example, all nations agree that we need to avoid the level of "dangerous anthropogenic interference" with climate, but we have provided precious little information to decision-makers about what that level is. We could use a fundamental issue such as this to make clear that what we need is a broad decadal research program that gives primacy to science, with all that implies, including observations. As a specific example, I think that there should be a major effort to improve our understanding of how stable or unstable the ice sheets are to the strong surface forcing that is now being applied to them. The ice sheets, I believe, will be the issue that defines the level of dangerous anthropogenic interference, yet this issue is treated simplistically by IPCC and receives short shrift here. The necessary understanding requires increased support for glaciology and development of improved modeling capabilities for this highly nonlinear problem, including full account of the effects of summer precipitation. Investigation should aim at determining how close we are to the point when future irreversible disintegration will be unavoidable.

One puzzling thing about this document is that it seems to be two documents stapled together. This raises concern that we may be getting layers of added bureaucracy, which could reduce science support in more ways than one. I could not study each of the more than 150 pages. However, I discerned a distinction between the two parts. For example, the modeling chapter in the second document starts out with a statement "The study of global change requires a strong base of observations", and in general I found many such good rational statements. Also I thought that the organization of this second part of the document was very good, so let me end my comments on this positive note. (James E. Hansen)

Soulen, J. Richard – Retired

A large number of efforts are called for in the Review Draft, many of which are continuations or augmentations of what is being done in the USGCRP. It is difficult, however, for the reader to discern which of the *areas* of effort are most vital, and, within each area of effort, which of its component *activities* are most necessary. Statements such as "Modeling is one of the most important components of the CSRP" and "Emphasize comprehensive climate response simulations" give some indication, but the final Plan would be improved considerably by the inclusion of well thought out *prioritizations* both *of* and *within* all areas of activity in the Plan. Such information would be important for effective management and direction (or re-direction) of the overall effort.

Regarding priorities among the major *areas* of effort, I believe that the *Science of Climate Change* is so vital that its importance can hardly be overestimated. Accurate answers to the core questions "What has happened?" "What has caused what has happened?" and "What will happen?" are crucial as inputs if we are to obtain accurate, reliable answers to questions regarding "downstream" areas, i.e., effects of climate change, and effective measures for mitigation and adaptation.

<u>Second Overview Comment</u>. ***State Needs More Explicitly***.

An example: Lack of adequate computing power has been a chronic problem of U.S. scientists addressing climate change. As mentioned in the Review Draft, NRC reports have called attention to it. Quoting from one of them:

> "The ability of the United States to assess future climate change is severely limited . . . by inadequate computational resources . . .."
> (Review Draft ref. NRC, 2001a, p. 24)

This is a serious matter, but the Review Draft is not specific as to how this situation should be rectified. It does state that "substantial and continuing investments in high-end computing" are required for success. It would be helpful, however, if the resources -- especially supercomputing hardware – needed to meet our scientists' needs were described explicitly.

Similar information should be provided for all high priority activities, especially those that have previously been lacking in the resources necessary for timely accomplishment of goals. Incorporating this information into the final Plan as a series of Appendixes might be the most effective way to highlight these needs.

Some people may believe that a document entitled Strategic Plan should contain general information regarding what is to be done, but not details on how things are to be accomplished. I believe, however, that the final Plan would be considerably stronger if, for high priority activities at least, all three of the questions, "What?" "Why?" *and* "How?" were addressed.

This would have two advantages. It would give those who requested the Plan confidence that matters have been thought through to the point of knowing, explicitly, what is required to accomplish the high priority goals. And it would tell those who provide funding the most important needs of the program.

Wagner, Thomas - University of Michigan

"What processes determine the temporal and spatial distributions of land cover and land use change at local, regional, and global scales, and how can land use and land cover be projected over time scales of 10-50 years?"

"How may the dynamics of land use, management, and cover change affect the global environment and national environmental and socioeconomic conditions, including economic welfare and human health?"

These two questions should be revised. The 1st question is too broad and unfocused and allows "fishing expeditions" of doubtful scientific consequence. Question 2 is the heart of the matter: this objective in studying landscape dynamics should guide future research but fails to recognize that land cover/use is a consequence of human activities as well. The question should acknowledge the cyclical nature of this process and explicitly state that social science is necessary to understand the drivers of land use/land cover change.

Weber, T.

Although this is not a formal comment, I suggest that the climate change staff read the following article:

Parmesan, C., and G. Yohe. 2003. A globally coherent fingerprint of climate change impacts across natural systems. Nature vol. 421 (2 Jan 2003), pp.37-42.

This is a recent study demonstrating the disruption of climate change on species and natural communities, and the need to provide habitat reserves and migration corridors. In

Maryland, we have identified and designed such a reserve system (http://www.dnr.state.md.us/greenways/greenprint/)

The Nature abstract reads: Causal attribution of recent biological trends to climate change is complicated because non-climatic influences dominate local, short-term biological changes. Any underlying signal from climate change is likely to be revealed by analyses that seek systematic trends across diverse species and geographic regions; however, debates within the Intergovernmental Panel on Climate Change (IPCC) reveal several definitions of a 'systematic trend'. Here, we explore these differences, apply diverse analyses to more than 1,700 species, and show that recent biological trends match climate change predictions. Global meta-analyses documented significant range shifts averaging 6.1 km per decade towards the poles (or metres per decade upward), and significant mean advancement of spring events by 2.3 days per decade. We define a diagnostic fingerprint of temporal and spatial 'sign-switching' responses uniquely predicted by twentieth century climate trends. Among appropriate long-term/large-scale/multi-species data sets, this diagnostic fingerprint was found for 279 species. This suite of analyses generates 'very high confidence' (as laid down by the IPCC) that climate change is already affecting living systems.

