

Prospectus for Synthesis and Assessment Product 1.1
***Temperature Trends in the Lower Atmosphere: Steps for Understanding
and Reconciling Differences***

Public Review Comments
July 7 – August 12, 2004

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General Comments

William Fang/Eric Holdsworth

EEI made rather extensive comments on the general guidelines, which we understand from the instructions accompanying the *Federal Register* notice (69 *Fed. Reg.* 42043) “are being revised,” taking into consideration the comments received” from EEI and others and notes that this prospectus does not reflect any of those comments, such as our comments on “selection of authors” and on the issues of “confidence levels” and probabilities. However, we also understand that when a “final version of the guidelines”

is available, this prospectus “will be revised if necessary to conform fully to the final version of the guidelines.” While that is certainly good, it is difficult to understand at this juncture how and to what extent those revisions will change our understanding of this Prospectus, particularly the phase 1 and 2 schedule, which is already too tight and inconsistent with the Strategic Plan, which affords up to “2 years” for completion. This schedule affords a little more than one year for completion, with a large segment of that time (Jan. 05-Nov. 05) devoted to review and clearance.

While it is important to get the work on the products underway, it is not in the public interest to rush them, particularly in light of the unfinished nature of the guidelines. According to Phase 1 of the “Draft Prospectus,” it is to be approved this month. Yet it is unclear when the guidelines will be revised and finalized. No date is given in the instructions. Most importantly, the Instructions do not indicate how and when the final version of the Prospectus is to be made available to the public with these revisions.

Wolf Grossmann

This is not my area of expertise. I feel quite comfortable at linking climate and society, in particular societal and economic change, and I have written papers and that subject, build dynamic integrated models on it and feel quite comfortable with this subject area as the workshop from Dec. 2002 came out with the conclusion that this is a subject area of outstanding importance for the future of getting things implemented. So, I hope I will hear initiatives also from that subject area and are very much looking forward to that!

Charles Keller

First General: Overall publication setup. Perhaps there's no other way to change it, but this format sounds at least somewhat like a way to minimize the UAH people's (John Christy and Roy Spencer) results of small warming trend.

The key element in this format seems to be to establish that uncertainties in observational measurements are larger than UAH has claimed in print. Once that is established, the small warming result's agreement with radiosonde data can be called into question.

If true, is this really the right way to go? Might not a better approach be to attempt to show by an independent evaluation just how well UAH and radiosonde results support each other as compared with RSS results?

Haroon Kheshgi

First General:

Using atmospheric records of temperature to improve our understanding of climate change presents an important component of climate change research. In particular, understanding what is the deficiency in our understanding that is the cause of the apparent discrepancy between tropospheric and surface temperature records is a key question to be resolved. The IPCC TAR SPM noted that "these differences are not fully resolved." While this prospectus begins to address this topic, it has a number of

weaknesses that will prevent it from fully assessing the topic of the apparent discrepancies. These weaknesses are elaborated in the following comments:

1. The focus of the prospectus on temperature "trends", as in the title of the prospectus, is misplaced. Trends are one metric of temperature records that has been popular for communication of results, however, it has obscured the real issues. Keys to understanding any differences in records might be expected from the spatial and temporal patterns of the differences which are lost in trends. The full estimation of temperature records must, therefore, consider the actual detailed records and estimate the correlation of errors if such an assessment is to be used to validate or invalidate models of climate change.

a. How good a time series may fit a linear trend has little to do with the uncertainty of the time series data. This led to the uninformative, and potentially misleading, summary statement in the IPCC TAR SPM on the uncertainty of trends.

b. There is little reason to expect the main features of the records, such as the anomaly from the 1998 ENSO, to fit a linear trend. A trend line is known to be a very poor model for the data.

c. Specific features of climate records are recognized to be the focus of discrepancies between climate models and records. The IPCC TAR SPM notes that "the difference occurs primarily over the tropical and sub-tropical regions." Aggregation of data into trends loses key information.

2. The separation of information and expertise in the 6 questions will prevent the assessment of accuracy of temperature records.

a. There has been an inability to validate, and an apparent unwillingness to invalidate, climate models. The same seems to be the case for temperature records. There are now multiple records for both surface and tropospheric temperature. To understand the accuracy of temperature records will require consideration of structural uncertainty, and the use of validation tests. In the prospectus it is unclear how structural uncertainty, and the validation of data sets and models, may be assessed.

b. Full use of radiosonde and MSU data should be applied to generate a sound combined record of tropospheric temperature and assess its accuracy. This could require specific consideration of the strengths and weaknesses of each type of data including the variety of corrections applied to the raw data. In the current breakdown of questions there appears to be no place for this task and therefore no sound means to assess accuracy. For example question 2 does not seem to consider the combined accuracy of radiosonde and MSU systems, only the accuracy of separate systems. The combination of these systems should allow a calibration for corrections to the detailed records (not trends or other gross aggregations) and so these systems should not be considered as disjoint.

