

Global-Change Scenarios: their Development and Use US CCSP Synthesis and Assessment Product 2.1b

Comments by Prof. Frans Berkhout, Free University of Amsterdam

Author team responses in italics:

1. Integration: I believe that the 3 main chapters could be better integrated: the scenarios reviewed in chapter 3 could more consistently be classified according to the framework developed in chapter 2; and the reviews in chapter 3 could be organized more systematically according to the issues identified in chapter 4. It may be worth considering a slightly rearranged order of the chapters: 1, 2, 4, 3 and 5

We agree, and have increased the integration of sections 3, 4, and 5 – as well as the explicitness of connections to the categorization in section 2 – in the re-organization.

2. The team could consider shortening some of the descriptive parts of chapter 3. At present it is long (50 pages) with the section on SRES being 15 pages alone. The question will be whether all of this material is functional to generating the conclusions in chapter 5.

We agree. We have shortened Section 3 considerably, in particular the discussion of SRES, and have moved the short cases to text boxes in relevant parts of section 4 as well as shortening them.

3. In the definition of user groups (section 2.6) seems to me to miss an important constituency – researchers and analysts. For instance, the main users of the SRES scenarios were climate modelers and climate impact analysts. I am also less convinced that one would be able to identify an “adaptation manager”.

Analysts and researchers as users of scenarios are now treated systematically in the sections on scenarios used in assessments. We think that many decision-makers can quite reasonably be identified as “impacts and adaptation managers”, and have provided several specific examples.

4. I believe that the discussion about probabilities in relation to socio-economic scenarios could be extended with reference to Berkhout and Hertin (2002, attached). Here the argument is made that complex and under-defined causality in social processes, innovation and reflexivity all play a role in making the future state of key parameters deeply uncertain, to the extent that attaching PDFs may be hard to justify.

The revised draft has added a discussion of reflexivity, principally in the context of representing decisions within scenarios, and makes reference to the arguments in the suggested paper. While attempting to assign probabilities poses many

difficulties and the issues raised by the reviewer make these even more difficult, we do not agree that they make any attempt to assign explicit probabilities inappropriate.

5. The questions of tautology (scenario assumptions pre-empting scenario outcomes) and circularity (for instance, is it correct to use a baseline scenario assuming no-policy, or should scenarios assume some policy even though this may a result of policy analysis using scenarios?) could be dealt with at slightly greater length (currently mentioned on p44, p61 and p121). They offer paradoxes which most scenario exercises need to deal with at some point. The report argues for a no-policy baseline for mitigation (p 121), but that may be highly artificial for the EU (and for certain US states) which is now implementing a whole range of climate policies.

The revisions deal with these issues in somewhat more detail, both in the discussion of consistency and integration in scenarios and in the discussion of representing decisions in scenarios. But while assuming a no-policy baseline when numerous policies are already enacted or committed is problematic, we believe it is problematic for relatively simple reasons – i.e., it is assuming something very unlikely or actually counter-factual – that do not much touch on the problem of tautology in scenarios. We find no logical incoherence in defining a baseline that assumes no incremental policies beyond sustaining those already adopted or committed (with some reasonable assumptions about implementation and compliance),

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Comments by Prof. Garry Brewer, Yale University

Author team responses in italics:

You asked for “additional observations and reflections” from my experience, and I shall begin with some of these before turning to both general comments about the mss and specific suggestions keyed directly to the mss.

Comments are related to page and line in mss.

Experiences

1. A commonplace in scenario design and use is the failure to distinguish well enough between predictive and heuristic purposes for the method. “Discovery is not Prediction,” is the way I tried to characterize this issue in a chapter I years ago contributed to a book on crisis management.¹ The failure is especially noteworthy when scenarios are employed by those trained primarily as scientists, for whom prediction represents the single and highest disciplinary objective. As a specific set, climate and energy models and modelers have demonstrated a consistent preference for predictive ends in their scenario activities.

A predictive end assumes that past trends will prevail on into the future and that the underlying and responsible generative systems, most particularly the human ones, will not experience structural or intentional changes. Physical systems are often exempted in these terms because they are immutable. The laws of physics are the laws of physics, more or less. However, in climate and energy problems, the human element is not so easily presumed or held constant, especially when the time frame of the analysis is long—say decades or generations in length. Humans are mutable and they are also “irrational,” especially with respect to our personal, interpersonal, and political habits and means.²

The heuristic end favors consideration of creativity and innovation, as when one focuses on outlier or aberrant behavior that in time and with basic system change may prove “normal.” It allows one to probe risk and uncertainty by posing and then analyzing the classic “What if?” class of questions to highlight the unknown. On rare occasions, the heuristic end may allow one to stumble onto some combination of elements and events that yields up an insight into a genuine “unknown-unknown.”

Humans act intentionally and so affect natural as well as social system outcomes and effects. Intentionality is considered in our laws, decision processes, and related ac-

¹ Garry D. Brewer, “Discovery is not Prediction,” in Andrew C. Goldberg, Debra van Opstal, and James H. Barkeley, eds., *Avoiding the Brink: Theory and Practice in Crisis Management* (London: Brassey’s, 1992): chap. 6.

² The so-called “human dimensions” efforts of Paul Stern, Tom Dietz, Lin Ostrom and a handful of others with and through the NRC comes immediately to mind here.

tivities and is clearly encountered within the range of normative social thought and theory. Humans are the causes of many natural and physical problems and we also suffer the consequences of our acts. Quite a bit of our plight can best be judged as irrational.

Human “irrationality” however is ordinarily considered within the confines of psychological or psychiatric theory and practice, if it is considered at all. The near total absence of social and behavioral elements in climate and energy models, analyses, and related considerations is a major shortcoming not readily resolved or mitigated by simple cutting and pasting of “human dimension” elements onto physical constructs and models.

The use of scenarios is one promising means to help redress this deficiency. Adopting a heuristic purpose may facilitate matters as well.

For instance, rather than trying to predict at what time the global mean temperature will increase by 1.0 C, and then wasting lots of time worrying about the spatial resolution or data quality used in one General Circulation Model versus another, suppose the analysis began with a stipulated end state at some agreed-to year in the future. The following simple hypothetical illustrates the point.

It will be 1.0 C warmer globally in 2075 than it is now. Regional differences will range both higher and lower than the global mean and can be assumed as follows [describe them.] Likely consequences following from these conditions are the following [postulate them.] Many of these consequences are costly in various human terms. Some however may be beneficial, as with the “winners and losers” economists are so fond of extolling.

Characterize more desirable or more acceptable end-state circumstances for the year 2075.

Under these conditions, how might we work our way back to the present and historical conditions to eliminate, reduce, or mitigate the unwanted or unacceptable consequences? What changes are required in existing human systems and arrangements to achieve a more acceptable end state?

Another way to engage human considerations, especially as a means to discover, is to rely on scenario-based games that mimic the classic “crisis game” known so well in military circles and analyses. The scenario in this case initiates a sequence of plays or responses meant to discover and explore various decisions and outcomes in circumstances never experienced before, e.g., thermonuclear war. The initiating scenario can be played by the same teams multiple times to elicit and generate different decision paths or it may be used by entirely different teams to explore and discover responses from different individuals, groups, or cultures. The so-called “A and B Teams” employed by the intelligence community at the height of the Cold War are illustrative. The “A Team” would operate in the business as usual mode, and often employed those responsible for that business, versus a “B Team” for whom decidedly contrary pessimistic or sometimes even optimistic views and assumptions about the world were featured.

The scenario in the classic crisis game “works” to the extent that it engages the human participants and helps them “think about the unthinkable,” in the morbid turn of phrase attributed many years ago to Herman Kahn. That this approach and mode of

thought can be beneficial is attested by concrete decisions made over the years not to rely entirely on strategic bombers but to deploy ICBMs on the ground and in submarines, in decisions to secure nuclear weapons with Permissive Action Links (PALs) to prohibit the “Strangelove Scenario” from ever taking place, and in numerous improvements in Communications, Command, Control, and Intelligence C³I across the entire strategic force.

In my opinion, few if any of these constructive uses and means have been employed in climate and energy models or analyses—this despite the fact that no one has any idea whatsoever of what human systems or decision pathways will look like or exist in the future most of interest some 25, 50, or 100 years hence. Simple extrapolation of “business as usual,” as was the case with nuclear warfare and intelligence estimates, is hardly satisfactory.

The key points in this are that “Discovery is not Prediction” and that scenarios can be usefully employed for an uncommon variety of different and appropriate purposes and reasons, especially when the subject is global climate change.

The revised draft provides more extensive discussion of the possibility of scenarios to serve heuristic and exploratory uses rather than more predictive ones. We agree with the reviewers’ suggestion that while this can be a valuable way to use scenarios, there has been little or no use of global change scenarios in this way.

General Comments

2. The report is excellent. It is thorough, pretty well organized, and written with unusual clarity—especially for a “group/committee” writing project.

No response required.

3. However, the audience is not evident. Indeed there are multiple potential audiences for this report and no one of them emerges as *the audience*. Lack of specificity here means that different parts of the report appear to be for decision makers (of many different kinds), modelers and analysts (of many different kinds), “the public” (whoever they might be), and probably a couple of other discernible groups and individuals. No straightforward solution comes to mind, although you might consider doing something unconventional to resolve this key weakness: Write three or four different Executive Summaries that clearly identify different audiences and then select and pitch the material to fit each group.

We agree that the draft failed to make the audiences for the report clear, although we did have a couple of specific audiences in mind. In the revised draft, we have extended the introduction to make the intended audiences explicit, and have also made modifications throughout the text to maintain consistency with these intended audiences.

4. For example, try to pull out the information that most relates and appeals to business people. The Global Business Network/Shell stuff is pretty well known; efforts by the insurance industry to cope with climate change are beginning to be known and could be elaborated. The insurance guys are in fact making decisions about climate change that have real and costly implications. Economic issues were mainly responsible for the political decisions that Bush and company made regarding Kyoto, and such issues could be culled out of the overall report with a bit of work. Tom Schelling's outstanding economic analysis in *Foreign Affairs* of how awful the Kyoto deal was for us is, on close reading, a scenario-inspired if not based, assessment. Now do the same for a couple of other key audiences: international decision makers; modelers—ecological, demographic, atmospheric, and oceanographic; high-risk populations; and so forth. The point of this recommendation/suggestion is to get more mileage out of the considerable efforts already expended in trying to cover the topic as this report does: both broad and, on occasion, deep.

Some of these actors fall within our definitions of the two classes of audiences for the report, although only in their discharge of certain specific responsibilities. While many of the arguments advanced in the draft could be of relevance to other users and decisions, we have decided not to extend it explicitly to additional classes of users, because this would further lengthen an already long report, and risk losing focus.

5. Somewhere very early in the report you need to state the obvious that all models are in fact scenario based and dependent. This is just another way of saying that simplifications are inevitable as we try to deal with enormous complexity of the sort found in the climate change topic. Similarly, there is no other way to think systematically about the future in such complex situations other than using scenarios. Finally, no one scenario can possibly capture everything of potential relevance, interest, or importance. All models (simulations, games, analyses) are simplifications. No one of them is necessarily “the best” for any and all situations.

We agree. These points are now made, both in the introductory material that defines scenarios and distinguishes them from models (among other things), and in the conclusions.

6. Somewhere late in the report, by way of summarizing many of the valid and important limitations you note (here, there, and everywhere) you need to collect and interpret the limitations in terms of “research needs” that range from the most common to the more specific and esoteric. Don't shy away from trying to set priorities and, if you have the courage and time, assign responsibilities to fund the work needed. The socio-economic aspects have been neglected for instance; likewise, the individual-level, human dimensions of these problems have been given short shrift. The crucial importance of scenario and analytic management is touched on here and there, but is not emphasized nearly enough. Having a disparate group of analysts pulled together to do a big, one-time study

of virtually everything related to climate change is far different from having a small group of analysts who routinely work together over long periods of time trying to understand and resolve specific problems. Lots of other potential topics are discussed throughout the report, but they are not collected, summarized, and lined up against the always useful “So what?” question.

We agree, and have extended the conclusions to discuss the crucial need for more efforts in development of scenario-based and related assessment methods, as well as certain specific needs such as socio-economic scenarios.

Specific Comments

[Keyed to page and line references – Numbering re-starts at 1]

1. Pg. 9, note at bottom: The role of “control” in the classic military crisis game is discussed in many of the open sources on military models, simulations, and games (MSGs). The problems related to who is in charge (is “God”) are comparable for climate analyses, although they are not as readily apparent nor are they commonly acknowledged. Since all models are simplifications, who decides on what eventually is included (and what logically is thus excluded) from the analysis? Who is the referee when disputes and other signs that consensus is not happening occur? Who has the responsibility (“power”) to end, redirect, or otherwise control the activities of groups involved in climate modeling and analyses?

The draft addresses this issue with respect to the need to involve identified users or their representatives in the development of scenarios, and the role of scenarios in coordinating and/or directing model simulations and research programs. We do not go more specifically into the processes by which the simulated responses to alternative decisions would be determined within a scenario-based exercise – i.e., the question of who is in charge – because this issue has not yet been engaged in climate-change scenario exercises, and is arguably less tightly connected to the creation of scenarios than is the case in military or security exercises.

2. Pg. 11, bottom half: There is another question that is even more important than the ones identified here. Is the objective to have one basic story, one big-deal with some pretensions about consensus, or to allow lots of different stories to be told? One or even a few (four or less) scenarios and stories will still be a very limited set of the possible ways problems as complex as climate change can be told. A standard limitation concerns the underlying assumptive bases used to construct the models, and this may in fact be more important than the models themselves. Such a finding has long been recognized in the

literature, but it is still remarkable to see how few people in the climate modeling (energy, economics, and a couple of other fields do somewhat better) own up to this.³

There are two points here. Both are important, and the draft addresses both. The first concerns the structure of a scenario set – how many scenarios are included, representing how many uncertainties. This is addressed in section 1.2 and section 4.6. The second concerns the dependence of all models upon scenario-based assumptions, because no model can endogenize everything (or even everything except observable starting conditions). This is addressed in the introductory material and the conclusions, as discussed in our response to General Comment 4 above.

3. Pg. 12, lines 13-21: Support for decision making. This needs emphasis. The technical guys go and “do their thing” and then someone at the end asks, “Who is the audience?” The answer to the question was actually presumed from the beginning to be “other technical guys like us.” This conceit becomes an issue when the technical analysis is then publicized with exhortations that the “decision makers” do something to avert this or that awful forecast outcome. Lack of specificity about which decision maker and what possible decisions might any of them in fact entertain and make is not a particular concern for the technical guys doing the analysis. No wonder responsible officials, as one possible type of decision maker, look askance or just ignore all this stuff.

We agree, and have argued extensively in the draft for the importance of clarity on the specific uses and users to be informed by a scenario exercise. This may include specific identified decisions to be informed, but may also include providing inputs for model runs that meet certain criteria, or more exploratory uses.