Specific Comments

Page 26, Chapter 3-General Comments

There are many strengths in this strategic plan for the management of climate data and observations. It emphasizes 1) life-cycle data management practices; the importance of obtaining and maintaining decades-long time series of data; 3) the provision of widespread and open access to climate data by both scientists and policy makers; 4) the maintenance of global monitoring systems; and 5) support for data processing. These are all important.

There are three areas in the plan, however, that still need to be addressed. First, despite the frequent reiteration of the importance of making climate data accessible to policy and decision makers, this strategic plan is too narrowly focused on data needs for scientific research and for the reduction of scientific uncertainty. Although one of the strengths of the report is its recognition that climate change data and information are needed for management, policy, and decision making as well as for science, the data needs of applied, non-scientific users are ignored. Moreover, the reduction of scientific uncertainty can be used to drive data collection, but should not eclipse the equally important policy and management need of identifying both short term and long term trends in climate—from whatever cause.

A strategic plan will never be able to anticipate all the data and information needs of future research and policy. What it should do, however, is establish an open and ongoing process that will identify data and information requirements as they evolve and change. The process should encompass data users among both research scientists and managers and policy makers, recognizing that although there will be areas of overlap between the two groups, there will also be some data needs that are unique to each.

Second, data rescue and reconstruction of time series, recommended in the strategic plan, are difficult research tasks. Time series data are essential for understanding climate change, and this plan not only recognizes their importance but also recommends data rescue (which it also calls data archeology) as a means of constructing such time series post hoc. Yet data obtained after the fact are likely to be incomplete or to be spatially or temporally patchy. Missing values must be estimated. There may be a need to splice disparate data series. These are research tasks that will require a deep understanding of the properties of the data and the underlying phenomena that are being monitored. Data rescue may also require the development of surrogate measures. The development of new measures—indirect measures—is implied in this strategic plan, but is not discussed.

New types of data will be needed to understand human impacts and anthropogenic forcing. The strategic plan calls for “comprehensive documentation about the full spectrum of climate forcings, feedbacks, and responses, especially over the past century when human influences have been most pronounced” (p. 27, lines 29-30). Despite this assertion, however, there is little discussion in the plan of the socioeconomic, institutional, or behavioral data needed to understand either the forcing functions or the impacts of climate variability and change. Greater attention should be given to the issue of how to identify and obtain these data.

Third, the requirements of the data and information system are not spelled out in the plan. The strategic plan speaks of life cycle management without explaining what this means, either now or in the future. At a minimum, life cycle management will require long term, permanent archives that are able to ensure that data from climate change observations and monitoring systems are not lost through technical obsolescence in the storage, documentation, or dissemination media. It requires technological flexibility and an active advisory structure that can articulate the changing data and information needs of science on the one hand and policy, management, and decision making on the other.

Roberta B. Miller, Center for International Earth Science Information Network, Columbia University

Page 38, Chapter 4 – General Comment:
Implicit in this Chapter and parts of Chapter 6 (Climate Variability and Change) is the assumption that scientific research can and will reduce uncertainties and that these scientific advances will clearly indicate which policy actions should or should not be undertaken. 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. Basic and applied scientific research on decision processes used by key decision makers is required for the development of tools and methods for adaptive decision making under uncertainty. More is known on this topic (and related topics of judgment and decision confidence and confidence assessment) than the report acknowledged. At the same time, a lot of work remains to be done, and the issues are often more complex than acknowledged. Decision support is far more (and far more complicated) than the “provision of timely and useful information that addresses specific questions asked by the decision maker” (p. 43, line 8). The draft does not address the fact that there are different types of decision makers and different types of decisions that will

need to be supported. Many of these issues have been addressed in the National Research Council Report, *Making Climate Forecasts Matter*. (In the interest of full disclosure, I was a member of the National Academy of Science committee who wrote the report.)

Elke U. Weber, Columbia University

Page 38, Chapter 4, General Comment

Decision support refers to the provision of timely and useful information that addresses specific questions of decision makers. The long term nature of climate change signals will not be able to address the prediction requirements of decision makers – generally in the 1 to 5 years range. A key contribution to addressing this gap – between the expectation of decision makers and the ability of climate change science to provide information – is through the seasonal and inter-annual time scale of climate variability. IRI research on climate variability and its management would be of immense value in this regard.

Much of the writing about anticipated future climate information use by decision makers has not taken into consideration the perspective or context of the decision opportunities. The shift in focus within the US, from energy policy to socio-economic development, in the development of climate change science information, requires addressing the spatial and time scale needs of decision makers. Seasonal to interannual time scales and state level spatial scales dominate the decision making matrix in these latter sectors. Addressing the climate information needs at such scales would require tapping into the experience of institutions such as the IRI .

Real world demonstrations of the value of climate forecasts can be a primary mode of instilling confidence amongst policy makers and decision makers – indeed this was alluded to in the keynote speech of Prof. Obasi, WMO Secretary-General, noting that for decision makers to better use seasonal-to-interannual information was a route to building confidence for making difficult decisions on global change. It is an opportunity for decision makers to recognize the inherent probabilistic nature of climate information, and become comfortable with utilizing such information. Research and real-world project implementation on climate variability and its management would be of immense learning value in this regard. The IRI is an institute committed to such demonstrations.

In order to effectively address climate change impacts we need to harness the successful experience in managing climate variability. At the IRI this aspect of research includes the following perspectives: 1. Scenarios for identifying key policy arenas. In addition to the climate variability other drivers include demographic change, land use, water use etc. The scoping and development of the scenarios would need to include key stakeholders. They also need to introduce appropriately scaled forecasts to identify key policy areas, and subsequent decision support activities; 2. Identification of the most vulnerable sectors and populations; 3. Identification and spatial mapping of society's vulnerabilities to climate and the opportunities for better use of climate forecast information, based on key indicators. 4. Identification of existing practices and policy arrangements and their potential to enhance the management of climate variability.