3. The time allotted for the assessment product is insufficient to carry out the intended task. As such, the assessment product may overly rely on the opinions of the chosen experts, and not form an objective assessment of this topic.

Michael MacCracken

The decision mentioned on page 4, lines 23-24 to exclude data set developers from evaluating the reliability of their products may seem appropriate in a legal sense, but in practice the developers often are the ones most aware of the strengths and weaknesses of the products that they produce. It seems to me that, to most effectively advance the science, the data set developers should be part of the team that does the evaluations. While they should not be the majority of the members, it is vital that they come to accept the evaluations, and this seems likely to work most effectively if they are considered part of the team to do this and ultimately can come to accept the evaluations.

2) The time schedule for the assessment as delineated on page 5, lines 1-25, seems to me to be turned on its head. As I read it, the assessment panel is asked to prepare its report over a period of 4 months, and then of order 9 months are allowed for various reviews. The issue being addressed by this assessment is one that has been the subject of efforts over the past 10-15 years and remains unresolved. This prospectus proposes to bring together as authors the leading experts on the subject, and it seems to me really very important to allow enough time for their intense interaction, through preparation of an initial expert draft, responses, redrafts, challenges, revisions, etc. I have been through similar assessment activities, and the richest interactions and most progress come during the time when the various authors and some independent experts are engaged in synthesizing, challenging, revising, reworking, etc. Unless enough time is allowed, all that will emerge will be the sterile statements of existing positions and no real progress on this important topic will have been achieved. Enough time has to be allowed for the authors to interact, and I would suggest that the time required is more than is indicated in this schedule.

3) The involvement of the NSTC (or any agency) in giving final clearance to this report seems to be potentially suspect. If this report is to truly be an official agency or interagency position paper, then the sustained use of an author team that is not composed completely of federal employees would seem to legally require that the authors be assembled as a federal advisory committee under FACA, a step that would require open, pre-announced meetings. As a more flexible alternative, the assessment panel could be organized under the auspices of a university—but, to ensure the academic freedom of the university community, this would require elimination of the requirement for NSTC clearance.

4) The prospectus seems to be missing a commitment to the release, simultaneous with release of the report, of a compendium of all of the comments made on the report (including identification of the commenter) as part of all of the review processes (i.e., NRC, expert, public, and NSTC/agency reviews) and responses to them (either that the suggested revision has been made, or if not, why not). This is essential if the process is to have the necessary credibility, particularly given that the NSTC is apparently given final review authority and has, in the past, effected changes in various reports (e.g., the CCSP Plan) without providing any enumeration of the comments. And, in fact, the promised set of comments and responses on the CCSP Plan has yet, to my knowledge, been released—and this is a year after the plan was completed. For an Administration supposedly

committed to openness and credibility in the scientific process, this is a serious shortcoming.

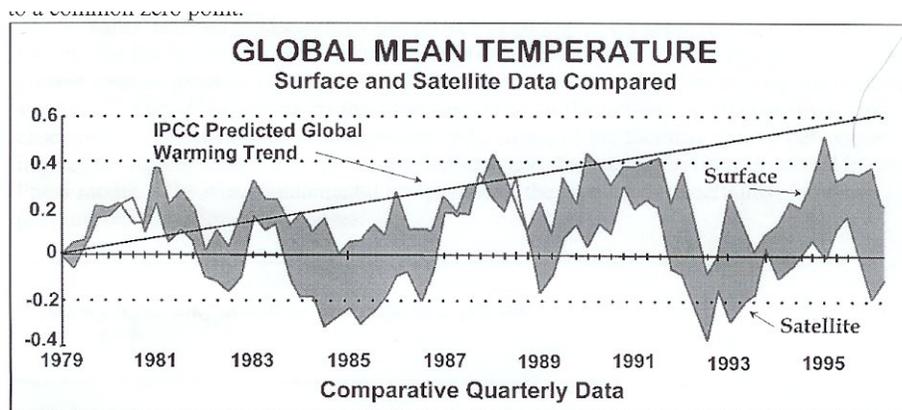
Jim Meyer

1)The mantle of Earth is very hot and a lot of energy is transferred from the mantle to the surface through the oceans of the Earth. The energy from the mantle is far more energy than all the energy used by the biosphere which includes all man made sources. The mantle energy is almost totally unknown except for deepsea vents that discharge hot water and hot springs found everywhere on Earth. The energy flux of the mantle is not even recognized as an energy source in current modeling even though it moves more energy than the biosphere processes. This is a detail that should be researched and factored into any realistic model since the mantle flux is a vital contributing source of energy to all processes that occur on the surface of Earth.