4. Pg. 19, lines 22-29: The importance of independent, competent, third-party MSG assessment is brought to mind with mention of Stanford’s Energy Modeling Forum. In its original guise EMF was created and funded to serve this essential role, and it did a pretty good job for a while when EPRI was still well funded and could “afford” to support things like EMF. I fear that in recent times the independent, competent, third-party functions have been neglected at EMF in the interests of being one of the climate change modeling players. There is a serious need to create and fund for the long term a couple of places whose only job is to assess and make transparent climate models. Requirements for comprehensive model assessment are not mysterious and have been around for more than 30 years. [More on this in Pt. #21, below.]

³ William Ascher, *Forecasting* (Baltimore: Johns Hopkins University Press, 1978); Martin Greenberger et al., *Caught Unawares: The Energy Decade in Retrospect* (Cambridge, MA: Ballinger, 1983); and Paul Craig et al., “What Can History Teach Us? A Retrospective Examination of Long-Term Energy Forecasting for the United States,” *Annual Review of Energy and Environment*, vol. 27 (2002): 83-113.

The text discusses the role of simple standardized scenarios providing a basis for model inter-comparisons. In addition, we have included a discussion of the need for providing comparison, explication, and quality control as one of the functions to be performed by the proposed new scenarios capacity.

5. Pg. 26, lines 9-23: Humans are mostly left out of the climate change stuff, and this makes for some real problems. This is not news: “The social and behavioral sciences provide an essential but often unappreciated knowledge base for wise choices affecting environmental quality. These sciences can help decision makers of all kinds to understand the environmental consequences of their choices and the human consequences of environmental processes and policies, as well as to organize decision-making processes to be well informed and democratic.”⁴ In short, humans are the cause and humans suffer the consequences of a goodly portion of what passes for the “climate change” problem. So where in the world are the humans in the majority of climate change MSGs?

In scenarios created for some climate-change purposes, human behavior is aggregated into emissions trends and their socio-economic determinants. We argue that this is likely to be adequate for scenarios to serve some purposes, e.g., informing decisions about impacts and adaptation. But for scenarios to inform mitigation policy decisions, it may also be necessary for scenarios to stipulate alternative choices or actions by other important actors – e.g., for EU mitigation policy to consider what the US does. Present global-change scenario practice does not include any examples of the latter, but the draft argues that scenarios of this type – including alternative specifications of choices by identified major actors – may be crucial for informing mitigation decisions by national officials or firms.

6. Pg. 28, Section 2.6: I believe that this section may be the most important one in the entire report if the main purpose of the report is to improve the use of scenarios in the climate change arena. If my belief is correct, then why bury this stuff instead of giving it much greater prominence? A reorganization of the existing text might help here.

This has been done. The section in question now appears at the beginning of section 2. Sections 4 and 5 have been reorganized in parallel.

7. Pg. 30, lines 2-8: Here is a stab at identifying some truly consequential audiences for this work. Given the obvious fact that we have few if any global decision makers, doesn't

⁴ Garry D. Brewer and Paul C. Stern, eds., *Decision Making for the Environment: Social and Behavioral Science Research Priorities* (Washington, DC: National Academies Press, 2005): 1.

it make sense to ask who does make decisions that seriously affect climate? On just the business side alone, this question once posed and answered yields an interesting collection of prospects—most of whom are never thought about except as an evil “They” whose profligate behaviors need to be reigned in or punished somehow to save the world.⁵ Actually, there are some very interesting and constructive possibilities to identify and factor into the business-as-usual climate change scenarios and models.⁶

We agree with the comment, which does not appear to require any changes to the text.

8. General Comment at Section 3: You have an “apples and oranges” comparison problem that suggests a partition into two separate sections, rather than this one where everything gets crammed together. There are the mainline (“usual suspects”) scenario-based models: IPSS, US National Assessment, UK CIP, and MEA. These should be grouped together and concluded with a crisp summary of their main, common, and useful aspects as well as their individual and collective limitations and weaknesses. There then follow a number of “Odds and Ends” or even “Odd Ball” studies: GBN, New York, Columbia River, Ozone, Gulf of Mexico, NAPAP/EMAP; and the insurance industry. This set needs rethinking. For instance you might add energy models, which makes a certain sense because you’ve already got ozone and sulfur (acid rain) models. There is lots of relevant experience in the energy realm, as I’ve pointed out in a couple of other comments earlier, and so this addition might be helpful. Alternatively you could get rid of ozone and sulfur entirely. If you did this I would suggest that you also collect out GBN and insurance and make this a separate section on business and the private sector. It would need some elaboration, but the importance of this sector in the climate change problem merits this treatment I believe. Andy Hoffman’s excellent survey, referenced at #7 above, is a good place to fill in the blanks, and since Andy is a colleague of Ted’s at Michigan, he might even be prevailed upon to add a couple of paragraphs specifically tailored to this report.

The section has been reorganized approximately along the lines suggested. The small specialized cases have been moved to text boxes within Section 4, leaving the four more extended treatments alone in Section 3.

8. Pg. 41, lines 11-23: There is a common tendency for those heavily invested in and/or responsible for a specific model to begin thinking and acting as though the model is the world rather than being a simple, frail representation of highly selected aspects of the

⁵ While not alone in his demonization of business and business people, J. Gus Speth, *Red Dawn in the Morning* (New Haven: Yale University Press, 2005), is both representative and symptomatic.

⁶ Andrew J. Hoffman, “Business Decisions and the Environment: Significance, Challenges, and Momentum of an Emerging Research Field,” in Brewer and Stern, eds., *Decision Making for the Environment*, op. cit.: 200-229.

world. The problem is clearest when lots of time and energy get invested in one or a few (four or less, again) scenarios or stories and where these scenarios depend heavily on lots of data that in turn depend on computer models. The problem, in short, is “the model is right, the world is wrong.” It also allows competing technical and professional egos to play a greater role than is healthy in these analyses. The “God” problem of those who perform the control function in simple, free-form, scenario-based crisis games that I mentioned earlier is alive and well in the climate change world.

The draft discusses this issue in considering uncertainties in scenarios, and also in the discussion of the predominant influence of quantitative models in determining the contents of scenarios in the SRES and US National Assessments.

9. Pg. 42, lines 16-23: This may be one of the most important paragraphs in the entire report. It merits more prominence—probably in the executive summary and also in the concluding comments.

This issue is highlighted more prominently in the revised draft, and called out in the conclusions.

10. Pg. 44, section on “Clarity about Uses”: The point is that increasing the number of participants in these exercises also increases the number of possible uses and misuses of the MSGs. I actually worried a great deal about this matter years ago for military and urban settings, but my concerns have been lost with the passage of time and especially for those who are rediscovering the issue in the climate change arena. Too bad, as it need not be so.⁷

The revised draft has separate discussions of the managerial difficulties involved in increasing the number of participants in scenario exercises, and the related problem of the difficulties that follow from increasing numbers and diversity of intended uses and users.

11. Pg. 46, line 12: Consistency of terminology. Earlier GCM was defined as *Global Climate Models*, which I found strange (pg. 21, line 38, and elsewhere.) I thought it meant General Circulation Models, as is the case here. Perhaps consider a Glossary of Terms?

⁷ Garry D. Brewer, “Some Costs and Consequences of Large-Scale Social Systems Modeling,” *Behavioral Sciences*, vol. 28, no. 2 (April 1983): 166-85; and Brewer, “On Duplicity,” *Simulation*, vol. 34 (April 1980): 140-43.

The terminology for GCMs has been made consistent, and the revised draft has been scrubbed for explanation of acronyms and technical terms at first use. A glossary of terms has not been added yet, but is being considered for the final published report.

12. Pg. 50, lines 14-18: The gross deficiency of socio-economic and human dimensions in climate change modeling is noted here, yet again. It is a key, central, critical (what else can I say?) limitation of all the technical stuff that passes for analysis in this field. Humans are the cause and humans suffer the consequences. So where are the humans?

See response to comment 5 above. Some elements of human behavior are represented in scenarios, although there are many uncertainties and weaknesses in the representations. Other aspects of behavior, particularly the strategic choices by other identified actors, have not yet been considered in climate-change scenarios, and the draft presents some proposals regarding how these might be used, to what benefit.

13. Pg. 51, line 18 through pg. 54, line 31: This pretty much sums up the core problem with climate change studies and analyses in a couple of pages. So why bury it in the middle of a 133 page report?

The implications of this material are treated more prominently in the revised paper.

14. Pg. 62, starts line 5 “Concluding points on MEA”: If one were to devise the worst possible way to do a study the MEA would be it.

The report is quite critical of the approach taken in the MEA, but also seeks to keep a focus on positive lessons for future scenarios practice, not excessively harsh criticism of past exercises.

15. Pg. 63, GBN illustration: (See previous comments about restructuring all of Section 3.) Andy Marshall created OSD Net Assessment in about 1974 and he still directs it some 30+ years later. He was a central war gamer at RAND in the 1960s and early 1970s, and he took what he knew about worst-case strategic analysis with him to the Pentagon. In the national strategic, nuclear realm there is a very high priority of this particular form of analysis. Not to prepare for the worst case and then to lose a war as a consequence is simply unacceptable. It is not clear to me that climate change is similarly burdened.

The revised draft includes more extensive discussion of worst-case analyses and interprets the GBN exercise in this light.

16. Pg. 80, Section 4.0: My previous question about possible audiences for the report looms large in this section. Parts of the draft are technical and designed for modelers, others are “text-bookish” and possibly aimed at students, and a few other parts are possibly of relevance and interest to “decision makers,” although various kinds of them are not identified. [Pg. 97, lines 12-44 is a pretty good start for this last audience, generally speaking.]

We agree. The revision of this section has cut the elementary pedagogic material, to focus more consistently on the two identified audiences.

17. Pg. 98, lines 10-11: Your own ambivalence about inclusion of acid rain and insurance shows up at this point. You need to think about and then resolve the inclusion/exclusion of examples question for Section 3.

This has been resolved in the revisions. The draft retains these two cases, but relocates them as text boxes near discussion of a relevant issue in Section 4.

18. Pg. 99, lines 26-35: “Who should be involved?” is a huge question. Those efforts that involved a “cast of thousands,” e.g., MEA, were a mistake obviously. Those efforts that were one-time, define the world and give three examples (scenarios) did not fare much better. What we have not seen so far is a dedicated group that is constituted and guaranteed funds for the long-haul of say 20-50 years to do this kind of work. The Energy Modeling Forum had some desirable characteristics, especially in the first decade of its existence, and it may be a good prototype upon which to design and construct something in the climate change arena. [Discussion on pg. 101, lines 16-27, flirts with some of the generally misunderstood and disastrously handled management issues.]

The revised conclusions stress the need for such an institutional capacity to review, compare, and critique scenarios.

19. Pg. 104, lines 4-14: The managerial issues related to scenarios may well be among the most important and under-appreciated of all the things you talk about in this report. There are some “lessons learned” in the community now, mainly learned the hard way and through trial and error, not by connecting to other previous experiences in other subject matters and fields. Incidentally, where does one go to learn how to design, run, assess, or manage scenario-based analyses? Those who do it have learned by the seat of their pants. As far as I know, there has been little effort to collect, codify, and then con-

vey these lessons to the current and upcoming generations of climate modelers. Naki and Arnulf Grubler learned this stuff mainly by hanging around IIASA when there was still a strong RAND influence on the institution. They did not learn it in a European university. So where would the current and aspiring generations of climate change analysts learn their trade?

The revised conclusions and recommendations address these points extensively.

20. Pg. 104, line 41 through pg. 105, line 5: The normative uses of scenario-based models, simulations, and games (MSGs) finally get recognition, almost as an after thought, on pp. 104-05. As I stated at the onset, this may in fact be the most important use of all given the complexity, values stakes and conflicts, scope, sweep, and scale of the climate change problem.

The revised draft gives more extensive discussion of normative scenarios and their distinction from the other cases we discuss.

21. Pg. 105, lines 15-21: The scenario assessment requirements are not any different than those required to evaluate and improve models, simulations, and games used for other kinds of applied problems. There are distinctive theoretical, technical, ethical, and pragmatic norms and standards to be applied in any case. That they seldom are is partly explained by the lack of communication that has historically existed between practitioner groups responsible for different substantive problems—such as urban, military, energy, and more recently environmental ones.⁸

The revised conclusions stress the importance of critical comparisons and development of scenarios methods.

22. Pg. 113, line 43 through pg. 114, line 3: State the obvious here. There is no global authority to make climate change decisions. Furthermore, the standard political cost-benefit calculus militates against and even prevents those having less-than-global range in responsibility to be disposed to taking the kinds of actions climate modelers and analysts want them to take. The political cost-benefit calculus: “Benefits now, for my constituents to be paid for later by someone else.”

The revised draft makes this point.

⁸ G. Brewer, *Politicians, Bureaucrats and the Consultant: A Critique of Urban Problem Solving* (New York: Basic Books, 1973); G. Brewer and Martin Shubik, *The War Game: A Critique of Military Problem Solving* (Cambridge, MA: Harvard University Press, 1979).

23. Pg. 122 and on, “Literature Cited”: This relies heavily on a set of current technical studies where scenarios are sometimes hardly the topic at issue. The reference list is very thin on scenarios, scenario methods, model evaluation and use and closely related topics—especially when one realizes that these kinds of activities have been going on, often for years, in fields not linked to climate change. The lack of linkage in no way diminishes the usefulness or importance of what has been learned elsewhere.

In the revision, both the text and the references cited have increased treatment of scenario methods and related topics.

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Comments by Ged Davis, World Economic Forum

May 31, 2006

Author team responses in italics:

General Comments:

Overall, this is a remarkable document that makes a strong contribution to global scenario practice. My comments should not be taken to detract from this overall assessment, but are intended to strengthen the effort.

1. Throughout the report underplays the critical issue of the framing of central questions. No doubt this is partly because the authors have presumed up front that the issue in question is narrowly focused on climate change, or some related area such as the global environment. But often in scenario work by far the hardest task is knowing how to frame the central questions that needs answering, in a way that is deeply relevant to users.

We agree. This is stressed in the discussion of decisions made in producing scenarios, Section 1.2.

Page-by-page comments.

2. Pg 1, line 27: The document says scenarios are used for issues with long time horizons, high stakes, and substantial uncertainty. “Long time horizons” is not quite right, as scenarios are also developed for short horizons, although in situations where there are still high stakes and substantial uncertainty. For example, companies and governments regularly do work on the potential outcomes of ‘crisis’ situations with time horizons measured in weeks or months. The appropriate time horizon for scenarios is determined by the time in which relevant processes unfold and the full consequences can be seen for the issues under review, and this may be relatively short or long.

This is corrected. The draft now stresses depth of uncertainty and size of stakes as the primary challenges that call for scenario-based thinking, with time horizon a secondary factor that may matter only because uncertainties deepen as time horizons lengthen.

3. Pg 6, lines 23-32. It is essential to evaluate and criticize scenarios in terms of the set of scenarios, not any individual scenario. The test of a scenario exercise is the relevance and usefulness of the set to policy, strategy and decision makers. Of course, individual scenarios need to be tested for internal consistency, alignment of narrative and quantification, etc.

We agree. The draft makes this point, in the introductory discussion of scenarios and the discussion of uncertainty in scenarios.

4. Pg. 7, line 17: A set of scenarios cannot cover and represent all uncertainties. There are always important elements of selectivity and design in scenario building. A set of scenarios is frequently developed to shift users' attention to new aspects of a problem, thus re-framing the problem and examining new, unexpected and challenging outcomes.

