Planning Meeting for NRC Decadal Study: Earth Science and Applications from Space
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Comments from: Judith Curry, Georgia Tech, curryja@eas.gatech.edu

1. **There have been many documents in recent years that contain elements of a vision for the Earth Sciences community, e.g. the U.S. Climate Change Science Program (http://www.climatescience.gov/), the recent NASA document *Earth Science Vision 2030* (http://neptune.gsfc.nasa.gov/vision), and the ongoing Earth Observations Summit (http://www.earthobservationsummit.gov/).**
    a. **Do you think there is a coherent vision for the climate and the Earth sciences in these or other documents?**
    b. **What do you believe have been fundamental impediments to an effective Earth science strategy in this country?**
    c. **What guiding principles should steer this study?**
    d. **Is establishing such a vision a desirable and achievable goal of the decadal study?**
    e. **Please write down in a few sentences your vision of the Earth sciences for the next 25 years.**

No, I do not think that a coherent, integrated vision for climate and the Earth sciences exists in any of these documents. The current documents mostly reflect an accumulation of discipline-based strategies and confuse the needs for climate change monitoring, initialization and evaluation of predictive models, scientific process studies, and decision support. The result is a long list of variables that "must" be observed at high space/time resolutions that is infeasibly expensive, with no prioritization. Most of these documents seem driven implicitly by disciplinary scientific interest in scientific process studies, rather than by socioeconomic and policy needs related to climate monitoring, predictive models, and decision support.

Consider the following example of the fruits of confusing these needs. Atmospheric temperature and its change has been highlighted as one of the 21 synthesis and assessment reports mandated by the CCSP. This emphasis has arisen because of the large differences among the different estimates of trends in atmospheric temperature profiles. It is stated that these trends are needed both to document the change and to evaluate the climate model simulations. To address this issue, there are numerous proposals on the table, including a global network of UAVs to drop reference temperature sondes. Consider the following alternate strategy that accounts for the different needs related to atmospheric temperature profiles for monitoring, predictive models, scientific process studies, and decision support. Decision support for direct socioeconomic applications (end users in resource management, industry, etc) doesn't really relate directly much to atmospheric temperatures but rather to surface temperature. Assimilation of atmospheric temperature information into predictive models (primarily a weather forecasting issue) is fairly adequately addressed by surface radiosondes and satellite observations (especially once NPOESS is launched). In any event, the optimal data assimilation for this purpose appears to involve the actual microwave and infrared spectral radiances, rather than the retrieved temperatures profiles. For evaluation of climate models and climate monitoring (a key issue for policy makers), it is essential to have reliable measurements that are absolutely calibrated and truly global; a strategy such as that proposed by Jim Anderson (GPS occultation and spectral measurements that do not explicitly retrieve temperature profiles) is likely to be far more effective at documenting climate change (and of course atmospheric temperatures contribute to such measurements implicitly).

The needs for scientific process studies can be met at a small number of locations using surface based and airborne measurements. I can envision a strategy that meets our scientific, operational, and societal needs that does not emphasize explicit measurements of atmospheric temperature profiles. The current strategy of confusing all of these different needs and focusing on variables that are really driven by scientists' research interests in process studies, results in a database that doesn't meet any of the needs very well and proposed strategies that are expensive and arguably unnecessary.

In my opinion, impediments to an effective Earth science strategy arise from the following:

a) "Easy money" for this field for the past 4 decades because of the promise of socioeconomic benefits from forecasts and observations. While we all complain that there is not sufficient money in our field, there is an enormous amount of funds in our field relative to other related fields that are driven primarily by scientific inquiry (e.g. geophysics). It is not clear that these socioeconomic benefits will be realized at anywhere near their potential level if we continue in the 'business as usual mode" and are guided by the current strategic plans. The current strategic plans may be well suited for making scientific progress, but not necessarily suitable to the agencies' mission related to providing socioeconomic benefits. If our only product is to be "good science" and journal articles, we deserve a much smaller budget commensurate with other scientific fields that do not have strong socioeconomic benefits, that could probably be solely funded by NSF. Mission agencies need to deliver something more than good science. The CCSP takes a step in the right direction in chapter 11, but there is no explicit strategy for how to accomplish these goals.