2)A system can be produced to use solar energy for power to transform carbon dioxide gas into a hydrocarbon fuel using water as the hydrogen donor. The solar cell need only generate a few watts of power per square meter on a large scale solar farm to make a system of this type very effective in reducing carbon dioxide gas in the atmosphere and reducing fossil fuel use because the product is a kind high grade crude oil. This system can be manufactured using currently available cells that transform solar energy to electricity and cells to reduce water to hydrogen and oxygen. The hydrogen then is used to separate carbon dioxide into liquid hydrocarbon and oxygen. The overall result is a reduction in solar energy heating the Earth and less carbon dioxide in the atmosphere. On a large scale the system will produce about three billion kilojoules of stored hydrocarbon per acre per year valued currently at ~\$15,000 which can be harvested on land where nothing of value can grow. And since a lot of the land on Earth is not usable for farming this system will add a new factor to crop production.

Asmunn Moene

Global mean temperature changes are unpredictable



The figure illustrates the global mean temperature changes from 1979 measured at the surface and in the lower troposphere by satellite (NASA). To compare the trends directly they are referred to a common zero point. The prognoses of the international panel of

climate change, IPCC, show a temperature increase of 1,4-4,5⁰ C in 100 years caused by a doubling of the CO₂ content in the atmosphere. In the figure a trend of 0,3⁰ pr.decade is given.

There is no significant relationship between the IPCC predicted trend and the two other curves which are generated by random changes. The random updating is of fundamental importance. Such systems are called autoregressive and they are nondeterministic i.e. they are unpredictable. Further the curves do not quite fulfil the requirement necessary in order to be an independent Gaussian process, which one must assume if one is to use a model for statistical forecasting.

The figure demonstrates another important fact: An increase of the CO₂ - content in the atmosphere should create a greater warming trend in the lower troposphere i.e. a greenhouse effect. It is evident that this is actually not the case. The warming trend is greater at the surface than in the lower troposphere.

According to the models introduced by IPCC the position of the two curves should be interchanged. The tropospheric warming trend is supposed to be 1,5 to 2,0 times greater than at the surface if the greenhouse gases are the cause.

It also appears from the last report of IPCC, (page 106), that the largest difference between surface and satellite measurements is found over the tropical and subtropical oceans where the most important accumulation of solar heat takes place on the earth.

The dominant warming trend at the surface therefore has to be caused by the world oceans covering above 70% of the surface of the earth.

William O'Keefe

The CCSP has chosen an appropriate set of technical questions to address in its first Synthesis and Assessment Product. The authors chosen to address these questions are well qualified and represent the spectrum of scientific opinion on this topic.

The credibility of this synthesis and assessment product would be greatly enhanced if an eminent scientist not directly involved in the issues being addressed was chosen as convening lead author for the summary question. There is no question that Dr. Wigley is a highly qualified climate change scientist, and we do not question his scientific integrity. However he, and any other similarly qualified climate change scientist, will have formed views on the issues under discussion in this product long before drafting of the summary begins. It would be very difficult for any scientist deeply involved in the issues to take an entirely fresh view of the information compiled in the synthesis and assessment.

The critical goal of CCSP synthesis and assessment products is to analyze the available information on climate change science issues and present it in a fashion that responds to the questions raised by policymakers. A fresh, neutral view of the underlying information is critical, and can provide insights that may have escaped those who directly involved in the collection and analysis of the data. An example of the value of a fresh

view is the role played by Freeman Dyson in the investigation of the Shuttle Challenger disaster.

An eminent scientist with a background in analyzing and explaining experimental data should be chosen as the convening lead author for the summary question. Dr. Wigley and the convening lead authors of the six underlying questions should compose the writing team for the summary question. They bring the expertise needed to ensure that all information and points-of-view are considered in responding to the summary question.

Some might argue that the CCSP review process ensures that fresh points-of-view are included in the synthesis and assessment product. While the review process should ensure that all available information is considered, it is not an interactive process. Reviewers submit their comments, which are considered by the author teams, but it is a static, one-off, process. The dynamic interaction that leads to the development of new analyses is missing. Having a distinguished scientist from outside the climate change science community challenging the conventional wisdom on surface and atmospheric temperature data and modeling in an interactive debate with the scientists most responsible for generating the underlying information offers an opportunity to generate new insights and approaches that should not be missed.