This raises a more general issue in scenarios. There ought to be something like a Hippocratic Oath for scenario builders, to be honest to the user group regarding the decisions made regarding framing and selection of the central uncertainties in a particular problem area. The worst sin of a scenario builder is to manipulate the framing to account for one's own personal prejudices.

We agree. The revised draft makes this point.

5. Pg. 8, especially the table: The emphasis given here to representing uncertainties in scenarios, while correct, tends to neglect the use of scenarios to elucidate those factors that are pre-determined. Identifying the factors that are pre-determined and important and elucidating their implications, is an important element of scenario creation and deserves a great deal of thought and analysis. The work of Peter Drucker provides a masterful illustration of the importance and value of serious reflection into factors that are determined but overlooked and under-analyzed – e.g., his work on the long-term effects of an aging population.

We agree. This point has been added to the revised draft in the discussion of dimensions of variation among scenarios in Section 1.2.

6. Pg. 30, lines 9-18 (but this issue appears in several places): To connect scenarios usefully to decision-making, one must think in terms of a nested sets of scenarios, each operating on a different time horizon. In much of his prior and current work there are normally three relevant time horizons, but climate change applications add a fourth. Most scenario exercises relevant to climate change thus far have tended to work on just one time-horizon, and attempt on that basis to draw conclusions for other time horizons. This is very difficult if not impossible given that different factors and forces are at work at shorter vs. longer time horizons.

In climate change, the four relevant time horizons are:

- 1) The 100-200-year time horizon of the relevant geophysical changes – looking into deep history and the deep future. This is the time-scale that defines the largest-scale planetary risks.
- 2) A roughly 50-year time horizon, which is the period for new technology sets entering and penetrating. In other energy and resource applications (i.e., not climate change), this is the longest time horizon considered.

(e.g., an OECD meeting last week was laying out plans for new R&D work related to nuclear fusion. They laid out a scenario in which the first commercial fusion plant would be deployed in 2050.) This time-horizon is important for explorers and technologists, but is quite different from the 100-200 year horizon. The work over this horizon is to understand potential new technological options and resource issues, and to make decisions (in the energy business) about exploration programs and basic physics and technology research programs.

- 3) Third, the 20-30 year investment horizon for new projects. These new investments are chosen from currently available technology sets, for which the longer-term technological development issues that are flexible in the 50-year time horizon are now constraints. Scenarios over this time horizon are built around issues of geopolitical risk, regulatory risk, consumer behavior, and other determinants of investment decisions.
- 4) Finally, a 5-year time horizon, that is concerned with immediately available actions, short-term flexibility of the system, and immediately available policy options, e.g., the bringing of renewables, or new nuclear plants, onto the agenda.

Each time horizon operates under different constraints. More factors are changeable over the longer terms, but these may also be constrained by decisions made in the near term. In such a nested set of scenarios, each time horizon bears on a different set of policy choices. Thinking of this structure helps resolve the question of why decision-makers have thus far derived so little use from scenario exercises. Most climate-change scenarios are a mix of the two longest time horizons, and they try to use potential developments over these horizons to influence shorter-term policy decisions. For example, in the SRES process, many decisions were made regarding what technology sets would be used, as though the resultant scenarios could advise on investment choices. They cannot without coupling them with consideration of shorter-term factors.

In subsequent work I led at Shell after the SRES, we tried to connect the scenarios more closely to decisions by focusing on the two middle time horizons. The work on a 2050 horizon mostly concerned resources and technology options; the shorter (20-year) work aimed at developing investment guidelines for fossils and renewables. (See “Energy Needs, Choices and Possibilities, Scenarios to 2050”, both the booklet and an abridged version for NAE journal “The Bridge”).

We agree. The revised draft includes discussion of the multi-horizon nature of some climate decisions and implications for scenario design.

7. Pg. 32 (this also arises on pg. 44): One of the great weaknesses of the IPCC scenarios, both in 1992 and equally in the SRES, was the need to make them conditional on an assumption of no mitigation policies. I argued against this

forcefully, but was informed we simply had to proceed this way because it was our mandate. This was a major impediment, because it is extremely difficult to do conditional scenarios that extend into the longer term.

In particular, this causes great difficulties for the highest-carbon scenarios. These not only require a reversal of historical trends of decarbonization as we shift back toward coal, but they also stretch the limits of credibility of the no-mitigation assumption. The difficulties are somewhat lesser for the lowest-carbon scenarios. While a similar negative-feedback process might be expected to operate on low-carbon futures, there is a long history of improving efficiency and decarbonization in the world economy, so one can construct plausible arguments whereby the continuance and strengthening of these processes generates low-carbon futures arise without intentional mitigation, although imposing this assumption still makes it somewhat more difficult to envision such futures.

The revised draft discusses this issue extensively, more in general and prospective terms than in terms of further criticism of IPCC.

8. Pg. 36, lines 10-18: Throughout the SRES process, there was a fundamental difference of view between the scenario practitioners and the modelers. This included terminology. Modelers use the word scenario for any quantified projection. Scenario practitioners are more concerned with classes of scenarios, which SRES was persuaded to call “scenario families.” By my accounts, SRES did not have 40 scenarios; it had four, each with one “marker” quantification plus several alternative quantifications. The proliferation of scenarios to 40 also complicates the assignment of probability, since the 40 comprise quantifications across families and non-representatively within families.

We agree. The revised draft makes these points.

9. Pg. 39-40: I disagree with the climate-change modelers regarding the absolute necessity of quantifying probability in scenarios. Scenarios done well are designed for a particular purpose and an identified user group. In constructing scenarios it is often most useful to take as a design target, the aim to make each scenario equally likely for the user population – i.e., they are to appear equally likely in the perception of the targeted users. You should not try to make one in a set more likely than the others. You must work to carry the case of plausibility for each scenario and demonstrate that it is worthy of consideration.

This advice is not meant to exclude consideration of lower-probability, high-consequence events. I call these “wild cards.” Where a set of scenarios might be constructed to capture perhaps 95 or 99 percent of the range of outcomes, wild cards are the important extremes that lie beyond that. As with scenarios, the focus of wild cards is on clarifying and informing decisions. The particular purpose of wild cards is to allow questions of the form 1) What should I do in the unlikely case a wild card happens?; 2) What should I do to monitor this possibility in order

to be able to respond more quickly?. For example, I would view the GBN work for the Pentagon as principally about identifying the consequences of ‘abrupt climate change’ and highlighting the need for appropriate monitoring measures of the possible emergence of this high impact phenomenon.

The revised paper discusses treatment of probability in scenarios, although we reach a conclusion somewhat more favorable for explicit characterization of probabilities. The treatment of low-probability wild cards is quite consistent with the reviewer’s comments.

10. Pg. 42: I agree strongly with the point regarding under-development of narrative scenarios in the SRES process. This work was approached principally by starting with the modeling groups and building the scenario frameworks around them. At Shell and my subsequent work, we do it the other way around. Begin with a rich narrative, then ask what must I model to make these real and credible, to enrich the resultant dialog with policymakers, so the dialog can take place with them being confident that the necessary analysis been done.

At the Paris workshop, several people were carrying models on laptops that allowed back-of-the-envelope quantification of the initial scenarios being sketched. These were necessary to give spine and structure and form to the stories.

Modelers were given freedom to model the fundamental assumptions about population growth, economic drivers, etc. These fundamental assumptions tended to persist through the rest of the exercise.

To develop global change scenarios properly, you would need not just one workshop, but much deeper work to develop narratives and the range of assumptions on drivers. Perhaps in the future climate-change scenarios should be developed using a small team of 3 – 5 people working full time. These people would play the role of honest brokers re assumptions, and would do much of the coordination and more detailed analysis. There was some of this in SRES, but not enough to offset the dominance of the modelers.

In addition, such an exercise needs a challenging advisory group – an energetic, expert group probing and questioning assumptions and suggesting alternatives early in the process, to anticipate potentially difficult issues that might emerge later. This group should be challenging the basic qualitative logic before proceeding to quantification, and continuing thereafter. For example, the controversy over purchasing power parity, which I agree was an overblown issue in the criticism of SRES, might have been raised earlier. Similarly, the strong assumptions about convergence of incomes between industrialized and developing countries – really adopted as a normative equity goal – would have been challenged earlier by realists, rather than being left hanging – and not very well justified or explained – as a point for attack of the results.

There was a problem in SRES of transparency of the models. The models were not complete black boxes, as each team provided information about their model structure, but their relationships in mapping drivers to consequences were not fully transparent. It was usually not possible to diagnose the origin of different results between modeling groups. The resultant loss of transparency impaired the process's capacity to explain and understand.

Transparency is enhanced if models are developed at least in part to serve specific purposes in the scenarios exercises. Models that come to the exercise complete have a particular responsibility to provide transparency. The value of transparency in analysis is so great that I would even prefer a simple, transparent spreadsheet over an opaque model (although controlling for transparency, I prefer richer models to spreadsheets.)

Scenarios are also devices for bringing to the attention of policymakers fundamentally new concerns for their consideration. Thus Shell's 2001 scenarios contrasted the familiar world of the 1990's which highlighted rapid globalisation and reliance on markets with an emerging, poorly understood world characterised by stronger states, nationalism, tensions and pervasive security issues. This work done prior to 9/11 encouraged strategic dialogue and attention to emerging challenges. (See attached document, "People and Connections, Scenarios to 2020", on Shell's scenarios, produced in 2001.)

Most of these points are made in the revised draft, although we have refrained from making any specific institutional suggestions regarding how to organize the scenario capacity we recommend. Instead, we identify criteria for successful performance. Clearly a strong advisory board would be one way to help advance several of the criteria we state.

11. Pg. 42: In my view, the SRES scenarios should have had descriptive names. The names were dropped because of a real fear that those who would find any argument to critique the scenarios would find the names an irritant or a particularly vulnerable target. Names, metaphors and images are important for embedding scenarios in the user's mind. They are helpful in memorising and can clarify and highlight the meaning of scenarios for the targeted policymakers.

We agree. The revised draft makes this point.

12. Pg. 80: A successful scenario exercise has an absolute need for narrative clarity and logic. Many climate change scenarios have not spent enough time getting that right. This makes all the rest problematic.

We agree. The revised draft makes this point, in Section 1.2 and the conclusions.

13. Pg. 84: While this is not the only example of good practice, the Shell scenarios work following SRES made a serious attempt to link narratives and models. (See “Energy Needs, Choices and Possibilities, Scenarios to 2050”, both the booklet and an abridged version for NAE journal “The Bridge”).

The revised draft refers to both these exercises, in the context of how scenarios have been connected to decision-making. Unfortunately, the published accounts of these exercises do not include methodological detail regarding how the integration of narratives and models was achieved.

14. Pg. 88: The problem of drawing on collective expert opinion is not just one of aggregating. Often the issue is trying to understand who is the most insightful person in an area. In effect, the storylines and logics of such a person or these people get more weighting. You also often find that their views are most strongly based on available evidence – a fact that makes it easier to present a judgment based on unequal weighting of relevant experts, as the reference can be to the evidence instead of to the individual experts.

We agree. The revised draft makes this point.

15. Pg. 90: There is a strong distinction to be drawn between scenarios and wild cards. Scenarios are for policy formation within the main bounds of the distribution, perhaps 95 or 99% of probability. They are used to draw out current assumptions about how the world is working and test them, to make sure we feel we have got robust premises for decisions. But it is also necessary to look at extreme cases. As one example, in designing the major new Troll rig in the North Sea, Shell did look at the 2050 situation with climate change and the potential worst-case of sea level rise, and decided as a result to build the rig one meter higher than they otherwise would have.

We agree. The revised draft discusses the importance (and specific applications) of wild-card scenarios.

16. Pg. 91. Of course scenarios cannot cover the total range of all relevant uncertainties, and this becomes more strongly true as the scenarios grow more complex. There is judgment and selection everywhere in the design of a scenario set.

We agree. The revised draft makes this point, in Section 1.2 and the discussion of treatment of uncertainty in scenarios.

17. Pg. 94: The argument that scenario builders should explicitly quantify probabilities presumes that scenario builders are better informed than users. But if the main thrust of a scenario exercise is to aid the users in their decisions, there is tremendous value in having strong discussion among users regarding probability assignment. If experts or scenario builders do this, a highly valuable

conversation among users is reduced. The one factor that would push the other way would be cases in which relevant probabilities are strongly defined by scientific expert knowledge, perhaps say in climate modeling. In climate change, this may map onto the hierarchy of nested sets of scenarios discussed above. The case for expert involvement in assigning probabilities to scenarios is strongest at the longest time-scale, 100-200 years, a period over which no one has experience and scientific knowledge of slow geophysical processes is likely to predominate. As the time horizons grow shorter, the case for user involvement in arguing and assigning probabilities grows stronger.

This is a very interesting proposal, which adds a new dimension to our discussion of assignment of probabilities. We have added a discussion of this to the section on quantifying probabilities.

18. Pg. 100: We have now developed procedures by which we can use up to 50-80 people in building scenarios. This is quite advantageous, since bigger sets of participants brings more diversity of view. (See attached document, "Scenarios: An Explorer's Guide")

We agree. The revised draft makes this point.

19. Pg. 104: While there is an important distinction between "positive-outcome" scenarios and those that are purely normative – i.e. aiming at achieving a desired state of the world, and identifying how we get there – many positive scenarios also have embedded normative elements. This is not easily avoidable and is not necessarily a problem, but does require that the intentions and assumptions of scenario makers be declared as explicitly as possible.

We agree. The revised draft makes this point.

20. Pg. 111: In developing scenarios to support decisions, the second time-horizon (20 years in energy) is a good starting point, because this is the period over which investment decisions are made. Scenarios can be extremely helpful in project evaluation. With the addition of some intermediate logical steps from scenarios to workable investment policy guidelines, one can use scenarios to test projects.

The revised draft has added this possibility to the discussion of uses that scenarios can serve for energy and technology managers.

21. Pg. 111-112: Scenarios and decisions. It is important to emphasize that scenarios are perishable goods. They have a useful life for policy-makers that is much shorter than the time horizons of the scenarios themselves.

We agree. The revised draft makes this point.

Global-Change Scenarios: their Development and Use US CCSP Synthesis and Assessment Product 2.1b

Comments by Dr. Robert Lempert, RAND.

May 31, 2006

Author team responses in italics:

OVERVIEW

This draft has a wealth of interesting and valuable material. It raises a number of important and interesting issues.

1. However, the draft does not yet seriously grapple with basic questions implied by the title – the development and use of global-change scenarios. Such questions include: Why do organizations turn to global change scenarios and what do they expect from them? What alternatives did they consider? Do current global change scenarios serve the goals for which they were intended? Why or why not?

We have attempted to engage these questions, both theoretically in the discussions of potential uses for scenarios and empirically in the specific cases discussed. We have also attempted to state and support more clearly our arguments and conclusions regarding the diverse expectations from scenarios, how they have been used, and what purposes they have served, in the revised paper. Providing complete and systematic answers to these large-scale and challenging questions, however, would require an extensive program of primary research well beyond our mandate and capabilities in this study.