Incremental improvements in scientific capability, or reductions of uncertainty, are not likely to lead automatically to improvements in decision capacity. That presently available information is generally not utilized speaks to the misfit of anticipated information needs from variable (and variably uninformed) perspectives of scientists and decision makers. Learning from the present is critically important for development of future what-if scenarios – especially regarding the networks that must be developed for use of information, in addition to the development of the information itself.

Neil Ward, Shiv Someshwar, Carolyn Mutter, IRI Columbia University

Page 68, Chapter 6 – General Comment

I would highlight two shortcomings of the Plan in the general area of "Climate Variability and Change" (Chapter 6):

The consideration of abrupt climate change puts too little emphasis on paleoclimate research. Contrary to the impression given on page 69, comprehensive models do NOT reproduce the large amplitude abrupt changes evident in the paleoclimate record. At present, we have no comprehensive models that generate Heinrich events or D/O events or something like the Younger Dryas period. In the latter, we now know that temperature changes 2/3rds as large as the difference between the ice age minimum and present values occurred in only 10 years. We have no idea why or how this happened, and no model has done anything like it. This was the tail end of the ice age, so conditions were quite different from the modern, so whatever happened may have little in common with the earth's immediate future. Nonetheless, it should be disconcerting that the models we rely on to foretell the future seem to be so much more sluggish than the natural climate system. It is important to understand why this is. It is also true that there are rapid changes far larger at times within the Holocene that are quite similar to the present which are not simulated in our models. Without further work on abrupt events in the paleoclimate record culminating in some understanding and ability to simulate these events, we should not rely on the models' predictions of a smoothly changing future.

Though the document mentions Reanalysis, it is not given much prominence. The present reanalysis products, which are flawed and limited in time, have become a mainstay of climate studies. They are, arguably, the most cost effective product we have. It should be a high priority to improve and extend the reanalysis.

Mark A. Cane, Columbia University

Page 68, Chapter 6 – General Comment

Question 2 – To what extent can predictions of near-term climate fluctuations and projection of long-term climate change be improved…

There is much good discussion of climate fluctuations and climate change in this section. There could be better definition given to the role of better understanding and predicting climate fluctuations (seasonal to interannual timescale) for (i) genuinely improving the representation of these features in global change scenarios, which most recognize is needed for realistic global change scenarios, and is essential for downscaling regional estimates, and (ii) by demonstrating ability at seasonal-to-interannual timescales, to build confidence amongst decision makers and therefore reduce perceived uncertainties in global change projections. There are examples of where this comes out (e.g. p73, lines 26-30, p74 line 31-32), but there is a danger of these points being lost, as they are contained within broader discussions of issues that are specifically oriented at change or variability. A separate section on the intersection and value of variability work to reducing uncertainties in change estimates (and especially regional change estimates) would be useful. The overall question might also be rephrased to reflect this.

Cross reference can be made back to Applied Climate Modeling (sub section 3 in Chapter 4), p48, section entitled “Enhance Model Credibility through a Formal Program of Model testing”. Testing of models for their ability to simulate and predict interannual variability is recognized as a valuable way to achieving enhanced model credibility. The IRI is already contributing to the climate community such an activity, co-coordinating the verification of model predictions from past years, and the generation of real-time 3-6 month ahead predictions, for a suite of state-of-the-science atmospheric GCMs. This comes about through IRI collaborating with NCEP and other U.S. and international institutions. This could be built upon further. Despite the fact that progress has been made in the creation of operational seasonal predictions, injection of enhanced technical infrastructure would further accelerate progress substantially, and IRI and others in the U.S. community are well placed to take advantage of such an increase in technical infrastructure and consolidate this key contribution to the international stage.
Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 69, line 9 – may be better to state that global change research has significantly *contributed* to our knowledge of the …. (along with other programs geared more directly toward the seasonal to interannual problem).
Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 69 between line 10 and 11- definition of climate effects – currently gives examples that are mainly environmental (floods, droughts, wildfires, sea level changes) – include some more specifically socio-economic, like economic recession, mass migration, increased poverty, slowed economic development.
Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page73, line 9. What is the ‘global average characteristics of climate variability’? Global coupled models still have difficulty with ENSO – this statement gives an over-optimistic impression that current models used for global change scenarios can accurately represent climate variability. Suggest it should be removed or reworded.
Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 73 line 6-7 –“Provision of probabilistic estimates of regional fluctuations in the climate resulting from ENSO extremes (5-15 years)”.
It is assumed that this refers to seasonal predictions with a lead-time of several months – this should be clarified as some may read it to be extremes associated with global changes in ENSO (our ability to provide information on which is considerably further off, though the aim referred to in this bullet is an essential step toward such a capability).

- Institutes like IRI are already developing methodologies for the goals in this bullet – arguably we are in a position to provide estimates already, and further improvements expected.
- It should be noted that there is great value in working in regions where the ENSO signal is higher, to develop such methodologies, rather than solely focusing on

mid-latitude regions like the U.S., where signals are lower and it is more difficult to robustly identify the best methodologies for downscaling.

- In this bullet, it should be driven home that achieving this goal is critical to capturing these effects in global change scenarios. Global models should be able to capture this tropics-driven interannual variability, and we should be able to confidently downscale it to regional scales (and testing on the interannual timescale is a route to building such confidence in downscaling). Only then will it be possible to attach scientific credibility to regional downscaled estimates of extremes based on global change scenarios.

Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 73, line 12. For this bullet, it is really not clear whether these are seasonal to interannual predictions or regional predictions in global change scenarios. If it is referring to seasonal-to-interannual predictions, reference can again be made to the methodological progress already made by IRI and others who are addressing this problem. Again, the 5-15 year timeframe seems long – these seasonal-to-interannual regional prediction questions are more likely the ones where significant further progress can be delivered in the next 2-4 year timeframe.

Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 73, Line 37-43, page 74 line 1-2Research Needs is very general. Needs to be sharpened to deliver the products and payoffs, especially those related to variability.

Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 76, Question 4. Whether and how are the frequencies, intensities and locations of extreme events, such as major droughts, floods …. Altered by natural climate variations and human-induced climate changes. Lines 10-27 – do not reflect variability – again, a key point can be how successful downscaling of seasonal predictions builds confidence in models and methodologies. Furthermore, since global change projections contain variability – any downscaling should be able to downscale successfully the variability (e.g. ENSO variability) within that global change projection. These issues are better described in the Question 5 piece, e.g. p77 line37 – p78 line 4.

Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 80, Chapter 7 – General Comment
The Water Cycle Chapter of the Strategic Plan for the Climate Change Science Program identifies a large number of issues and research areas organized by two major "overarching questions": 1. How do water cycle processes (including climate feedbacks) and human activities influence the distribution and quality of water within the Earth system, to what extent are changes predictable, and how are these processes and activities linked to ecosystem and human health and the cycling of important chemicals, such as carbon, nitrogen, other nutrients, and toxic substances? 2. How will large-scale changes in climate, demographics, and land use (including changes in agricultural and land management practices), affect the capacity of societies to provide adequate supplies of clean water for human uses and ecosystems and respond to extreme hydrologic events?

In general, these are reasonable questions. While the activities sketched out in support of these questions will no doubt lead to useful insights as to the role of the water cycle in climate and human vitality, a clear sense of priorities for scientific enterprise or policy support does not emerge from the document. Rather than dissect the document and attempt to synthesize such priorities form the material therein, I offer an alternate perspective with respect to these overarching questions.

1. The importance of global, regional and local water cycles to (a) human sustenance, (b) ecology, (c) basic and applied science, and (d) understanding and foretelling climate, is such that it would be useful to set up a separate entity that funds and prioritizes Water Cycle Research including its interface to the Climate programs. The Water Cycle research agenda is clearly much broader, and at least as important as that of the Climate and Global change programs. Arguments in support of this recommendation include:

i. Need for theoretical development of the integrated water cycle dynamics: While theoretical climate dynamics defined in terms of circulation of the atmosphere and its coupling with the ocean is reasonably well developed, the dynamics of the water cycle (spanning, ocean, land, ice, atmosphere, the subsurface and the biosphere) is not as well understood. The role of water vapor in the radiation balance, as identified in the USGCP document, is critical, but is only one of the factors of interest. Evaporation, precipitation, melt and water transport dominate the energy budget of the planetary heat engine. It is well known that precipitation is intermittent in space and time. There is evidence that evaporation, atmospheric water transport, oceanic flows, and land water transport are also dominated by intermittent or organized fragments of movement, at virtually every spatial or temporal scale of interest. Understanding the organization, scaling, regimes and recurrence of such fragmentary motions is a basic scientific challenge, and key to any assessment or prediction of the seasonal or longer variations in global, regional or local variations in floods, droughts, biological productivity or geochemical transport. To address this challenge, one needs to go well beyond the representation of climate and water in existing climate models, to identify the critical features that dominate water cycle organization and predictability and how best to model them. At this time, we cannot offer clear scientifically and observationally supported statements as to the dominant mechanisms of variations in flood or drought potential across the country, or of the factors that determine the seasonality of flooding, nor explanations for the severe sustained droughts that occurred in the Western U.S. in the last 6 centuries, nor an ability to explain or predict the dramatic decadal rise and fall of closed basin lakes (e.g., Devils Lake, N. Dakota).

ii. Need to extend the vision of classical "water" disciplines beyond their traditional interfaces: Funding mechanisms for scientific research inevitably constrain or stimulate the directions of research. Thus, a culture has evolved where there is little formal thinking or interaction across the "media" (e.g., ocean, atmosphere, surface hydrologic processes, riparian zones with biological activity, ground water) that traditionally define the movement of water. Each group has developed knowledge within its boundaries, treating water cycle dynamics or exchanges outside its boundaries as essentially exogenous rather than interactive. The climate focus has been beneficial in stimulating much interaction across these boundaries, and forcing the recognition that it is only through an understanding of the interaction across such interfaces and scales that we can understand how water and related fluxes organize. The time is now ripe for a formal focus on the multi-media and multi-scale nature of the water cycle and to foster an understanding of how information is communicated across scales and media.

iii. Need for a comprehensive, integrated multi-scale hydrologic observing system: Given the intermittence alluded to above, and the many scales and processes intersected by the water cycle, it should be clear that there is a need for specialized data sets that allow a description of the phenomena to be put together. Given the concern with change, and the evidence of multi-year and multi-decadal variations in regional water supply and floods, it is clear that a comprehensive paleo-reconstruction of the planetary cycle is an important objective. The use of remote sensing and isotopes and other technologies to better define spatial and temporal aspects of terrestrial and atmospheric water movement are equally important. These are all highlighted in the USGCRP document. However, no argument is made for an institutional effort to bring together such efforts into a coherent hydrologic observing system (with integrated data access) where key data gaps can be identified and additional focused measurement programs stimulated to better identify and define specific mechanisms of water cycle interest. Such an observing system would stimulate exploratory analyses of the nature of hydrologic extremes, teleconnections, cascading of information from coarser to finer scales (and the reverse), and the relative role of different "interfaces" in retarding or enhancing organization and predictability, in addition to providing support for model testing and water balance analyses. The exploratory or diagnostic analysis framework is critical for the field since it will provide a definition of what it is that we need to explain or understand, rather than focusing on the improvement of the resolution, calibration or parameterization of existing models that may or may not translate into understanding or prediction of the mechanisms of real interest. The empirical observation of the El Nino Southern Oscillation is an excellent example. It took over 5-6 decades after the initial observation to synthesize a formal description and provide an adequate mechanistic description of the phenomena and to make a successful prediction. The process of improving the parameterization or representation of this phenomena in generalized models of the global climate continues. Focused field campaigns and monitoring of the phenomena instituted since the specific mechanisms were understood have contributed significantly to the improved predictability of the general models.