Ellis Remsberg

Several months ago an important paper appeared in the May 6, 2004 issue of the journal Nature (Fu, Johanson, Warren, and Seidel, pp. 55-58). In my opinion their study largely explains the relatively long-standing discrepancy about temperature trends in the lower troposphere as obtained from the MSU satellite instruments versus those from surface-based temperature records. My expertise in the area of atmospheric remote sensing by atmospheric emission techniques from satellite platforms leads me to see how it is easily possible to have measurements from a vertical weighting function that peaks in the lower stratosphere to be "contaminating" the record of trends from a more primary vertical weighting function that is centered in the lower troposphere.

In my opinion the findings of these authors should be hailed as definitive for the settling of this so-called "controversy", and the climate research community should be moving on to other issues, such as how to ensure that a good quality temperature monitoring record be maintained with future global, satellite observing systems.

John Stone

This is excellent. The choice of questions should probe our physical understanding of the system we are observing, the detection of changes from natural variability, and the attribution based on our physical understanding. The choice of the author team is similarly excellent. You have been able to engage some first class scientists. I am encouraged that you have included some non-US scientists. I trust that in the future you will look to Canada to assist you in the production of other assessments. It will be important that all data sets and analyses are available to the scientific community who wish to review the conclusions of this assessment - as is the practice with the IPCC. I look forward to seeing the draft in December.

Specific Comments

Page 1, Line s 12-13: It should not be implied that different techniques giving different results is necessarily incorrect or wrong. Often the techniques for making measurements are measuring different quantities (directly or indirectly) intended to be representative of different volumes of the atmosphere and for different time samplings. The problem is not so much that there are differences, but that all of the differences have yet to be fully resolved, confirmed, and/or reconciled.

Michael MacCracken

Page 3, Line 46: Insert the word “potential” in front of the phrase “human impact.” Whether human activities have significantly impacted on past climate and the degree to which they might impact on future climate are questions still being actively debated by the scientific community. This debate should be reflected in the phrasing of the summary question.

William F. O’Keefe

Page 4, lines 13-30: Section 6 is titled “Proposed Approach for Evaluation and Communication of Uncertainty and Confidence Levels, Where Applicable.” It states (lines 16-19):

When presenting results addressing uncertainties and confidence levels in our statements regarding the temperature trends, we note that increased understanding of the complexities of the vertical temperature variability can lead to increased uncertainties regarding long-term behavior patterns.

The draft prospectus further indicates that “uncertainties” will be communicated quantitatively “in many instances,” but “it is clear that” mathematical estimates “do not reflect the full range of uncertainty.” Thus, the “intent is to follow the protocol developed in the IPCC (2001) assessment and any updates provided by IPCC” (lines 29-30).

EEI, in its comments on the general guidelines, raised serious concerns about the use of the term “confidence levels” (see Specific comments, Page 2, Lines 22-25). Since then, it has come to our attention that the Intergovernmental Panel on Climate Change (IPCC), in its cross-cutting themes, indicated that the issue of probabilities needs further examination. Therefore, we call into question the proposal for addressing “confidence levels” in this section 6 by following the “protocol developed” by the IPCC in its third assessment and “any updates provided by IPCC.” Those “updates” could take many months before they are approved by the IPCC. Moreover, the CCSP should not adopt them without an opportunity for further public input.

William Fang/Eric Holdsworth

Page 4,line 23 It may be important to include developers opinions especially if evaluators come up with much different assessment of reliability of "products". This to avoid, where possible, developers rejecting outright evaluators' assessments as being uninformed.

Charles F. Keller

Page 5, line 16 this rather tight time constraint assumes that data reliability "evaluators" are already able to make an informed estimate of the uncertainties since usually there is no such detailed evaluation in the refereed literature excepting that done by the "developers". Is this true?

Charles F. Keller

Page 6 Line 3-4 ff Since Q. #1 deals with vertical variability of temperature in the troposphere, one might expect to see at least one lead author who is noted for studying this (as evidenced by their cited publications). As excellent as the listed authors are, none seems to have concentrated on this aspect of atmospheric physics. Interestingly one such expert, Steve Sherwood appears below as a lead author for question 2. Might he not be more important as a participant in question 1?

Charles F. Keller

Page 10, Line 5--Question 4 is a very awkward construction. It is not at all clear to the reader what this question is getting at or what characterization of uncertainties is being questioned. What's going on here? Are you saying that previously published estimates of uncertainties are wrong and therefore are misleading the discussion of whether there are indeed significant vertical differences in temperature trends?

How about a different wording such as:

"How have estimates of observational and methodological uncertainties limited our understanding of previously reported vertical differences in temperature trends?"

Charles F. Keller

Page 14, Line 7: Insert the word "potential" in front of the phrase "human impact." Whether human activities have significantly impacted on past climate and the degree to which they might impact on future climate are questions still being actively debated by the scientific community. This debate should be reflected in the phrasing of the summary question.

William F. O'Keefe