2. The failure to address such questions contributes to the draft's ambiguity about what they authors mean by a scenario and make it unclear how broadly or narrowly the reader ought to read the draft's discussions on the uses of scenarios. Once the authors grapple with these questions, the rest of the report should fall more easily into place.

We do intend our definition of scenarios to be broad, but not ambiguous. The revised draft has added several paragraphs clarifying the boundaries of what we count as scenarios and distinguishing them from other things they are often confused with. These clarifications have been added both in the introductory sections and in the opening passages of the conclusions, because some reviewers objected to the conclusions on the basis of a different conception of scenarios than we were using. We have also been more explicit about delimiting what types or subsets of scenarios particular conclusions apply to.

COMMENTS ON CONCLUSIONS

The first two of the draft's conclusions illustrate this lack of specificity as to what is meant by "scenario." The third conclusion illustrates questions about how broadly or narrowly a reader should interpret the draft's discussions about the use of scenarios.

Responses are under specific elaborations of the comment below.

3. The first conclusion reads, "Scenarios are required for responsible decision-making on global climate change." The authors contrast scenario analysis to: 1) not thinking about long-term risks or 2) assuming the future will be like the present. Clearly organizations have a broader range of alternatives in thinking about the future than this listing would imply. Thus, this first conclusion can be interpreted in several ways. The authors might be arguing that the uncertainty facing policy-makers is so deep that they cannot responsibly use traditional decision analytic methods based on subjective probabilities and expected utility analysis. Or they might conceive scenarios and subjective utility analysis as synonymous. Or they might regard scenarios as a subset of subjective utility analysis, particularly useful for examining and communicating interesting points in the distribution of future states of the world. Any of these conclusions might make sense, but it makes an important difference to how one interprets the claim that "scenarios are required" whether the authors regard scenario analysis as an alternative to, synonymous with, or a subset of the subjective expected utility decision framework.

The clarification of the definition of scenarios presented in the revised draft addresses this. In addition to distinguishing scenarios from other types of descriptions of future conditions intended to inform decisions (e.g., projections, predictions, forecasts), the text now also distinguishes them from assessments, models, and decision analyses: scenarios can provide inputs to any of these when they need future conditions stipulated, but they are not an alternative or substitute for any of them.

4. The second conclusion, "alternative decision strategies – including the pursuit of robust strategies – do not avoid the need for scenario-based thinking about potential future conditions" further confuses what the authors mean by scenarios. All robustness frameworks of which I am aware are based on a notion of multiple future states of the world and are entirely consistent with at least some concept of scenarios. Kees van der Heijden who works in the Shell Oil/Global Business Network (GBN) scenario tradition argues that one important purpose of scenarios (in the GBN sense of the word) is helping organizations assess robust strategies. Our robust decision making work identifies sets of future states of the world or sets of multiple probability distributions over which strategies are robust and specifies certain important clusters in these sets as scenarios. The economists who examine the robustness of monetary policies use alternative structural models of the

economy, each of which can be regarded as a scenario. This conclusion may rest on some implicit definition of scenarios. If so, the authors should make their definition explicit.

We agree that these are all consistent with using scenarios, defined as alternative stipulated future states of the world. The clarifications of the definition of scenarios in the revised paper detail how any of these approaches to decision-making are consistent with – indeed, can require – scenarios.

5. The third conclusion reads, “scenarios of greenhouse gas emissions and resulting global climate change are needed by so many different users for so many different purposes, that they should be provided in a coordinate manner for the CCSP... these can be provided centrally, provided the underlying reasoning and likelihood judgments are made as explicitly as possible.” This may well be true, but it raises questions about how broadly the reader ought to take such statements about scenario use. For instance, we live increasingly in a networked world where information is available from a plethora of sources and the key players, from Wikiedia, to eBay, to Google to iTunes, position themselves as trusted intermediaries that provide structured access to this vast array of information. Why should CCSP be the central provider of scenarios and likelihood judgments? Is this a very narrow conclusion or a broad one? Have the authors considered and rejected an alternative model where, for instance, CCSP is a clearinghouse for all the emissions and climate scenarios generated worldwide and provides assistance to diverse users in finding and evaluating those most useful to them? Or are the authors just reiterating the important but common observation that analysts ought to make their underlying assumptions as clear as possible when reporting information to decision makers?

This conclusion has been revised and clarified. It now proposes that CCCSP support the development of a capacity for scenario production and use. Rather than propose a specific institutional mechanism for achieving this, we discuss several criteria necessary for success and identify only a couple of institutional mechanisms to be avoided.

COMMENTS ON SECTIONS 1 AND 2

6. Section 1 entitled “Scenarios, their Characteristic and Uses” could say much more about the uses of scenarios and provide a more structured discussion of the diverse types of scenarios used in various applications and the important differences in the ways people use the word “scenario.” The list of scenario definitions that starts the section is interesting, but provides no structure for the draft’s subsequent discussions. If the authors want to make the point that there are many different types of scenarios they should offer a categorized list of different types of scenarios, or use one already in the literature, such as the excellent typology proposed by Marjolein van Asselt and her colleagues.

The revised draft does this. While it retains retain the simple sorting of climate-change scenarios according to where they fit in the simplified causal chain of the climate issue, it also introduces three additional key definitional characteristics of types of scenarios that draw in part on the Van Asselt et al taxonomy.

7. More importantly, the authors should emphasize and organize early on the very different uses to which organizations put scenarios. At the most basic level these range from the Wack and Schwartz school which uses scenarios as tools to change the mental models of specific decision makers, to the use of scenarios as a set of standardized input cases for comparative runs of different simulation models. These are clearly very different purposes, and imply different meanings of the word scenario. Clarifying the different uses at the start of the document would greatly help the authors organize and situate their subsequent discussion to avoid the problems such as those with the conclusions mentioned above.

The revised draft now provides more detail on the many potential uses of scenarios, including exploratory and heuristic uses as well as more direct decision support.

8. Section 2.6 “Scenarios for Climate-Change Decisions” takes a narrow view of decision-making. It focuses on providing information to *individual* decision makers who will presumably use it to decide what is best to do. It neglects organizational and group decision-making. Yet with the possible exception of the abrupt change scenario GBN developed for the Pentagon’s Office of Net Assessment, all the case studies presented in this report involve the use of scenarios by organizations. This focus is not surprising, since the use of scenarios as tools for organizational decisions making is a central theme of the scenario literature. The need to gain consensus among individuals and organizations with vastly different interests, values, and expectations about the future is a central problem for climate-change decision makers. Yet such issues are entirely missing from this discussion.

We disagree that the prior draft only addressed applications of scenarios to individual decision-making, but the revised draft nevertheless provides expanded discussion of the uses of scenarios in settings involving multiple and heterogeneous decision-makers, and the distinct challenges that arise in such settings.

9. As one tiny example, lines 22-24 on page 30 states, “ national officials ... will need this information principally aggregated to the national level.” But aren’t many national official in democratic countries intensely interested in the distributional consequences of potential policies, since there is a strong correlation between such distributional consequences and the political support or opposition a policy gathers in the national legislature?

We agree, and have modified the draft to reflect this comment. The draft now notes both national decision-makers' need for finer-scale as well as nationally aggregated information, and the potential involvement of sub-national officials in both adaptation and mitigation-related decisions.

10. More broadly, scenarios are often offered, as one of their central purposes, as tools to help groups with differing views agree on a common, operational vision of the future. There is no sense from this section of the draft on why this might be an important concern for climate-change decision-making and how different types of global change scenarios might aid or detract from this goal.

Two changes in the revised draft take note of this point. First, we have noted "clarifying points of potential agreement or disagreement" as one of the exploratory uses of scenarios. Second, the revised draft provides more detailed discussion of the diverse potential uses of scenarios in pluralistic political settings.

COMMENTS ON SECTION 3

11. Section 3, "Review and Critique of Global-Change Scenario Exercises" provides a wealth of interesting and useful information about the details of scenario implementation, but, with only a few welcome exceptions, is largely devoid of discussion of why different organizations turned to scenarios and how those scenarios helped or hindered those organizations in achieving their goals.

This recapitulates comment #1 above, and is addressed in our response to that point.

12. On p. 31/ln 35, the draft states that the mandate for the 1992 IPCC scenarios explicitly excluded any mitigation policies. This is a key issue. Why was this mandate made?

This comment, along with a few that follow, poses cogent questions about the reasons particular decisions were taken in the scenario exercises we review. While we are still conducting a few more inquiries to track down some of these uncertainties about the reasons for particular past decisions, producing well-founded answers to all of these will require more primary research than we are able to provide in this report.

13. On p. 40/ln 39-40 the draft glosses over what would appear to be a central question. The draft notes that the SRES scenarios began their life as GBN-style scenarios illuminating key driving forces, they are generally used as a range of emissions projections. Why is this so? Does it necessarily need to be the case? Is the GBN vision of scenarios as a set of focused narratives that change decision makers'

mental model impossible to implement in the large bureaucratic settings where climate scenarios reside? Is it impossible to implement by climate and emissions modelers who don't personally know the climate change decision makers? If the GBN vision could be implemented would it be valuable to do so? There are a host of questions about the real use of and potential value of scenarios in climate-change contexts raised by the life history of the SRES scenarios that would usefully comprise a chapter in this report. They deserve much more discussion than they are given in this draft.

The revised draft provides a little more detail on this point, but for the most part our response to comment #12 above applies. This is a research question that we were not able to answer definitively in this report – and it is an important and general one, as the concluding discussions now indicate.

14. The comment on p.42/ln 9-12 that “the failure to consider less fortunate futures, including ones that might seriously challenge the adequacy of current responses, institutions, and decision-making capabilities, may represent a significant weakness in scenarios to be used in planning long-term management of climate change” is exactly on the point, and an example of the sort of issue this draft ought to highlight. How serious is this problem, how pervasive is it, and is it a fundamental problem of the scenario process or a cosmetic one, what can be done about it? More discussion of these points would be very beneficial.

In terms of drawing this judgment about SRES and characterizing its seriousness, we believe the present draft is already as directly critical as is appropriate, given our primary purpose of providing guidance for future scenario exercises. In terms of the reasons for this decision, the revised draft provides some discussion in terms of SRES being instructed to respond to criticisms about implications of the IS92 scenarios that some observers found objectionable.

15. The problems with developing the SRES story-lines, mentioned on p. 42/ln 25-32 are similar to those well-documented in the ethnographic studies of scenario processes conducted by Marjolein van Asselt and colleagues. This draft should put its observations in such broader context.

The issues identified in the van t’Kooten and van Asselt paper are distinct from those we identified here, but we have noted them in the discussion of the two-by-two scenario structure, where we find them more apt. (Note: We have confirmed with the reviewer that this was the paper he was referring to.)

16. The authors might compare the SRES process to that of the Global Scenario Group (GSG), which similarly developed a set of storylines articulated by model runs. But the GSG seemed to have less trouble combining the storylines with the quantitative

results. There are a number of differences in the processes, including that the SRES scenarios were produced under the direction of governments while the GSG is a collaboration of non-profit organizations, and the SRES scenarios aim to provide a non-biased scientific view while the GSG took a more advocacy position.

We have briefly noted the GSG exercises, in the context of the revised report's expanded discussion of normatively based scenarios. We do not find much in common between the challenges faced by these two activities, however. The normative orientation of the GSG exercise is a crucial distinction. More fundamentally, the modeling underlying the GSG project is a simple spreadsheet-based accounting framework, so the challenges SRES faced of integrating distinct causal logics of narrative scenarios and quantitative models did not apply to GSG.

17. On p. 57/lns 22-27 and p. 58/lns 10-12 the draft mentions that the UKCIP scenarios are being used by several organizations to inform decision making. It would be very useful to have much more information on why these organizations are using the scenarios, what they are using them for, and how well they meet their needs. At very least, would it be possible to talk to the users and report what they had to say?

The revision provides a little more detail on these points. As with several of the points requesting further research, however, this would require primary research that has not been done yet. We have drawn on the few evaluations that have been done of the application of these scenarios.

18. The discussion in Section 3.5 on the Pentagon/Global Business Network Abrupt Change Exercise suggests that the authors interviewed scenario-developer Peter Schwartz. Schwartz writes extensively, as a principal exponent of the Shell Oil/GBN scenario school, on the topic of the questions missing from this study – why organizations turn to scenarios and how those scenarios can help organizations achieve their goals. It is thus disappointing that this section offers no critical assessment of how Schwartz's vision of how organizations ought to use scenarios -- described in such detail in his, Pierre Wack's, and Kees van der Heijden's writings - - played out in this particular cases

The revisions provide more elaboration of their heuristic and exploratory conception of scenarios. The GBN exercise is not a particularly good illustration of this philosophy, however, as it was largely a staff exercise done with limited input from the client office only at the beginning and end of the process.

19. On p. 70/ln 7, why haven't these scenarios been incorporated into any operational decision?

Because the relevant decision-makers have more immediate priorities and are only slowly coming to recognize the need to incorporate climate change in their long-range planning decisions. The revisions provide more detailed discussion of this issue, more generally than just in this case.

20. Section 3.8 on Scenarios of Ozone Depletion in International Policy-making presents a success story for scenarios. The draft reports that this scenario exercise was “highly influential in breaking the deadlock in international negotiations.” It would be interesting to learn more about why this was the case, what characteristics of the scenarios were aligned correctly with what characteristics of the political environment in which they appeared made the scenarios so influential? More broadly, perhaps Section 3 of this report might begin with this case study and others where you can say something concrete about how organizations used the scenarios and why. In this list I might include the Pentagon Abrupt Change Scenario, the work of the Global Scenario Group, your case study on NAPAP vs. EMAP, and the use scenarios in the most recent California Department of Water Resources (DWR) Water Plan, whose 2005 Public Advisory Committee update describes some of the success and frustrations DWR experienced with their scenarios. Then you can move to discussions of the other scenario case studies in the draft where it may be harder to assess why and how organizations have used them for decision-making.

The revised draft has extensively reorganized the treatment of the short cases, to couple them with the thematic discussions where they are most relevant.

21. On p. 71/ln 10, why were changes of sea level rise not considered in this scenario analysis?

(This comment refers to the case on climate-change in the Columbia River system.) The demands that were assessed and projected all occur substantially up-river from the region subject to any tidal and sea level rise effects.

22. On p. 75, this discussion of how and why the EMEP scenarios were used is excellent and should be a model for the rest of the draft.

The revised text has retained this case and attempted to sharpen its lessons.

COMMENTS ON SECTION 4

23. Section 4 on “Issues, Challenges, and Controversies in the construction and use of scenarios” contains much useful discussion, but could usefully be reorganized to provide more structure for the overall report, and for the controversies discussed in this section. The following discussion will address the subsections in the order in

which I believe they should be presented. This ordering, which emphasizes how scenarios are used rather than challenges facing their developers, would help address the draft's relative lack of attention to the former and provide useful context for the discussion of the latter.

The revised draft has re-organized Section 4 along the lines suggested.

24. Sections 4.5 “Scenarios and Assessments in Climate Policy Debates” and Section 4.6 “Scenarios and Decisions” describe two of the key uses to which organizations put global change scenarios. Both provide some good discussion of the key questions underemphasized in this draft -- Why do organizations turn to global change scenarios and what do they expect from them? What alternatives did they consider? Do current global change scenarios serve the goals for which they were intended? Why or why not? To emphasize their importance, the authors should consider beginning Section 4 with these the current Sections 4.5 and 4.6, and do much more to highlight their main themes and lessons in the introductory comments (currently just three lines) to this section.