iv. Need for a more direct involvement of the water user community in the research agenda: The USGCRP document develops the notion of improved decision making with water cycle research products, and of the associated needs for technology transfer. The direct involvement of users (ranging from public or private sector water "managers" to scientists who rely on water cycle information) will be much easier to achieve in the development and implementation of a research agenda if they had a more direct role from the outset in defining a water cycle research agenda as part of a program in which they had a sense of identity and ownership. The priorities of such a program could differ (e.g., they may be more local or regional scale/problem focused, as in overarching question 2) from those necessarily relevant to a climate science focus, even though an exposure to the climate related issues is very beneficial in providing the scientific and physical context. Thus, this is an issue of generating targeted and relevant missions and substantial funding resources to support that mission independent of the clearly important climate science mission.

2. As noted by the USGCRP, assessment/identification of change,

seasonal to interannual prediction and finally adaptation strategies constitute the building blocks of a viable science plan. In this regard, a water cycle research agenda within the existing Climate Change Science Program framework could be stated as:

i. Develop and document changes (trends, oscillations, regimes) in floods, droughts, seasonality of flow, ground water tables, water quality parameters, and their spatial organization, using historical and paleo-hydrologic (a significant effort is needed to develop such data) data sources. For the spatial structures identified, describe the characteristic time scales (e.g., inter-annual corresponding to El Nino Southern Oscillation, decadal, multi-century or secular corresponding to CO2 induced changes), and assess the importance of different causative mechanisms.

ii. Explore and document the synergy between the changes identified above and those identified in other climatic, biotic or other bio-geo-chemical indicators to robustly assess the underlying mechanisms and to describe how the "common signal" is modified and transmitted to different scales and processes. For instance, one needs to relate how intermittence in daily/seasonal/inter-annual/decadal/multi-century precipitation in a region maps into changes in streamflow, groundwater, lakes, biota and landscape changes - what aspects of the organized intermittence are emphasized and what are muted, and hence what are the potentially predictable features for each scale/media.

iii. Develop a capability to use this knowledge to generate "scenarios" that can be used to explore a variety of management, adaptation, and policy modification issues in the context of regional resource management, national environmental policy and regulation, and global impact assessment and response to water hazards. For instance, it is now well recognized that even in the absence of anthropogenic climate change, climate statistics are non-stationary, i.e. a given 30 year or longer period of data is not likely to be representative of the climate (and hence water fluxes) we can expect in the next 30 years. However, we continue to develop and use estimates of a 100 year flood, or of 10 year 7 day low flows, or of the probability of a particular severity and duration of regional drought under the assumption of stationarity. As a formal understanding of the nature of inter-annual and decadal variation in water fluxes evolves, in part from focused and improved observation and data bases, and in part from the interpretation and modeling of these data, one could propose an alternate adaptive management structure that replaces the prescriptive, regulatory practices currently in vogue. Such institutional changes will not be easy, but are necessary en route to any strategy for adaptation to anthropogenic climate change. Seasonal to interannual prediction and the generation of GHG scenarios is clearly but one part of what needs to be developed. A broader set of tools than the numerical models of climate and related processes currently in use by researchers in these two communities is needed to facilitate the integration with the social system through support for generating appropriate probabilistic scenarios.

Upmanu Lall, Columbia University

Page 100, Chapter 9 – General Comment

It is important to dispel the myth that the natural carbon sinks discussed in this section of the strategic plan can effectively solve the problem of carbon management. Fossil carbon emissions of the next century are very likely to exceed all biomass carbon and could easily be four to five times larger than that. The uptake capacity of the naturally available carbon sinks would be overwhelmed.

Carbon Cycle research is important for carbon management. The central goal of such research, however, should not be an attempt to manipulate the cycle for increasing the available sinks, but to understand the dynamics of the system and the integrity and stability of these sinks. The storage capacity of these active natural sinks is too small to accept the fossil carbon that could easily be consumed over the course of the next century. Nevertheless, natural fluxes of carbon between these reservoirs are very large. Even though they tend to cancel over longer time scales, at any point in time they are far larger than the rate of anthropogenic injection. It is therefore important to understand how anthropogenic changes will affect the natural carbon cycle. Unintended feedbacks between for example warming and carbon emissions could have severe consequences. Rather than focusing on these issues, the illustrative research questions in this chapter seem to be focused on sequestering additional carbon, but they reflect a very limited idea of carbon management. On the scales of fossil fuel consumption, other options for carbon dioxide disposal must be considered, the natural carbon sinks will fall far short from solving these problems.

In this chapter the question is asked, “How will the Earth system respond to various options being considered for managing carbon in the environment?” Implicit is the assumption that the terrestrial carbon sink and the ocean carbon sink will play a big role in solving the problem of increased concentrations of atmospheric CO_2. But the simple observation that even pre-industrial Britain could deplete its biomass carbon storage, suggests that terrestrial carbon storage is not going to be a big player in the attempt of canceling out anthropogenic carbon emissions. Even the ocean sink falls short of what is required. It certainly will not respond fast enough to accept all the carbon that will need to be sequestered. If it were to take up all that carbon dioxide, the resulting changes in the carbonate ion concentration and pH would lead to unacceptable changes.