b) Disciplinary focus in the strategic planning results in groups of scientists and program managers working to defend the funding base for their subfield, primarily to support their interest in disciplinary scientific research and process studies. For the mission agencies, this impairs a coherent and integrative vision and makes prioritization virtually impossible.

c) Lack of viable programs and financial resources to support research and applications in socioeconomic decision support. This absence hampers strategic planning and prioritization of resources by the mission agencies.

The goals of the decadal study should be to:

- provide a defense of NASA and NOAA expenditures in terms that are clearly understandable by congress and OMB
- provide a framework for strategic planning for NOAA and NASA that includes a strong element of increasing and documenting socioeconomic benefits of the observations, predictions, and science
- torque a component of the science in the direction of multidisciplinary studies of decision support and applications

My vision statement for the Earth sciences for the next 25 years, that is appropriate for NOAA and NASA and independent of the NSF-style mission to support basic research in Earth science:

> *To realize the substantial investment in Earth science research over the past four decades by improving the use of observations, forecasts, and scientific research to support decision making by policy makers, resource managers, industry, and the citizenry. In turn, increasing sophistication and needs in decision making will motivate the need for future scientific research, observing systems, and forecast products.*

2. **What are two or three of the greatest weaknesses in the present global observing system? How could they be addressed?**

a) The lack of true monitoring of climate change that is global and absolutely calibrated. This is essential for policy making and for evaluating climate models. The proposal put forward for GPS occultation and spectrometer measurements is a good starting point for considering this issue

b) The lack of a coherent strategy for use of suborbital platforms (especially UAVs). UAVs have the potential to save considerable $$ that wouldn't need to be spent on satellites for certain applications. Consider one example: the most important application of the IceSat satellite is to document portions of the ice sheets that are changing rapidly. Deployment of relatively inexpensive low altitude UAVs to regions of most rapid ice sheet change could provide continuous high-resolution observations in the specific regions of interest at a cost that would be several orders of magnitude cheaper than a satellite. Additionally, UAVs and their targeted, high-resolution observations could be deployed to regions of most impact for decision support (e.g. for hazards, resource management). Hence UAVs can both save $$ and provide better information for some decision support applications.

c) The lack of support for educational programs that integrate science and engineering to provide the next generation of scientists/engineers capable of advancing the observing technologies and who understand the interdisciplinary problems of earth science and decision support so that rational prioritizations of resources can be made.

3. **Provide a broad perspective on what a new integrated global observing system should contain and how it could be effectively used to meet societal needs.**

Explicit consideration needs to be given to the interplay between prediction models, surface and suborbital observations, and space observations from NASA and NOAA and from the private sector. This situation is not in a steady state: explicit plans for NASA and NOAA satellites should account for developments and possible cuts in surface and suborbital observations, improvements to forecast models, and increasing capabilities of the private sector to provide satellite observations. Since most of this is government funded, some coherent government strategy is needed not just for the NOAA and NASA satellites, but that also integrates with the surface and suborbital observing systems and the predictive models and their projected changes.