The revised draft has re-organized Section 4 along the lines suggested.

25. Once the draft had laid out the ways in which scenarios are used and the goals for their use, it could then describe the process of developing scenarios. Section 4.3 contains much useful information. In particular, many useful points are raised in the discussion of the differences between the relationships between scenario-developers and clients in the classic GBN model and that often found in the climate change area.

Having described the uses of scenarios and the process of developing them, the authors could then usefully turn to the consistency and integration of scenarios, currently in Section 4.1, and then to the treatment of uncertainty, currently addressed in Section 4.2. The draft's current discussion could greatly benefit from such placement in an overall context.

The revised draft has re-organized Section 4 along the lines suggested.

26. The current discussion in Section 4.2.1 and 4.2.2 offers a lengthy five-page detour describing probabilistic estimates of future projections differentiated by values of one quantitative parameter. This is obviously a limiting case that allows for simple discussion of the subjective probability framework, and also describes the most commonly used output of the SRES process – the range of quantitative emissions scenarios. But the basic point of the draft's discussions – that one can usefully place probability distributions over the range of values for a single parameter – is sufficiently obvious to require little more than a few sentences. The relevant and

interesting question – why did the SRES exercise devolve in its audiences’ minds to a single range of emissions paths – is not addressed by this discussion. There are numerous other topics which the authors could usefully mention in these five pages, ranging from a description of different formalisms for characterizing the types of uncertainty addressed by scenarios (e.g. single subjective probabilities estimates, probability intervals, imprecise probabilities, belief functions, the quantification of scenarios as vulnerabilities of robust strategies which derives from robust decision making, etc.); a discussion of the vast literature on how organizations use, misuse, and process uncertain information; to a description of the Peter Schwartz/Pierre Wack concept of scenarios which in a significant lack is never presented in any coherent fashion anywhere in the draft.

The section has been substantially cut, as suggested.

27. Section 4.2.5 on the debate of quantifying probabilities provides a useful discussion of an important topic, but could easily stand on its own without Sections 4.2.1 and 4.2.2. The authors identify the core issue when they write (p. 95/ln 35-38) “a final argument against quantifying probabilities is that the attempt to do so may represent an unhelpful distraction that consumes times and resources, generates conflicts, and is of little value to scenario users. Whether this is the case, of course, is in part a judgment to be made by scenario users, not developers.” The authors should certainly cite any literature of which they are aware that shows that organizations that use scenarios with quantified probabilities obtain better outcomes than organizations that use scenarios without such probabilities. Otherwise, the authors should acknowledge that their preference for placing probabilities on scenarios is based on their personal judgments as to what is most useful. The authors’ argument that the usefulness of quantified probabilities may be enhanced in situations where there are large numbers of diverse decision makers is a useful contribution to the debate.

While we are unaware of any systematic research indicating differential effectiveness in scenario use depending on whether probabilities are expressed or not, the revisions have provided more explicit supporting argument for under what conditions we expect explicit probability assignment to be most useful, and why.

28. The authors seem to assume without justification that there is only one way to provide quantitative information comparing the likelihood of alternative scenarios. Given the degree of uncertainty, perhaps some type of imprecise probability of belief function would be more useful than a single probability density function. Perhaps a range of probabilities would be more appropriate, so that scenario developers might specify that none four SRES scenario families have less than 10% or more than 90% likelihood. Perhaps, consistent with the draft’s discussion on p. 106, probabilities ought to be contingent on socio-economic-political events for which the scenario-users’ subjective probabilities might be better than those of the

scenario developers. For instance, the scenario-developers might make clear that a high probability of low SRES emission scenarios is contingent on certain future political events about which the users might have better knowledge than the climate experts. Whichever way the authors choose to argue, they should make clear whether they are arguing from literature on how organizations use such information or their own judgment about how organizations should use such information.

The revisions provide more detail on various means and degrees of specificity with which probability judgments can be expressed.

29. Section 4 could then close with the current section 4.4 on “Communication of Scenarios.” It would be useful to add some discussion of communicating the narrative components of scenarios, in addition to the quantitative information.

We have taken this suggestion in re-organizing Section 4.

SUMMARY COMMENTS

30. The authors of this draft face a challenging task. Scenarios are widely employed. But there are many competing scenario concepts, and the literature on how they can be most effectively used is sparse. In contrast to many of the other CCSP reports, the authors of this draft had to structure a body of information characterized by a very high ratio of anecdote to formal findings. The authors have assembled a great deal of useful information and raise some important points. Nonetheless, the document fails to grapple with the central issues implied by its purview – how and why organizations use scenarios, how these scenarios do and do not help these organizations meet their goals, and how future global-change scenarios could improve their contributions. Once the authors have decided what they can say about these questions and organize their draft accordingly, much of the useful information they present here should fall into place.

This reprises earlier comments. Some of these suggest reorganizations, which we have largely adopted. Others request additional empirical research. We have provided a little more in the revised draft, but several of these requests go beyond our capabilities or mandate in this project.

31. The authors should also keep in mind for both themselves and for the reader the caution that begins the draft, that is, that the judgments expressed here are often based on the personal experiences of the authors and not scientific studies of how scenarios have and could contribute to organizations’ goals. Given the nascent state of the literature in this area, this draft more usefully than most might suggest areas where future research might contribute most significantly to our understanding of the development and use of global change scenarios.

In the revisions, we have attempted to make this point more clearly and forcefully.

MINOR COMMENTS

32. p. 2/ln 13: Conventional methods often generate errors often generate serious errors projecting out less than 10 or 20 years. Remember the peaceful end of the Soviet Union, 9/11, the Internet bubble?

This section is revised so it can no longer be taken to imply that there can be reliable socio-political projections a few years in advance.

33. p. 11/ln 21-26: The quote from Pierre Wack comes from a concise statement of the purpose of scenarios which is counter to much of what is described in this draft. It seems odd to use it here without taking on the broader argument.

It is hard to get around the fact that much of what is presented as scenario-based analysis in the climate-change field would not be recognized as scenarios by Wack. The broad purview and broad definition of scenarios we have adopted make such disjunctions inevitable.

34. p. 29. Nothing on state level officials? More climate policy is going on at the state level right now than on the federal level.

The earlier draft had considered state officials for adaptation and impacts decision. The revisions have now noted that they may also be involved in mitigation-related decisions.

Global-Change Scenarios: their Development and Use US CCSP Synthesis and Assessment Product 2.1b

Comments by Prof. M. Granger Morgan, Carnegie-Mellon University

Author team responses in italics:

1. The definition of "scenario" appears to be quite elastic. Over time it seems to grow to include what others have typically termed "forecasts," "projections," or even sensitivity analysis. This became apparent to me only gradually (e.g. as one reads about the model runs used in the National Assessment – page 46, lines 33-38). None of the definitions quoted on pages 4-5 use either the word "forecast" or "projection." Indeed, by the end of the paper, even a full Bayesian decision analysis sounds like it would be included.

The revised draft has clarified the definition we adopt. This definition does not include sensitivity analysis or decision analysis. The distinction between scenarios on the one hand, and projections or forecasts on the other, is subtler and the text discusses it in some detail.

2. It might be wise to note early in the discussion that one of the big problems with scenarios is that while the more detail one adds, the less likely the specific outcome becomes, psychologically the reverse is often the case. See for example:

P. Slovic, B. Fischhoff, and S. Lichtenstein, "Cognitive Processes and Societal Risk Taking," *Cognition and Social Behavior*, 1976.

While I am not familiar with it, I think there is also a literature on participants over generalizing from experience in war games. I have heard Garry Brewer talk about this.

The revised draft discusses this point in detail.

3. Since "forecasts" and "projections" seem to be included, it would be wise to talk a bit about how poor the track record is in engaging in such activities. See, for example, Chapters 3 and 6 in Vaclav Smil, *Energy at the Cross Roads*, MIT, 2005.

The revised draft makes this point.

4. While it becomes apparent over time that the authors recognize the point, early discussions of scenarios and uncertainty (e.g., page 7, lines 22ff) do not make it clear that point estimates cannot have associated probabilities.

This point is made explicitly in the discussion of uncertainty in scenarios. We do not make it here because it appears to be premature to make a technical point

that applies to only one conception of scenarios (i.e., scenarios as time-paths of one or a few quantitative variables) when we have not yet made the distinction between this and other conceptions of scenarios explicit.

5. Page 11, line 6: "then" should read "they".

This is corrected.

6. Page 25, line 9: sea level rise "can be described precisely and compactly" ...be a little careful. While mean planetary rise may be described with a single estimate, there is a lot of regional variability (subsidence, rebound, etc.) that can make even this measure more complicated.

The point that regional sea level impacts depend both on eustatic sea level rise and on specific regional coastal uplift or subsidence is made three paragraphs before the text in question.

7. Page 30, lines 32ff: You suddenly start referring to scenario types by numbers. If you are going to do this you might consider a table or some other easy reference that maps numbers into the long discussions that have come before. Otherwise by this time readers will have lost track (I had). Alternatively, you could use words, since this seems to be the only place in the report that the numbers get used.

This is corrected.

8. Page 36, line 5: ...there is a reference to Shell - which I understand but which I would guess most readers not in this field will not. Perhaps I missed it, but if there is not an earlier description and discussion of Shell's experience, that would be a useful addition.

The revised draft adds the needed introductory discussion.

9. Page 42, lines 26-39: The discussion here is much more restrained than it should be. The SRES scenarios do not contain a consideration of a range of negative outcomes that are at least as plausible as the outcomes considered – Africa or other parts of the industrializing world descend into chaos and lose ground economically and in terms of social development; massive pandemics occur; nuclear war occurs; etc. It is pretty clear to me that the reasons such futures were not considered is that they were deemed politically unacceptable to some governments and other participants in the IGCC process. The imposition of "political correctness" on scenario development strikes me as an issue that deserves much more direct and explicit discussion. The "may" in line 38 of page 42 is far too weak.

The draft makes this point clearly, in criticizing the SRES scenarios and in discussing the importance of future scenario exercises being insulated from

political pressures to consider only attractive futures. Given our focus on providing guidance to improve future scenario practice, however, we find no value in making the criticism of SRES or other past exercises any stronger.

10. Page 43, lines 4-11: my own view is that another reason that the details of the SRES story lines did not get much used is that nobody was able to figure out what to usefully do with all that detail.

The SRES narratives never gathered much detail, and were continually reduced in centrality and importance as the exercise proceeded. We do not agree that narrative scenarios cannot provide value, but the revised draft clearly states the need to improve representation and use of narrative scenarios and their connection to quantitative models, as key needs in methods development.

11. On page 48 lines 16-31: it is easy to misunderstand that the climate data set was an input to VEMAP not a VEMAP output. Also on line 25 some readers may not be sure which "model" is being referenced, since the paragraph has been talking about at least three.

The draft discusses in some detail how climate data is an input to VEMAP, which provides corrected and interpolated climate data as output.

12. Page 49...top of the page: recent literature suggests that the IPCC range has probably been overly narrow. Also, pretty much all precipitation forecasts are dubious.

The comment refers to weaknesses in the climate models used to produce climate scenarios based on the IPCC emissions scenarios. The report does address the issue of uncertainties being added at each stage of the causal chain, which is relevant to these climate-model problems. But because the report is not concerned with climate-model limitations, we do not think these criticisms are sufficiently relevant to make explicitly.

13. Page 49, line 24-5: "three distinct types of scenarios..." I was there and never viewed these as "three types of scenarios..." but rather as three alternative analytical approaches. As noted on the first page, there is an expansion of the term "scenario" to include a variety of activities that others do not lump under this term.

In USNA publications and discussions on the NAST, these were referred to as three types of scenarios. The revised draft clarifies the slightly problematic usage of this term in a footnote.

14. Page 50, line 23-24: I do not believe that the approaches used were "more tractable" but rather that most participants had little or no policy analytic experience and simply did not grasp the concept. See discussion in:

M. Granger Morgan, Robin Cantor, William C. Clark, Ann Fisher, Henry D. Jacoby, Anthony C. Janetos, Ann P. Kinzig, Jerry Melillo, Roger B. Street, and Thomas J. Wilbanks, "Learning from the U.S. National Assessment of Climate Change," *Environmental Science & Technology*, 39, 9023-9032, 2005.

All these factors contributed to the failure to use these other approaches. We do not find it accurate or useful to assign responsibility for the failure exclusively to the analytic teams who were the recipients of the proposed approach.

15. Page 51, lines 3-7: This seems at odds with the extended preceding discussion of SRES.

This section has been extensively revised, with a view to reducing any risk of perceived inconsistency.

16. Page 52: ...reference needed for footnote is above.

This is added.

17. Page 55, line 36 and ff: I have not looked at the UK report in any detail. Early on I had a discussion with the woman who was coordinating it and ventured that opinion that they must be giving a lot of attention to the possible shut down of the Atlantic Meridional Overturning Circulation. She blithely told me that since HadCM2 said it was not going to shut down, they did not have to consider it. However, if indeed they never did, that strikes me as something worth talking about.

This point is correct, and is added explicitly.

18. Page 63 ff...: You might also make reference to the symposium report "Naval Operations in an Ice-Free Arctic," ONR, Naval Ice Center, Oceanographer of the Navy, and the Arctic Research Commission, 2001 April 17-18.

The citation has been added, as an example of potential scenario uses by impacts and adaptation decision-makers.

19. Page 72 ff: The EPA SAB held one of its meetings in Dallas in May of last year and one of the briefings we got was on coastal wetlands restoration. I asked explicitly about whether they were factoring climate change and sea level rise into their thinking (e.g., would some of the investments they were proposing soon get wiped out)? The folks giving the briefing had clearly given it no thought at all, and said they had no resources or ability to consider it. I urged them to collaborate with some universities and explore NSF-CDM money.

The draft makes the point that many decision-makers who on all accounts should be considering climate-change in their decisions are not. The comment reinforces this point, but does not require any changes to the draft.

20. Page 84, line 19: fix grammar.

This is corrected.

21. Page 86, lines 6-19 and elsewhere: I am troubled that some of the discussion here and elsewhere sounds as though you believe there is a "true" distribution for some future variable but we just don't know it. I am also troubled by the apparent acceptance of the need to include second-order uncertainty. Here's an extended quote from the CCSP draft uncertainty paper (quote deleted)

The revisions adopt a more skeptical attitude to second-order uncertainty, and avoid any such implication on the ontological status of probability distributions.

22. Page 87, lines 18-21 same issue: In my view this sentence is not sensible.

The section is edited, including clarifying the sentence in question.

23. Page 88, lines 1-2: same issue.

See the response to point 21 above: The revisions adopt a somewhat more skeptical attitude to second-order uncertainty. (There are differences of view on the promise and value of using second-order uncertainty among the author team).

24. Page 90, section 4.2.3: You might find some relevant discussion in: Casman, Elizabeth A., M. Granger Morgan and Hadi Dowlatabadi, "Mixed Levels of Uncertainty in Complex Policy Models," Risk Analysis, 19(1), 33-42, 1999.