This section of the Strategic Plan explicitly asks for field studies, manipulative experiments, and model investigations. However, the draft leaves out all experimental investigations below the open field scale. In order to gain an understanding of the multi-scale complex interactions in ecosystems, experiments must be performed at any level possible. While it is possible to do some manipulations under field conditions to perform experiments and thus test hypotheses and models, a place like the Biosphere 2 Center, in Oracle, Arizona would be far better suited to do quantitative ecosystem scale experiments. It would help bridge the gap between laboratory experiments and growth chamber experiments on the one side, and open field observation and manipulation on the other side. The Strategic plan completely omits experiments focused on smaller scales. These are also necessary to begin a comprehensive approach to the problem of carbon cycle science.

Klaus Lackner, Columbia University

Page 101, Lines 32-33: Question 1: What are the magnitudes and distribution of North American carbon sources and sinks and what are the processes controlling their dynamics?

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In the draft of November 11, 2002, it is stated that “There is growing evidence of a current Northern Hemisphere terrestrial sink averaging 1.8 billion metric tons of carbon per year." This view has been controversial and is no longer accepted. Hence, it should be deleted. This estimate is based on an older publication by Fan, S., Gloor, M., Pacala, S., Sarmiento, J., Takahashi, T. and Tans, P. (1998) [A large terrestrial carbon sink in North America implied by atmospheric and oceanic carbon dioxide data and models. Science, 282, 442-446.]. This has been superceded by a recent work by Gurney, K. R., Law, R.., Denning, S. A., Rayner, P. J., Baker, D., Bousquest, P., Bruhwiler, L., Chen, Y-H., Ciais, P., Fan, S., Fung, I. Y., Gloor, M., Heinmann, M., Higuchi, K., John, J., Maki, T., Makyutov, S., Masarie, K., Peylin, P., Prather, M., Pak, B. C., Randerson, J., Sarmiento, J., Taguchi, S., Takahashi, T. and Yuen, C-W. (2002) [Toward robust regional estimates of CO2 sources and sinks using atmospheric transport models. Nature, 415, 626-629], which is more extensive and reliable than the earlier work. The results are briefly compared below. The difference is primarily a result of the data selection bias in the earlier work. While the results of Fan et al. (1998) indicate a predominant sink in the temperate North America, the new results show that Eurasia forests are the dominant sink and the temperate North America is about one half the Eurasian sink, nearly proportional to their respective areas. This makes more sense than an unexplainably high sink flux in North America.

	Gurney et al. (2002)	Fan et al. (1998)
Boreal North America	+0.26 Gt-C/yr	-0.2 Gt-C/yr
Temperate North America	-0.83	-1.4
Eurasia	-1.75	-0.2
Rest of Land	+0.95	+0.2

Taro Takahashi, Columbia University

Page 101, Line 35:
"Other studies suggest that elevated CO_2, nitrogen deposition …" should be changed to "… suggest that elevated <u>atmospheric</u> CO_2 <u>concentration</u> …..", if the author means plant growth enhancements due to increase in atmospheric CO_2 .
Taro Takahashi, Columbia University

Page 101 Lines 32, 37 (and else where):
When the term "terrestrial sink" is used in the context of CO_2 flux, it means the "net terrestrial sink flux". The author is advised to differentiate the "net " flux from the "gross" flux for clarification. Note that the "net sink" may be intensified either by an increased carbon fixation rate or by a reduced respiration rate. From atmospheric CO_2 measurements, we cannot tell which is the cause for changes in the net flux.
Taro Takahashi, Columbia University

Page 102, Line 12:
Since a large expanse of the Pacific and Atlantic Oceans affects the atmospheric CO_2 over the Northern Hemisphere, "<u>adjacent</u> ocean basins" sounds too restrictive. Replace it with "northern oceans".
Taro Takahashi, Columbia University

Page 104, Line 2:
Question 2: What are the magnitudes and distributions of ocean carbon sources and sinks on seasonal to centennial time scales, and which processes control their dynamics?

A carbon program in the Southern Ocean is introduced here abruptly without any context. It should be explained very briefly why the Southern Ocean should be investigated. For example, the following bridging sentences may be added: “The massive abyssal waters occupying more than 75% of in the major ocean basins originate in the Southern Ocean. Hence, the region represents a direct conduit for CO_2 exchange between the atmosphere and the deep ocean reservoir, which contains about 50 times as much CO_2 as the atmosphere. Its conditions affect the long term storage and cycle of CO_2 in the whole ocean system.

Taro Takahashi, Columbia University

Page 106, Line 31:
Question 4: What are the effects of past, present and future land use change and resource management practices on carbon sources and sinks?

It must be pointed out here that fundamental tools for investigating the below ground processes, especially those for carbon in deep root systems, should be developed in order to understand and enhance the long term storage of carbon in forests.

Taro Takahashi, Columbia University

Page 112, Chapter 10 – General Comment
This chapter is notable for its endorsement of experimental ecosystem science and the importance of this in gaining understanding of the linkages and feedbacks between ecosystems and global change. It is realistic in recognition that important linkages among physical chemical and biological components of ecosystems are exercised over a wide range of spatial and temporal scales in the biosphere. When outlining the needs for ecological experimental facilities it notes that major efforts might be directed at enhancing existing facilities.

The following comments are meant to strengthen the case for enhancing our capability in experimental climate change science at larger scales, approaching the ecosystem scale. I pick up the need for *“developing the capability to perform large-scale (over an acre) whole-ecosystem experiments that vary both CO_2 and climate”* (NAST Synthesis on Climate Impacts, Nov 2001).