Increase the efforts at NASA and NOAA in decision support research and its application. This kind of research and application is relatively cheap (compared to paying for supercomputers and satellites) and has the potential to provide substantial socioeconomic benefits and thus provide a much better "bang per buck" for the government investment in NASA and NOAA. Both agencies have anecdotes about how their models and data are being used for decision support, but I suspect that these applications are less than 1% of the potential applications. The small applications programs at NASA and NOAA are on the funding block. There seems to be a sense among Earth scientists and program managers that it is sufficient to provide data and forecast products in a user friendly way in order to support applications of the products. Social scientists conduct research of relevance to decision and support, and are irritated when decision makers don't use their research. It is becoming increasingly apparent that the gap between Earth science and applications is intellectually as great in scope and challenge as Earth science itself. The shambles of U.S. climate change policy (e.g. energy emissions control) certainly reflects this gap: more and better climate model predictions don't automatically translate into sensible policy or into any policy at all. I first became aware of this gap between research and applications as it relates to resource managers and industry in discussions with meteorological and environmental consultants, who lament the very low user uptake of decision making support tools using data and forecast information. At the CRC briefing by the CCSP last spring, Richard Clarke of Harvard eloquently described this gap as the "valley of death." In my own informal investigations on the topic, I have found the following barriers to using forecast and data products in decision making: insufficient information and lack of forecast precision, uncertainty over forecast accuracy, competing or conflicting forecasts, fear of using bad information and risk of innovation, difficulty in relating the forecast information to the decision, difficulty in assessing forecast value, lack of expertise to use the forecasts and to implement decisions based on them, and cost of validating and implementing the forecasts. Until these issues are understood and addressed, the best data and forecast models combined with optimal decision support tools will not have the desired impact. Effective use of the data and forecast products requires interdisciplinary research teams involving Earth scientists, social scientists, and decision makers. I think that the greatest areas of untapped applications are in industry and finance. The profitability of virtually any company could be improved using environmental data and forecast products. Many industries face environmental threats from multiple, cumulative, and interactive stresses. The financial sector has enormous vulnerability to weather and climate, as well as tremendous opportunities for improvements using forecast and data products; this sector could become the greatest customer for climate information. I have found the following references in this area to be of particular interest:

- Institutional Investors Group on Climate Change http://www.iigcc.org/
- UNEP-FI http://www.unepfi.net/: Climate Change and the Financial Industry;

Nobody is funding this kind of research (the small applications programs at NASA and NOAA are on the funding block), and in my opinion this is a very important element of NOAA and NASA's mission and is necessary for justification of continued support of these agencies at current or expanded levels.

Implementation of an assessment plan is needed that is directly tied to a strategic plan and includes performance metrics, targeted at Congress and OMB. We need to make the case for continued and enhanced investments in NOAA and NASA in terms that they understand. Each agency and program within it presumably provides some sort of assessment of its productivity in terms of number of satellites launched, field programs, bytes of data, journal publications, etc., both to the individual agency advisory boards and in defending the agencies' budget to congress, OMB, etc. But it would be far more effective in defending the overall investment in NASA and NOAA to have an integrated annual assessment report that might include metrics such as those related to:

- quantifying, understanding and reducing uncertainty in observations of individual global variables (both with validation studies and improved sensors);
- quantifying, understanding and reducing uncertainty in global forecast models, focusing on decision-relevant variables such as surface temperature and precipitation (e.g. model evaluation against data, model sensitivity studies, issues related to ensemble forecasts; new model parameterizations and their evaluation)
- documenting applications using the data and forecast products (at least those projects funded by the agencies; it would also be worthwhile to fund some studies to investigate this more broadly) and quantify their socioeconomic benefits (e.g. dollars saved, improvements to local environmental quality, lives saved)

This kind of assessment would help document the "bang per buck" and justify further investments in NASA and NOAA. This would help NOAA and NASA strategic planning and prioritization of resources. It would also help improve the quality of our science, which too often ignores a rigorous evaluation of uncertainty, which in other fields would be rejected as "sloppy" science.

In my opinion, scientists funded by mission agencies should have a responsibility beyond publishing journal articles, to contribute to efforts like the proposed assessment.