A brief discussion of the argument made here has been added to the section on extreme changes.

25. Page 95: Here and elsewhere why "quantitative probabilities"? What is a non-quantitative probability? For a discussion of the very serious limits (I would say uselessness) of qualitative language used to describe uncertainty see:

M. Granger Morgan, "Uncertainty Analysis in Risk Assessment," Human and Ecological Risk Assessment, 4(1), 25-39, February 1998.

Same issue also lines 1-4, page 96.

While the cited article demonstrates the risk of serious misunderstanding in using informal qualitative language to denote probabilities, there are still many ways of communicating ranges, ordinal relationships, and approximations that can

convey some information. The revised draft provides examples of ways of describing probabilities less precise than assigning numerical values.

26. Page 95-96: If no indication at all is given that a scenario (defined as an interval in the space of interest) may come to pass (i.e., I don't know if the probability is 0.3 or 10^{-7}) what good is it. If (page 96, lines 11-12) there is some probability threshold, its value should be stated, otherwise users will have no idea what to make of it.

There are differences of view on the value and importance of assigning specific probabilities to scenarios (or associated ranges of quantitative variables), within the author team and in the broader community. The draft seeks to advance this debate by identifying specific conditions likely to increase or decrease the validity and value of assigning probabilities. Given the breadth of types and uses of scenarios that we are considering, we do not find a general conclusion that scenarios are useless without specific probability assignments valid.

27. Page 97, line 9: grammar?

This is corrected.

28. Figure 4.4.1: Figure is on its side (x axis is vertical) but then caption further complicates (if orientation is correct, the box will be in upper left corner).

This is corrected.

29. Figure 4.4.2: raises the obvious question – what are all the possible states of the world and views about climate science that could lead to these two distributions. That is an interesting question, but may not be one you want to address in this piece

An interesting question, but not sufficiently relevant to the task of this report.

30. Section 5: Conclusions.

As you might have guessed by now, I do not buy many of the conclusions. Part of this springs from the fact that to me (and I suspect most readers) "scenarios" has a rather narrower meaning than the one that has been adopted (at least implicitly) by the time one gets to page 116. Thus, I start right out being uncomfortable with a statement like "scenarios are required for responsible decision-making on global climate change." Analysis is clearly required. But I think scenarios analysis, as conventionally done, often does more harm than good and I think most readers will take this opening conclusion as a ringing endorsement of conventional scenario analysis.

See the response to comment #1 above. The clarification of our definition of scenarios, and their relationship to various forms of decision analysis and support, addresses this concern.

31. "Robust strategies...do not avoid..." True if you define all projection and forecasting as "scenario-based thinking," but many do not.

See the response to comment #1 above. The relationship between scenarios and various modes of analyzing or evaluating decisions under specified (scenario-based) assumptions about potential future conditions is now clarified in the introductory section on defining scenarios.

32. On rich qualitative storylines...I have yet to see anybody make effective use of all that detail, nor has this paper offered any compelling examples. What all that detail can do is (through availability) result in folks ignoring a wide range of plausible futures that might have gotten them to the same end points on a few specific variables they care about.

The revised draft clarifies the specific uses that such detail can serve, as do several of the sources cited from the security and military scenarios literature (e.g., Brewer 1992). It also states clearly that these detailed narrative scenarios have not served their potential purposes in climate-change scenarios to date. Whether or not they could if handled better is an open question.

Global-Change Scenarios: their Development and Use US CCSP Synthesis and Assessment Product 2.1b

Comments by Dr. Nebojsa Nakicenovic, IIASA

May 31, 2006

Author team responses in italics:

1. Generally the report is very informative, balanced and well written. The main overall shortcoming that it is not clear what the purpose of the assessment is and who are the potential audiences (clients). This should be stated more clearly at the outset. In particular, the front matter on what the scenarios are, how they are used and for what purposes is based on many such similar introductions in IPCC 1995 Evaluation, SRES, TAR WG3 Ch2, MA, etc.

This report, and in particular the opening sections, goes substantially beyond these prior sources in the specificity with which it seeks to distinguish scenarios from related modes of analysis and decision support, and from other types of statements about future conditions. The revisions have sought both to clarify the specific intended users, and make the level of exposition throughout the report more consistent with addressing these user groups.

2. The history of the role of scenarios in climate assessments in general and IPCC in particular is very useful and well presented. The same is true for the general introduction to the process and history of IS92 and SRES.

No response required.

3. However, there are some strong biases and unbalanced presentations of issues in the report particularly concerning SRES. Most concerning is the often anecdotal style of presentation of issues and arguments. For example, on page 43, lines 30 to 35, text is really more in a journalistic style being quite accusative rather than factual. It should be clearly stated which specified values generated implausible model specifications and which participants hold these views. Saying in this context "some participants" and in other places in the report actually attributing unpublished, internal IPCC documents without permission of those concerned is not a good practice (e.g. footnote 78 "Beijing MM notes, Oct 98, pg 2, or footnote 65 "appears that PPP was post processing" which in fact it was not as stated in SRES report.). As only one SRES LA is listed as author, it could be assumed by a reader that all such statements should be attributed to that person, so more precise references and statements are clearly called for. Given that SRES included 80 members of the writing team, it would be good to present both the criticisms and views of others who might not share the same views.

The first passage identified concerns the report's criticism of fixing output target values for harmonized model runs. This section does not claim that any particular model result within SRES was implausible. In fact, the text argues that attempts to discredit a scenario exercise by claiming a particular scenario value is implausible are highly suspect, and any such externally advanced claims of implausibility must pass a high hurdle. Rather, the section is making the more general argument that attempting to replicate specified results using multiple models with different structures requires ad hoc adjustment of internal parameters, or in some cases model structure, and that such adjustments make interpretation of the results difficult and preclude the examination of inter-model variation as an indication of structural uncertainty. This seems an obvious general analytic point, and one of substantial importance for the design of future scenario exercises, which is our main concern. In addition, it is clear from the record of the SRES work that modelers faced substantial challenges in achieving the harmonization targets. The discussion in this section has been revised to stress the general aim of identifying lessons and challenges for future scenario exercises.

The statement that the PPP outputs from the MESSAGE model were achieved by a post-processing of MER-determined outputs has been deleted, as it was tangential to the main thrust of the discussion in the section on the PPP/MER controversy.

The draft has also been carefully edited to eliminate any inappropriate informality of tone.

The concern about source materials used is addressed in point 5 below

4. Another similar example of imprecise and potentially misleading statements is given on page 44, lines 31 to 39. The reader might have the impression that SRES scenarios were reported only at the level of 4 world regions so as to avoid being an "easy target for attack". While it is probably true that the IPCC approval process would be more difficult for country-level scenarios compared with global-regional scenarios, it is not clear at all that higher-level of aggregation avoids being an "easy target for attack" compared to more disaggregated regional resolution. The true reason for such a high level of aggregation was that this was consistent with regional specification of the six SRES models. Actual and higher regional resolutions are available from individual modeling groups, various websites and publications (e.g. RIVM group).

The text retains a general discussion of how the inevitable discrepancies that arise between detailed results produced by global models and more detailed national data can be exploited in a contentious political environment to call the credibility of a scenario exercise into question. The text is revised to note the existence of other persuasive reasons for aggregating reporting, in particular the issue of inconsistent regional boundaries between participating models.

5. As mentioned, a general concern in this context is that much of the material is based on internal documents, private notes and other similar sources (apparently without explicit permission of those concerned).

The aim of this report is to draw on experience to date to identify issues, challenges, and lessons for future scenario exercises. This requires examining current experience with enough specificity to illuminate challenges, difficulties, controversies and their resolution, and areas of weaknesses. Our treatment of sources has sought to balance this need with our aim to avoid gratuitous criticism of worthy past efforts or imposing professional embarrassment. To this end, we have refrained from using materials that could plausibly be construed as private communications – i.e., individual emails – and also from identifying individual parties to particular conflicts or controversies in past scenario exercises. We have, however, made use of internal working communications such as meeting minutes and reports. These cannot reasonably be construed as private communications, since they were circulated to dozens of people. Nor are they confidential government materials – as evidenced by the fact that the complete internal working materials from the IPCC Second Assessment Report have been deposited in the Global Environmental Archives of Harvard University Library for scholarly access. We remain convinced that using these sources is appropriate, and greatly enhances the usefulness and persuasiveness of the report.

6. On a more substantive note, the report is very complementary about many aspects of SRES. For example, it highlights the use of storylines in conjunction with different modeling approaches. However, it also states that SRES fell short of full integration of the qualitative and quantitative scenarios. This can indeed be the case as much more could have been achieved. Nevertheless, from the wording of the report the reader could conclude that this is a serious deficiency and that other scenarios have resolved this challenge much better than SRES. Should this be the case, it should be stated. However, this more likely not to be the case as the scenarios that use both storylines and models are rare. In fact, SRES had a much higher degree of integration than virtually all other studies to date. For example, the MA scenarios were developed a few years after SRES and are characterized by elaborate storylines but rather quite crude link to a single model (that was basically calibrated to SRES scenarios).

Making effective, consistent, and mutually beneficial use of qualitative and quantitative components of scenarios is a major challenge, which no current global-change scenario exercise has adequately resolved and which will be important for achieving progress in the usefulness of global-change scenarios. But the fact that no other current exercise has achieved more success on this challenge than SRES does not mean that there is no basis for identifying this as a significant weakness of SRES. The record from the work of SRES is quite clear that treatment of the storylines and their integration with quantitative models

were persistent sources of difficulty. This is understandable, since the process was attempting something novel and difficult, but it is also important enough for future scenario efforts to merit a discussion. The revised draft retains this discussion, but focuses more strongly on the importance of effective integration of qualitative and quantitative components for future scenario exercises, drawing on current experience of SRES and other exercises to illustrate how hard this is, and providing a few specific suggestions for how to proceed.

7. Another substantive myth propagated in the report is that there has been a canon or some kind of pressure for modeling groups to harmonize scenarios at all costs. This is simply not the case, there are a number of SRES scenarios that have not been harmonized.

The draft does not state or imply that modeling groups were pressured to harmonize “at all costs.” It notes that non-harmonized scenarios were produced and published. The record is clear, however, that modelers were requested to harmonize with the Marker scenarios and much of the analytic effort of the project consisted of people trying to do this – and succeeding in most cases. We remain critical of the prominence given to harmonization in the exercise, and of a seeming lack of clarity about the purposes to be served by harmonizing other model runs with the marker scenarios. Changes in the revised text seek to ensure that three points are clearly made: 1) prominently noting the existence and origin of non-harmonized scenarios; 2) ensuring that the text does not imply that modelers were in any sense compelled to harmonize, and; 3) shifting the main thrust of the discussion forward to identifying and discussing challenges for future rounds of scenario exercises, rather than criticizing past efforts.

8. Last but not least, there is a wide range of conditional convergence across the set from high in A1 to very low in A2. Nevertheless, it is true that SRES does not include a scenario with conditional divergence (but does of course in terms of absolute income). It ought to be stated also in this context that IPCC included such a mandate in the terms of references.

The text has been revised to note this variation between SRES scenarios more prominently, while still noting that all scenarios assume enough income convergence that even the poorest regions pretty much solve their development problem, with real per capita income rising to well above \$5,000 – i.e., scenarios without any income convergence, and other forms of plausible but undesirable future, were not considered. This requirement did not appear explicitly in the final form of the terms of reference, although the SRES process was under clear instructions to do this, in response to the Parikh critique of the IS92 scenarios. The text does note this direction imposed on the process, and the revisions shift the focus to the future problem of the need to consider undesirable futures in some global-change scenario applications.

9. In sum, the draft report a major step forward in assessing the goal-change scenarios and it is precisely because of this valuable contribution that it would be of paramount to improve on at least some of the deficiencies outlined in this review.

No response required.

Global-Change Scenarios: their Development and Use US CCSP Synthesis and Assessment Product 2.1b

Comments by Prof. John Robinson, UBC

May 31, 2006

Author team responses in italics:

General Comments:

In many ways, this is an excellent report, as might be expected given the authors involved. In particular, perhaps reflecting my own interests, I found the discussion of the SRES process to be fascinating and informative, and the discussion of issues and challenges in section 4 and conclusions in section 5 to be challenging and provocative.

I do however have some serious concerns about a number of the arguments being presented. These have mainly to do with the discussions of uncertainty, likelihood, and the purpose of scenario analysis on the one hand, and the discussion of the use of scenarios by decision-makers on the other. Both sets of concerns are closely connected to more general issues about the role and status of scientific knowledge and its use in decision-making. In other words, I believe that at least some of my concerns derive from questions at this more general level of philosophy or social studies of science.

I mention this partly to provide some context for my subsequent comments but also because I think it is quite relevant to the subject-matter of this report. I think it is important not to convey the impression that some of the most fundamental assumptions underlying this kind of analysis are uncontroversial. There do exist fairly deep differences across disciplines and philosophical schools of thought about issues related to interdisciplinary understanding, the role and status of science, the science/policy relationship, etc. As I think my subsequent comments will make clear, these play out at very detailed levels of application with respect to scenarios and modeling related to energy and climate change issues, and find very concrete expression in many of the questions discussed in this report. To give only one example, I think the discussion of SRES, and also the general discussion of the merits of attaching probabilistic judgements to scenarios, reflects a particular view of the role and status of scientific knowledge which is itself at play in the policy debates that the SRES scenarios contribute to, and also in the modeling and scenario analysis methods used.

I don't want to over-emphasize this point. I am not suggesting that there are irreconcilable and unbridgeable philosophical differences that make any general judgement impossible to reach. But, as argued in more detail below, I do feel that in a number of places this report takes a particular philosophical position, without acknowledging it, which strongly colours the analysis and the conclusions reached. And I believe this approach is somewhat at odds with the views held by a significant portion, and perhaps the majority, of scenario analysts themselves, at least in certain fields. My

comments will speak from, and to, this somewhat different viewpoint, and my only suggestion is that this alternative perspective be somehow acknowledged in the report.

To put this viewpoint most generally, I believe that there is a strong principled argument in favour of approaches to scenario analysis that are explicitly normative, and a related argument in favour of holding to a non-probabilistic approach to scenario analysis. This in turn is connected to certain views about the use of scenarios and the nature of appropriate participatory processes. This is not purely a theoretical argument. In fact it began in the 1970s as a set of practices that were developed in conscious opposition to the then dominant predictive forecasting approaches in the energy field. So there has been built up quite an extensive applied literature in this area. I think that both the theoretical arguments and the applied work could be better reflected in this report.

These points are repeated, and addressed in our responses, under specific comments below. The general response is that the revised text has made the treatment of normatively derived scenarios – typically scenarios that define targets, which are subsequently analyzed for conditions of feasibility, requirements, costs, etc. – more extensive and more consistent, and provided some discussion of the conditions and uses for which this approach might be preferred. The revisions have also qualified the argument about assignment of probabilities to note that this is not appropriate for scenarios that are stipulated as goals or targets. We do not, however, accept the claim that this alternative approach to scenarios is generally preferable for all uses and applications.