The thin green veneer of terrestrial and aquatic ecosystems, together responsible for the transformation of planet Earth over billions of years, remain important engines of planetary sustainability. These elements of the biosphere are also among the most responsive components of the Earth system to climate change. Global climate change impacts, perhaps first manifest through increased frequency of extreme events, may be with us already (witness El-Niño 1957-8, 72-3, 82-3, 86-7, 91-2, 97-8, 2002-3), and there is now little doubt about measurable global change effects on ecosystem processes in the last half of the last century (Walther et al 2002; Nature 416, 389-95).

In reviewing the ecological effects of climate fluctuations, experts now recognize *“The need for proper experiments exploring the underlying causal mechanisms is clear”* (Stenseth et al 2002; Science 297, 1292-6). Distinguished climate modelers realize *“ The economic stakes are very high. To attain maximum credibility we will need all of the* *experimental approaches and their integration...”* (Tans and Wallace 1999; Tellus 51B, 526-71).

Bob May (1999; Phil Trans R Soc B 354, 1951-9) anticipated that *“our lack of detailed understanding of the changing balance of* CO_2 *on land, in the atmosphere and in the sea undercuts predictions about the effects of climate change, and could impede the clear implementation of the Kyoto proposals for reduction of emissions”*. These concerns were quickly confirmed as the United States and Australia declined to sign the Kyoto Protocol because policy decisions of this magnitude must be based on sound experimental evidence. The sound experimental evidence is not yet available.

We have seen that effective policy responses to previous global climate threats, such as expansion of the ozone hole, came about because of sound observation and clear experimental evidence (recognized by a Nobel Prize in chemistry), backed up by basic research in the private sector (DuPont) that delivered alternatives to the ozone depleting refrigerants then in use. Then as now, the science community accepts that the global change policy debate will be turned into action only when models and predictions about climate change are based on data from appropriately scaled and controlled experiments, and when this evidence is translated by the private sector into opportunities for innovation and mitigation.

Unfortunately, the synergies in biophysics and biochemistry that have so effectively served so many emergent disciplines in the past are not being explored in experimental climate change science or earth systems science (ESS). As enunciated by Harte (2002; Physics Today 55, 29-36):*“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”*. Harte bears witness to the *“dysfunctional consequences of this biomodal legacy”* and pleads the case for seeking a synthesis of Newtonian and Darwinian traditions in ESS. Foremost among his ingredients for synthesis 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 ESS models are derived from weather forecasting models and are designed to be predictive tools. 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. Although many of these climate models begin to rival the system they simulate in complexity, their predictive capacity has been advanced by improved representation of ecosystem processes (Sellars et al 1996; Science 271, 1402-1406; Berry et al 1997; in van Gardingen P, Foody G, Curran P, eds. *Scaling-up, from Cell to Landscape*, pp347-369, SEB Seminar Series 63,Cambridge University Press). Even so, IPCC 2001 reminds us *“The range of uptake rates projected by process-based models for any one scenario is, however, considerable, due to uncertainties about (especially) terrestrial ecosystem responses to high* CO_2 *concentrations, which have not been resolved experimentally...”* (IPCC 2001).

In summary, we face huge uncertainties and our understanding of climate change impacts on ecosystems at present is limited and unconvincing. Present understanding is largely based on observation and on (perhaps) inappropriate modeling. It is poorly supported by experimental insight at appropriate scales. By analogy with the response to another global threat, the HIV-AIDS pandemic, we can ask where we would be in controlling the epidemic today if we had left the response to epidemiologists alone. Just as the full arsenal of experimental bio-medical research has been mobilized to address the pandemic, so we need now to mobilize the whole arsenal of experimental capabilities in natural sciences and engineering in support of climate change science, from the molecule to the biosphere.

We have to build Harte’s bridge and cross Newtonian-Darwinian divide, and this bridge needs to be constructed on a sound experimental basis. Only by this means can we expect to reduce uncertainty, improve predictability, discover mitigation technologies, gain credibility with policy makers, and strengthen political will. If we as experimentalists, observers and modelers fail now to engage in this way, global change may well be inexorable, and irreversible over the next 2-5 human generations.
Barry Osmond, Biosphere 2 Center, Columbia University

Page 121, Chapter 11 – General Comment
Governments invest in global change research out of concern about the possible effects of climate change and other global environmental changes on human beings, societies, economies, and other things human beings value. Social science research can help to estimate the potential benefits, costs, and risks of global change and to assess the potential effects of strategies to adapt to or mitigate it. Such research should investigate a variety of outcome variables (e.g., economic, health) and should disaggregate these outcomes (e.g., by economic sectors, regions, social groups, stakeholders, etc.). These points constitute a key scientific imperative identified in Chapter 7 of the National Research Council *Pathways* report. The draft strategic plan sometimes omits these questions entirely or offers an unbalanced and insufficiently articulated research agenda. Chapter 11, for example, covers technological change, but fails to address institutional change. As another example, the social science perspective is not integrated well into the Scenario development section of Chapter 4.
Elke U. Weber, Columbia University

Page 122, Line 30-33:
"adaptive capacity in responding to the impacts"
means at least, certainly, altruistic behavior, caring, willingness to take risks for strangers. all of these are abetted by religious conviction, and here perhaps you might want to say that.
Robert Pollack, Columbia University

Page 123 Line 11-12:
"Researchers who need to model human actions in order to project future conditions and consequences often find the foundation for quantitative models lacking." Begs the question of whether this is an absence of evidence, or evidence of an absence. that is, perhaps the major driver of human action under large stress is precisely the non-predictable, non-rational capacity of self-sacrifice to emerge when needed. One need not

model the intrinsically unexpected, one need only note that it exists and sets a boundary to the certainty of any model at all.