4. **Given that NASA and NOAA have identified a need for a decadal study of the type, please provide your suggestions on a process that would be inclusive, transparent, and lead to a vision and specific plan, with priorities, that would be successful and have broad community support. What are the challenges to performing this study to the satisfaction and benefit of the community as a whole?**

With reference to the CCSP process, which I think is our most recent analogous reference point, I think that the strong points of that process are:
a) the web site
b) frequent requests for community input on the web site
c) town meetings

In my opinion the weak points of this process have been:
a) the CCSP was written mostly by program managers, and did not really include the scientists and stakeholders. In my opinion this procedure did not result in the best possible science plan, does not promote "buy in" from the academic scientists and stakeholders, and leaves the document open to charges that it is politically rather than scientifically driven. Once the document is written, providing academics and stakeholders the opportunity for input does not achieve the same goals and results as having them involved in developing the strategy and writing the initial text. Also such a process leaves the plan open to criticism that it is driven by politics rather than by science and the need to provide support for policy decisions.
b) the CCSI apparently does not have an adequate email list for notifying people of deadlines for input on the web based documents, of the meetings, etc. For example, I am not on any such email list, although I am a member of the NRC Climate Research Committee.

The other critical issue is the leadership for the study. People are needed that have substantial stature in the community, good ideas, and management and leadership skills. The disciplinary orientation and balance of the leadership is in my opinion fairly irrelevant; I think that disciplinary balance is generally overly emphasized in selecting these groups. What is needed is people with substantial breadth of perspective that can strategize beyond promoting their personal research interests (otherwise we end up with an amalgam of disciplinary lists of needed observations; we already have those and need to move beyond this).

For the study, I would start from the assumption that the strategic planning for the disciplines in existing documents is adequately complete and doesn't omit anything critical. What is needed is a cross-cutting strategy that is clearly salable in terms of socioeconomic benefits. The TRMM satellite is not going to be rescued to keep scientific researchers entertained; rather it will presumably be rescued because of its importance to hurricane forecasting and warnings.

Study/task groups that cut across disciplines should be formed, including groups such as:

i) *Data assimilation*. Operational forecast modelers to strategize about the optimal variables for data assimilation, including the entire range of NCEP models. This community tends to use what is available (certainly not everything that is available); it is not clear that much new strategic thinking has been done in this area.

ii) *Forecast model evaluation and improvement*. Forecast modelers need to strategize on appropriate ways to evaluate their models to document their strengths and weaknesses and to point in the appropriate directions for model improvement. Globally-averaged trends and "blob" analyses (whereby averaged global maps of simulated vs observed fields are examined visually) are useful only at the zeroth order. Almost everybody says their data set is going to help the global modelers; this rarely comes to pass in a meaningful way. In my opinion, the use of this argument to justify satellite missions should be rejected until the community develops a much better strategy for using observations to evaluate and improve the global models. Rather than providing a list of variables, it is more important that innovative analysis methods be proposed in conjunction with the strengths of the observing systems (which may not be to relate the model prognostic variable to the same retrieved variable, but rather to calculate variables that the satellites actually measure, such as refractive index, radiance, etc.) and using appropriate statistical methods.

iii) *Observing system.* Observationalists need to define current uncertainties in the individual variables on the extensive "list", and the prospects and resources needed to make improvements over the next decade. In other words, don't just provide your list of variables that are "essential" and the scientific rationale; provide an estimate of uncertainty for each variables related to the measurement technology and sampling of the current observing system, along with a decadal plan for improving the observation. Prioritization can then proceed in a framework where the fundamental scientific priorities are set relative to needs to climate monitoring, forecast model initialization and evaluation, and decision support, along with information on how well we are currently doing with the present observing system and costs and technological readiness for making improvements for specific variables.

iii) *Decision support.* Resource managers, industry, policy makers, and other decision makers to address ways to overcome the "valley of death". It is very challenging to identify such people with the appropriate breadth of perspective and willingness to devote time to such efforts. But a very useful community to engage in this endeavor is the weather/environmental consulting community, who are actively trying to use data and forecast products for various applications. This community is easier to engage for these purposes and individuals from these consulting groups typically deal with a wide range of applications. The key issue here is to articulate a strategy for NOAA and NASA to support and encourage these applications in useful and effective ways, while not stepping on the toes of the meteorological/environmental consulting community. Another group to entrain more is program managers from the other mission agencies such as EPA, DOE, USGS, DOT, etc. I realize that this was done for CCSP; this interaction should be expanded and enhanced to identify how space observations can support their missions.