Specific Comments (keyed to pages and lines in version 7.1, Mar 28, 2006)

1. 2: 9-16. While it is of course true that much decision-making focuses on short-term issues it may not be that the risk of “error” increases with the planning horizon, nor that “error” is the best concept here. If uncertainty does increase with the time horizon of analysis, is it not enough to say that the range of choices and relevant factors expands, thus increasing the need for some organized way to think about the future?

While the revised text no longer uses the concept of error here, we find it hard to imagine cases where uncertainty does not increase with time. We agree that tools are needed to manage this uncertainty, particularly tools that make the existence of uncertainty explicit and resist, rather than exacerbating, the widespread tendency to underestimate uncertainty.

2. 2: 23-9. I have some trouble with the hierarchy here. Surely there are lots of long-lived social phenomena (religion, marriage), and lots of short-term variability in physical, chemical and biological phenomena. It is true that we tend to think of natural scientific *laws* as unchanging (though in fact our views of them have changed rather a lot in the last few hundred years) but the phenomena they describe are of course very variable. And presumably people will still be interacting, socializing, creating institutions, etc. in the future (i.e. we have fairly

reliable general knowledge about social practices). I am not sure that we have more knowledge about specific future physical, chemical or biological events than about specific social ones. I think that Holling's distinction between fast and slow variables (in all realms) might be a more useful approach to this question.

The statement is not about the stability of phenomena, or whether they operate on fast or slow time-scales, but about the confidence of our knowledge about underlying and enduring causal processes. As one example, we can predict insolation at 60N in 10,000 years to an accuracy better than 1%. This fact will have a significant impact on the climate 10,000 years hence. We are unable to think of any claim that could be made about social systems with remotely comparable confidence.

3. 2: 31-40. Again the implicit hierarchy is a bit problematic. My guess is that it wouldn't be hard to find natural, biological and social science examples in each of the three categories described here.

Perhaps I am belabouring what is not an important issue but I think the hierarchical conception of human knowledge expressed in this paragraph may be one of the reasons for the focus on probabilistic approaches to scenario analysis that is defended so strongly below (e.g. pp. 81, 88-9). Alternative epistemological orientations might give rise to a different view on that topic.

The hierarchy stated here is not normative, but concerns degrees of confidence in knowledge of causal processes.

4. 3:4-19. This discussion of the use of scenarios in the climate change literature might be usefully amplified by a brief discussion of the precursor energy field, where there is a very rich tradition of the use of scenarios. Some of this work carried over into the early IPCC work.

The connection has been noted in several places in the revised draft. In view of the already excessive length of the report, we do not believe a more detailed treatment of the earlier energy work would be justified.

5. 4:3-6. The use of the language of "scientific inference" is again suggestive of a particular orientation. This statement appears to suggest that such collective judgements are of a lower order than scientific inferences. Of course we have several thousands of years of humanities scholarship based on collective, or even individual, judgement. Is scientific inference what we need or want in order to assess the production and use of scenarios? I would have thought that judgement might be a rather critical component of such assessment.

The text stresses repeatedly that that creating and applying scenarios necessarily involve judgments, and cannot be done through mechanical application of scientific inference.

6. 4: 33ff. Having recently published a paper with Rob Swart and Paul Raskin (“The problem of the future: sustainability science and scenario analysis”, *Global Environmental Change* 14 (2004) 137–146), that provides yet another definition I can’t resist quoting it here: “In the context of sustainability science, integrated scenarios may be thought of as coherent and plausible stories, told in words and numbers, about the possible co-evolutionary pathways of combined human and environmental systems. They generally include a definition of problem boundaries, a characterization of current conditions and processes driving change, an identification of critical uncertainties and assumptions on how they are resolved, and images of the future. The characterization of the nature of human and environmental response under contrasting future conditions is key in scenario formulation. Reflecting respect for the uncertainty inherent in such systems, scenarios are neither predictions nor forecasts.”

This is not very different from what is in the quotes you included, but it introduces some themes that you focus on in the report. I think the paper provides a bit of background to some of my comments below.

You might also want to look at the 1999 NAS report *Our Common Journey* for some compatible arguments.

The revised draft takes note of the use of scenarios in the 1999 NAS report. We think that the set of definitions included already makes our intended points about the commonalities and diversity of definitions well enough without the need to add others.

7. 6: 19-27. I was expecting here to see some discussion of the Shell tradition and also the fairly large energy scenarios literature of the 1970s and 1980s. Note that parts of that tradition focused strongly on backcasting approaches (see comments below). BTW, Brewer and Shubik, 1983 is not in the list of references at the end.

Discussions of the Shell approach, and some references to earlier work in energy, have been added, as has the Brewer and Shubik reference (which is actually 1979, not 1983). Note, however, that references are still not complete in this draft.

8. 7: 22-33. This might be a good place to discuss the vexed issue of base case (“non-intervention”) and intervention scenarios. Thought I think that such approaches are extremely problematic, because they privilege the base case in inappropriate ways, they have nevertheless been common. In some ways, by arguing for multiple baselines, SRES took a position in conscious opposition to this approach, which dominated the early climate change mitigation scenarios. Of course in other ways SRES reinforced that approach since the requirement that the SRES scenarios not include climate policy virtually guaranteed the creation of the

post-SRES “intervention” scenarios. I think this whole story needs some discussion.

The fundamental issue here has to do with the meaning of a baseline scenario. As you note later on, SRES explicitly renounced predictive language, and you take issue with that approach. However, it might be worth introducing here the underlying methodological issue of the role and status of the concept of baseline scenarios, since this is relevant to that discussion.

The difficulties in defining baselines coherently are discussed extensively in the sections on the relationship between scenarios and decisions. In view of the already excessive length of the report, we do not believe a more detailed discussion would be justified.

9. 7: 35-46. This paragraph starts to get to the heart of my concerns about the way scenarios and uncertainty are described in this report. The paragraph begins with a statement about “confidence” and goes on to argue that scenarios must necessarily imply claims about likelihood. I would want to avoid the language about confidence and provide a slightly different focus for the discussion. To me the claim is less about likelihood than about feasibility. These are of course related but I think the distinction is important. If I say a course of action is feasible I am not claiming it is likely. So feasibility is a different claim than likelihood, more related to plausibility. I think it is also a more fruitful way to think about scenarios, for reasons that I hope will become clear in later comments.

I am a little surprised that there is no discussion here at all about backcasting or explicitly normative approaches to scenarios analysis. The report makes some quite useful arguments about normative approaches later on (e.g. page 117) so it would be useful to note here that there is quite a large tradition of such analyses in the energy field going back to the mid 1970s. Having published six or seven journal articles on this phenomenon from 1982 to 2003, arguing the value of such an approach, I am of course not disinterested but, given the extent of this history, I think it is reasonable for it to be described. It also, of course, speaks directly to many of the methodological questions discussed in this report.

A discussion of normatively derived or target-based scenarios has been added in several places throughout the revised draft. We do not, however, understand the proposed distinction between likelihood and ‘feasibility’ or ‘plausibility.’ We read all these terms as synonyms for relative subjective probability. Absent some supernatural ability to see the future, what could be meant by a statement that one scenario is feasible and another infeasible, or one plausible and another implausible, but that the first is judged more likely than the second?

10. 8: 20-29. Given my previous comment I would add one major choice to this list of major choices in scenario development: the question of whether the scenario is intended to be predictive (not a good idea for reasons you discuss but still not

uncommon), exploratory or normative/goal-oriented. This could be rolled into your category “questions to be addressed” but I think it is important to specify it explicitly. Not only does this choice strongly affect the focus of the analysis, but it has significant implications for the kinds of models that can be used (see comments below).

This distinction is added as an explicit design dimension of scenarios.

11. 9: 1-8. This discussion presents a fairly linear view of the science/policy relationship. How about a purpose related to social mobilization, or the development of a political constituency for change of various kinds. Of course such a goal is sometimes best fulfilled by normative backcasting analyses. Omitting this purpose here leaves out a major role that scenarios have actually played in, say, the energy debates of the past three decades. There is an interesting literature on this (for one reference see my comment on p. 104, below).

This is accommodated to some degree by the expanded treatment of normatively motivated scenarios in the revised draft, and in the discussion of uses of scenarios in pluralistic policy debates.

12. 11: 4-26. I think it is important to connect the narrative question to the issue of modeling. As you discuss at length with regard to SRES and other projects later in the report, this is a crucial nexus. An important point here is that different types of models are better or worse able to address different components of narratives. This in turn connects back to the issue of the use of scenarios. Certain kinds of models lend themselves much better to certain purposes. Macroeconomic models based on econometric calibration or general equilibrium principles are predictive by their very nature and thus are only awkwardly connectible to scenario analysis focused on exploring alternative futures and still less suited to backcasting analyses. That is why many of these kinds of analysis have used input-output-based economic modeling instead. I think this report would benefit from a discussion of the connections between different types of models and their implications for scenario analysis.

We do not agree that the connections between specific types of models and alternative types of scenarios are as well developed or understood as the comment suggests. The report does stress the importance and difficulty of achieving consistency and integration between qualitative and quantitative elements of scenarios, but principally identifies this as a challenge for research and methods development, not an area in which current experience indicates any clearly viable approach.

13. 12, section 2. I like the use of Figures 2.1 to 2.6 to organize the discussion. However, these figures are very linear and uni-directional. Figure 2.2 shows a more appropriate circular process but is not used to organize the discussion, In

any case I would have thought the IPCC TAR SYR Fig 1 is a more useful way to convey the non-linear nature of the relationship among these categories.

The crucial point that needs to be made, I think, is that emissions, mitigation, impacts and adaptation are all rooted in underlying socio-economic conditions. This comes up in section 4.6 and it would be useful to lay the groundwork for that discussion here.

Another general point is that as the knowledge moves along the chain from socio-economic conditions to emissions to climate processes to impacts, there is an interesting sociological phenomenon among the modelers and analysts. My experience is that everyone in the chain wants simplicity and parsimony at the input end but wants also to produce complexity and multiplicity at the output end. Climate modelers want only one or two emission scenarios but tend to produce multiple climate scenarios. Impacts people would prefer only one or two climate scenarios but multiply their impact analyses, etc. This tendency may be connected to, or at least intersect in interesting ways with, Mackenzie's uncertainty trough argument: analysts are least knowledgeable about and interested in (and therefore more credulous about) the complexities of work several disciplines over.

The point of these figures is to illustrate the simple assumptions of causal relations normally made in scenario-based analysis, and to contrast these with the more complex form used in IA models that actually attempt to represent all the important interactions of the climate issue. The figure used in TAR is less suitable for our purposes than the one we have drawn from the SAR, precisely because it abstracts away from many of the specific causal linkages that IA models aspire to represent. The interesting phenomenon described is broadly consistent with our discussion of the challenges of producing scenarios that are useful to some specific audience, although not entirely consistent with the experience of the scenario exercises we review.

- 14, 14: 6-7. This is a very interesting point. I didn't notice it being picked up in later discussions.

This point is discussed extensively, in Sections 2.1 and 4.1 of the reorganized draft.

15. 17: 10-45. This description seems at odds with my knowledge of emissions scenarios based on modeling, and also at odds with your later discussion of the SRES process.

This material is deleted in the revised draft.

16. 18:30-33. This is of course a description of a backcasting analysis. I am at a bit of a loss why the connection is not made.

We agree. The connection is made in the revised draft.

17. 19: Fig 2.4. Shouldn't the arrows between the first and second, and second and third ovals be reversed?

No. The assumed direction of causation is still forward. The different shadings of the ovals, which denote the part of the causal chain on which the use of the scenario is focused, are reversed between Figures 2.3 and 2.4.

18. 28, section 2.6. Again, the social mobilization and political uses of scenarios are ignored. I think this typology of decision-makers is much too limited and narrow. Where do I put the City of Vancouver manager who is directing a mitigation program and starting to try to develop an adaptation program? Where are energy policy-makers, who are often in different ministries from environmental policy-makers concerned with climate change? Much of my work with the IPCC this time around is about looking at linkages among mitigation, adaptation and sustainability. These kinds of linkages are not contemplated in this typology.

(This material now appears in Sections 2.1 and 4.1). The purpose of the typology is to provide a clear, compact representation of the most important classes of climate-change decisions and decision-makers, not to provide an exhaustive list. The revised draft does note that some sub-national officials are engaged in mitigation decisions. The Vancouver manager you describe has two distinct tasks. In directing a mitigation program they serve as "energy resource and technology managers." In their capacity as organizer of an adaptation program, they serve as an "impacts and adaptation manager." While both tasks involve climate change they are distinct, as Vancouver's emissions play essentially no role in determining the climate change Vancouver will experience and the impacts it will have to adapt to. In this case, reducing the complexity of linkages, and thereby clarifying responsibilities might make for better decision making. Much of the work of the IPCC may be about looking for linkages between mitigation and adaptation, but it is far from clear that emphasizing those linkages is likely to produce better decisions.

19. 31: 13-25. While clearly you had to limit the scenario project you considered, it seems to me that the Global Scenarios Group work, and the UNDP et al World Energy Assessment scenarios might have been as or more relevant than some of the ones you included in section 3.5 to 3.9.

These would have been worthwhile to consider, as would several others, but limits of time, resources, and report length have precluded our doing so with more than brief references. We have briefly noted the GSG exercises, in the context of the revised report's expanded discussion of normatively based scenarios.

20. 39: 14-15. This is one of the few references in the whole report to the post-SRES analysis. I would have thought that deserved a bit of discussion since it illustrated

so clearly the issues involved in making the SRES scenarios “non-intervention” scenarios. The post-SRES summary findings in the TAR are also relevant to some of the later discussion, and could usefully be added to the list on lines 22-35.

The problems of defining SRES as non-intervention scenarios are in our view adequately illuminated without an explicit, separate discussion of the post-SRES scenarios. These are discussed briefly, but a more detailed treatment is precluded by limits of time, resources, and report length.

21. 40: 32-42. Here what I think of as the probabilistic bias of the report shows itself more explicitly. I will respond at more length to this argument below but want to note here that this slant on SRES is not universally shared and colours your interpretation here.

The revised report substantially expands the discussion of the reasons for and against explicit probability assignment, and the conditions under which it is more and less desirable. This reflects explicit discussions within the group and consideration of evidence from the SRES experience – so to the extent that there is bias present, it is not the sole foundation for the conclusions, and is moreover not shared by the entire author team, as we had a vigorous discussion of these points.

22. 42: 9-12. I agree that limiting SRES to convergence futures is methodologically problematic but this raises an interesting point about the purpose of such scenarios. Is it to lay out the range of likely futures (as you implicitly suggest throughout and argue explicitly below), of plausible futures, or of desirable futures? Different answers to that question might give different answers to the utility of this convergence condition.

*The stated purpose and mandate of SRES was **not** to produce pictures of attractive futures, but the condition of income convergence was nevertheless imposed on the activity based on the perceived need to respond to normatively based criticisms of the IS92 scenarios. Consequently, while the revised report accepts the potential value of scenarios produced explicitly for normatively based reasons, we find this aspect of the SRES experience to be an example of the confounding of normative and positive bases for scenarios that makes their interpretation and use deeply problematic.*

23. 44: 7-18. This discussion seems a bit odd. Surely all scenarios, without exception, include implicit assumptions about policy. There is not such thing as a policyless scenario. The only issue is which kinds of policies are made explicit, which are implicit, and which are deliberately excluded from the scenario (if any). (You return to this issue on p. 112, where you make a point similar to what I am suggesting here, I think.) So the argument that the decisions made in the low emission SRES scenarios were not policy-related is not wrong in principle, though it may indeed be implausible in practice, depending on the specific decisions.