Robert Pollack, Columbia University

Page 125 Line 22:
"how can society use improved information about climate change .. to adapt more effectively ...? please see the application on this (re 9/11, but applies as well to battery park under water) assembled by my colleague Andrea Villanti, who will send it to you directly if you get back to her by email, above.

Robert Pollack, Columbia University

Page 126 Line 17: ".. private, governmental and social decision making ..." an interesting use of "social" to represent anything neither governmental nor private. in a world of >> 1 billion Muslims and >> 1 billion Catholics, surely one may say "religious" here as well?

Robert Pollack, Columbia University

Page 127 Line 21: "illustrative research questions"
why not put in the intentional use of terror to disrupt water supplies?

Robert Pollack, Columbia University

Page 155, Chapter 14 – General Comment
International collaboration and cooperation on the SI prediction and application problem can be a key contributor to the international goals of the U.S. climate change science program.

An example is provided by the training institute on Climate Variability and Food Security mentioned by panel member H.Virgi (START) during the panel discussion on the International Chapter. This institute is being coordinated by IRI and is building capacity and developing research methodologies on the use of downscaled climate information for adapting agricultural strategies to enhance food security in developing countries. We emphasize the combined aspect of training and collaborative research and development in methodologies for using climate information. The IRI, by generally focusing on regions with strong predictability on seasonal to interannual timescales, is able to participate in methodological development for use of probabilistic climate information, that can provide general lessons for using climate information in other contexts, such as seasonal predictions and global change projections for the U.S. Thus developing such collaborations and research methodologies provides a learning opportunity for adaptation to climate information (p156, line 20), and this type of work could be referenced in this context.

The IRI has also led capacity building and research methodology development in the more specific climate science issues of downscaling of seasonal to interannual climate predictions. These activities blend regional climate science expertise with international perspectives (p156, line 24), and could be mentioned in this context. Downscaling global change scenarios for mid-latitude regions like North America will require accurate representation of the climate of tropical regions, because of the tropical-extratropical inter-connectivity of the climate system, encouraging investment in an international community of climate expertise.

It may be valuable to seek as wide input as possible from the stakeholder oversees international institutes at the earliest time. Developing countries may be particularly concerned with predicting droughts and floods on a year-to-year basis, and be more motivated to enhance data availability when it contributes to this immediate goal (p155, line23-24), while such data also can contribute to the global change goal. As mentioned in the chapter (p159, line 37-41), the IRI is hosted by the U.S. and is an institute that emphasizes cooperation with developing countries. This has led the institute to develop a number of partners on the international stage and this network (and associated experiences in capacity building and research methodology development) may prove to be of value to the international program.

Neil Ward, Shiv Someshwar , Caroln Mutter, IRI Columbia University

Page 149, Chapter 13: We firmly support the panelist comments (Breakout Session 24) by Janine Bloomfield that the climate science program's reporting and outreach component should build upon existing stakeholder relationships, established educational networks, existing media distribution networks and established public-private partnerships.

In Chapter 13, Reporting and Outreach, we concur with the introductory remarks (p. 2 of 8) that call for an interagency inventory of outreach activities and we recommend that this also include NGO activities in colleges and universities, utility, business and trade associations, environmental organizations and other NGOs, as well as state level outreach activities and resources. We concur with the intent to facilitate outreach without duplicating efforts (p. 3 of 8)

In Chapter 13, under heading of Local/Regional Governments etc., (p. 4 of 8) we do not understand the placement of the sentence at the end of the first paragraph in this section, "Researchers need to understand how uncertainty is used in decisionmaking so that uncertainties are effectively communicated."

We support the call for collaborative efforts in outreach and will follow up these general comments with more specific recommendations for implementing a cost-effective outreach program, based on our own experience in the areas of acid rain and climate change.

Thank you very much for the opportunity to comment and for making this climate science program initiative and draft strategy so accessible to the public.

Center for Environmental Information, Thorndike

Page 155, Chapter 14: The Center for International Environmental Law (CIEL) is a public interest, not-for-profit environmental law firm founded in 1989 to strengthen international and comparative environmental law and policy around the world. CIEL’s Climate Change Program supports the creation of a coherent international regime to protect the climate system, consistent with Article 2 of the UNFCCC

How Can the CCSP Benefit CIEL's Climate Change Program?

The plan for international collaboration would be greatly improved and CIEL'S program would benefit by strengthening the CCSP in three areas:

- Clarify US support for IPCC
- Identify and assess international opportunities for mitigation
- Evaluate costs and benefits of climate impacts, mitigation and adaptation

Clarify Support for IPCC

To ensure a coherent international scientific process, CCSP should clarify its support for the IPCC. CCSP must not become an alternative to the IPCC, or worse, a competitor. Emphasize refining rather than revising the work of the IPCC, for example, by focusing on regional impacts and reducing uncertainties

Identify and assess opportunities for mitigation

The CCSP should put more emphasis on identifying and assessing international opportunities for mitigation. This information should be readily available to policy makers, who may need to act quickly in the future to avert severe impacts from climate change. Identify "no regrets" actions that could be taken immediately

Identify the *real* costs and benefits of averting climate change

Identify and compare costs of climate change impacts, adaptation and mitigation at global, regional and local levels. This information is essential to resolution of the policy debate, which CIEL believes is skewed in favor of inaction. Policy makers have tended to focus on the costs of mitigation and are insufficiently informed about the benefits. Indeed, many credible studies, some by US government agencies, suggest that costs of mitigation may be relatively moderate, in some cases perhaps even negative, whereas the costs of adapting to the impacts of climate change may be much higher than most decision makers realize.

Of course, for some communities, impacts cannot be quantified in mere dollars. For example, the inhabitants of the Arctic have at risk their culture and way of life. The CCSP must find a way to reflect non-economic as well as economic losses that may result from climate change.

Center for Int'l Environmental Law - Goldberg