The charge of SRES was to explore how CO₂ emission might evolve in the absence of explicit policy intervention to reduce emissions. This is not an intrinsically incoherent requirement. The problems were rather 1) that the “no intervention” assumption was not defined carefully and consistently enough – it should have included explicit specifications re how to treat already enacted or committed policies, policies with other motivations that affect GHG emissions, and degrees of implementation and compliance with announced policies; 2) that some SRES scenarios resisted the mandate to assume no incremental policies, producing low-emissions futures that are only minimally plausible absent additional policies explicitly targeted on reducing GHG emissions.

24. 45: 11. It would be nice to see some summary and conclusions to this section.

We agree. These are now provided.

25. 45-58. It would be interesting to compare the Canadian Climate Impacts Country Study to the US and UK studies. It was quite different from either.

We reviewed the publications of the Canadian impacts study, and do not find enough methodological differences to justify the additional length in what is already an overlong report.

26. 75: 29-40. I think these conclusions are crucial ones that do not get picked up in sections 4 and 5 as strongly as they should be.

We agree. These are now treated more extensively in sections 4 and 5.

27. 31-79. Reading section 3 made me think of our QUEST work. Six Canadian cities have now built a QUEST and several of them are planning major use of it in public information processes. The US Department of Transport recently ranked QUEST first among the 82 (I think) urban planning models it assessed. While it is of course not a global scenario analysis tool (though it contains global scenarios within it which constrain the regional scenarios that users create), it is a scenario analysis tool intended to address many of the same issues as the projects you describe. My point here is simply that QUEST embodies a different approach to scenario analysis than any of these studies, one in which the participants or users, not the research team, create the scenarios, in which this is done in groups of 15-20, and in which these scenarios are backcasting scenarios that express the users preferences about future outcomes. It seems to me that this is a quite different approach to scenarios than that discussed in this report. It is a form of participatory integrated assessment (PIA), which of course is happening in many places in Europe, and is the subject of quite a vigorous literature. I would think that some discussion of the PIA tradition and activities would not be useful in this report.

In fact I was surprised not to see some explicit discussion of integrated assessment modeling in this report. PIA represents an offshoot of that tradition which speaks particularly strongly to some of the issues discussed in sections 4 and 5.

The principal area of connection between PIA and scenarios concerns precisely how and how much users are involved in the creation of scenarios. The report discusses this issue extensively. In view of the already excessive length of the report, we do not think any further discussion of PIA would be justified.

28. 81: 2-8. I would take a different tack on the consistency question. At one level, if scenario assumptions are inconsistent then the scenario is impossible (i.e. self-contradictory). If the inconsistency is less absolute, then one could say that inconsistent scenarios are infeasible or implausible. No relative claims of likelihood are required. The probabilistic approach taken here leads to arguments about error and bias that don't seem to capture what is at issue.

See response to point #9 above. We are unable to distinguish between statements of degrees of 'plausibility' except as statements about subjective probability.

29. 81: 40-46. This question carries over into the discussion of the complexity of the scenario. If the question has to do with possibility, feasibility and plausibility then it is not obvious that more complex scenarios are less useful. I would have thought that uncertainty is somewhat fractal, and is rather large at all scales of analysis. Does adding more "reality" really increase implausibility? I could imagine the opposite: adding complexity may make the scenario more plausible. I think the focus on likelihood is constraining the analysis here in unhelpful ways. (cf. my comment on p. 92 below)

We do not understand what it means for uncertainty to be 'fractal'. Adding complexity in the specification may well make it appear more plausible (that is, subjectively likely). This is a misunderstanding, however, perhaps reflecting a systematic cognitive bias, since more specificity or complexity must decrease the likelihood of that specific scenario occurring unless the conditional probability of the added conditions or complexity is 1. Where such cognitive biases are known or suspected to exist, it is important that the scenario architects be aware of them and structure their products so as to best communicate uncertainty.

30. 82: 22-43. Excellent points. Note that backcasting gets relegated to a footnote.

The treatment of normatively derived scenarios and backcasting is increased throughout the draft.

31. 83: 1-44. Excellent points.

No response required.

32. 84: 1-21. Ditto.

No response required.

33. 87: 20-39. Ditto.

No response required.

34. 88: 40-46 and 89: 1-3. Never generate an odd number of scenarios, since users will see the middle one as most likely. Have others said this? I have been saying it for years about our QUEST work but haven't heard it expressed by others. But it seems likely to be a common insight. Note that you later provide an argument (with which I disagree, see comments on p. 106, below) in favour of this view that the middle is more likely than the "upper" and "lower" scenarios.

We believe this rule of thumb is widely known among scenario producers and modelers. This is not incompatible with a judgment that when several scenarios span a range in some variable, the middle of the range is normally presumed to be more likely than the ends (or rather, sub-intervals that lie near the middle are judged more likely than sub-intervals of equal width that lie near the endpoints).

35. 89: 7-15. It seems to me that the issue of extreme scenarios is quite different in principle from the issue of bifurcations. The former can happen without any bifurcations just because of the continued high or negative growth of a key variable or two. And the latter may not lead to extreme outcomes. So I wouldn't reduce the bifurcation issue (which I think is a critically important one) to questions related to extreme scenarios. Bifurcations raise questions about irreversibility and foreclosure of opportunities, which are of critical practical importance. (Cf. comments on p. 106)

The discussion applies to extreme outcomes, which can arise either through discrete qualitative changes in underlying logic, or through the accumulation of incremental events all tending in the same direction. The problems posed for constructing and using scenarios are similar in these two cases, so we do not think they need to be distinguished explicitly.

36. 91: 4-44. Excellent points.

No response required.

37. 92: 8-18. We are back to the complexity = unlikelihood argument. If formal uncertainty reasoning indicates this is the case then this seems to me to provide some reason to question the applicability of such reasoning in this context. Think of storytelling. Is a more complex story a less plausible one? I don't see why that is necessarily the case. Even if we restrict ourselves to likelihood, one needs to

distinguish the level of analysis. More complex scenarios are indeed less likely in that there are more specific details that can be wrong, but this does not obviously mean that the scenario as a whole is more or less likely. The simpler scenario, if expanded to a comparable level of complexity might be even less likely.

See responses to points #9, 28, and 29 above. While it is true that in comparing two distinct scenarios, the one with more detail need not be less likely, nevertheless any addition of incremental detail to a given scenario must decrease its likelihood. High rates of economic growth (defined as some range of growth rates) and a high rate of technological innovation (defined equivalently) must be less probable than either condition alone. Users may perceive the combined, more detailed scenario as more likely, but if they take actions based on this misinterpretation they are unlikely to make good decisions. It is the responsibility of those who produce scenarios to anticipate and guard against such misinterpretations.

38. 93, section 4.2.5. Here we get to the heart of the probability question. My general comment would be that the conclusions reached here are predetermined by the general approach to this question which, as noted in previous comments, is made clear early on in this report. But it is not the only possible position on these issues.

We do not think this is the case. In increasing the detail given to normatively derived or target scenarios, the revised draft acknowledges that the arguments advanced for explicit probabilities in scenarios do not apply to these. In addition, even for scenarios intended to have some degree of predictiveness, the revised draft has expanded and qualified the arguments for explicit use of probabilities. We do not believe that our arguments and conclusions were pre-determined by our starting assumptions, particularly because these points reflect the results of vigorous discussion among the authors.

39. 94-5. I think the arguments against attaching probabilities to scenarios, or at least to scenarios of the type represented by the SRES work, are very powerful. You mention three principled arguments. On the first, see my comments below on p. 94, ll. 34-45. On the second one, I agree with your argument that different scenarios can represent very different worlds, and the difficulty of assigning boundaries is not crippling to the assignment of probabilities. Your third argument about whether it is useful to assign probabilities seems a bit disingenuous to me. You are happy to say elsewhere that the analyst are best able to make critical judgements about the probability of scenarios and should not leave this to the users. But here somehow the analysts' judgements about what is worth doing are not derminative. But obviously such judgements are and must be rife throughout the analytical process. You note that analysts may judge that simply passing some probability threshold (i.e. be judged as feasible?) is enough, but argue that this is only legitimate if users don't want more. But surely a judgement of this kind is more not less important if users are asking for more. The fact that users would like certain kinds of analysis is not a reason to provide it if in the judgement of the

analyst this would not be meaningful to do, or would provide invalid or misleading results.

The relationship between those producing and those using scenarios will differ strongly among cases. Where feasible, the report argues for intensive engagement of users or their proxies in the process of scenario creation, and notes that under these conditions the case for explicit articulation of probability judgments is less compelling. We do not argue that analysts should make probability assignments instead of users. Rather, we argue that probability assignment is most desirable when users are too numerous and diverse to be closely engaged in the process of scenario creation. In addition, we argue that in some cases, attempts by users to articulate their own explicit probability judgments may be much more useful than any such assignment by analysts or modelers. We strongly agree that analysts should NOT provide analytical products that they believe are not meaningful, however much someone is asking for it. Similarly, they should avoid providing analytical products in a form that is likely to lead to misinterpretation by users.

40. 94: 10-12. Being explicit about probabilities does indeed organize a certain kind of knowledge and make risk assessment possible but this is not very useful if it is based on spurious precision, or if these probabilities are not the question that the scenario analysis is asking. I am not sure that “sophisticated decision-makers” do actually need probabilities. I think all kinds of decision-makers make important decisions without specification of probabilities, and it is not obvious that such specification always improves the nature of the decisions. Finally, I am also not sure that the analysts are the best able to make such probabilistic choices, especially about complex value, political or social questions.

See response to comment # 39 above.

41. 94: 34-45. This distinction between frequentist and Bayesian approaches is a critical one but what is left out here is a third position, which has found its way, for example, into the guidance notes on uncertainty prepared for the IPCC 4AR. This is that socio-economic scenarios address questions of choice and intentionality that cannot usefully be addressed either by frequentist approaches based on likelihood (typically used in the TAR by WGI authors) or Bayesian approaches expressed in terms of confidence levels (typically used in the TAR by WGII authors). This is why, it has been suggested, WGIII authors in the TAR typically did not use either approach. As far as I can tell, you raise the issue of agency and choice only once (on page 106) but use it for an entirely different purpose.

Of course this third approach, which is also treated at some length in various chapters in the Rayner and Malone volumes, underlies the non-predictive exploratory and backcasting approaches to scenario analysis discussed above, and the first principled objection to adding probabilities to scenarios that you mention

on p. 94. Adding a discussion of it here would help to contextualize many of the issues I have tried to raise in these comments. It is discussed in the Swart et al paper referenced above, and in many other papers on scenarios and futures studies.

I believe that this third approach to uncertainty in turn provides a more robust basis for the SRES position that is argued against on pages 93-6 of the report. In that context, I would like to suggest that the unsupported allegation in lines 10-12 of page 95 is a bit unworthy.

We agree regarding the inappropriateness of using probabilities to characterize scenarios constructed as targets based on normative considerations, and have modified the treatment of this type of scenarios in the text. But if this comment is intended to apply more broadly than to this type of scenarios, it is not proposing a third approach to uncertainty, but rather a claim that in socio-economic domains uncertainty is not an appropriate way to think about alternative possible futures. Other writers have advanced this claim on the basis of “reflexivity” – i.e., the proposition that because socio-economic futures contain elements of human choice, probabilistic characterizations are fundamentally incoherent because these characterizations will themselves influence the choices that determine what futures are realized. We disagree with this argument, and have provided supporting discussion in the revised draft in Section 4.6.5.

*The claim cited on page 95 was **in no way** intended to suggest deceptiveness on the part of the SRES. Rather, it sought to make two general points: 1) Prominent reporting of more detailed results produced by global models would inevitably raise discrepancies with national data that are more detailed and in some cases superior; 2) Within a contentious political environment, such discrepancies, although unavoidable, can be exploited to call the credibility of the exercise into question. We still believe these observations to be correct, with significant implications for global-scale scenarios exercises conducted in a political environment. Nevertheless, the former text failed to make clear that in addition to avoiding such discrepancies (which itself does not indicate any attempt to deceive), there are other good reasons for aggregating reporting – in particular, inconsistencies among participating models in how they define the boundaries of smaller-scale regions. The text has been revised to highlight this other possible reason, and to make clear that we are making no suggestion of impropriety, while still making the two general points above.*

42. 99: 26-45. Contrary to the arguments made here, I think it is quite possible to have large numbers of people involved in scenario analysis exercises. In our Georgia Basin Futures Project, for example, we ran dozens of QUEST workshops involving hundreds of people. Our video-based Science World QUEST ran twice a day three times a week in Vancouver’s science museum for about two years, attracting about 18,000 users. And the City of Calgary is on record as saying that

they would like 100,000 Calgarians to play Calgary QUEST. So large numbers of users are possible to imagine. And remember, QUEST users actually create their own scenarios, thus learning something about the trade-offs and consequences involved in different policy choices.

These huge numbers of participants are possible due to two simplifying characteristics of QUEST. First, scenarios are generated by individuals or groups interacting with a computerized model, regional database, and interface. Consequently, the numbers actually collaborating to generate scenarios on each occasion are substantially smaller than these aggregate user statistics would suggest. Second, the QUEST system imposes a highly restrictive structure on the causal modeling embedded in the scenarios. A single global context is chosen from a few possibilities, eliminating any uncertainty in subsequent relationships. And once global context is chosen, a single deterministic modeling system maps a highly detailed and specific set of policy and development choices onto dozens of specific consequence measures, many of them spatially referenced. While this approach allows great enrichment of scenario-based activities on some dimensions, it greatly restricts them on others. The revised draft has noted the possibility of greatly expanding participation using such systems, and has cited both the QUEST and POLESTAR systems as examples.

43. 100, section 4.4. I think this is a critical discussion, but it goes beyond issues of graphical representation. In our current CIRCUITS project, we are looking at how best to use landscape visualization, information visualization and adaptive interface design to better convey complex multi-dimensional scenario information to non-expert audiences.

The revised draft identifies other forms of visualization as well as graphical and tabular formats.

44. 104: 23-46. It is nice to see some discussion of backcasting-type analyses here but I see two problems with the way this discussion is presented. First, it is implied that the injection of normative content into the content of scenarios is necessarily a bad thing (“political sponsors” may “seek to inject normative concerns” into scenarios (ll. 23-5). While this is consistent with the arguments made elsewhere about the scientific nature of scenario analysis, such a traditional approach to what used to be called the “fact-value” debate is not universally shared. Second, the somewhat grudging acknowledgement of the value of normative analysis at the bottom of the page implies that such uses are distinct from the use of scenarios for strategic planning, risk analysis or assessment. I would think that such normative analysis can be quite useful for all three.

In the next page there is concern expressed about the potential for scenarios to be used for political purposes. Our book *The Politics of Energy Forecasting* (Oxford, 1987), contained 7 or 8 country cases studies of the way energy forecasts were used in each country. The general finding was that in each country

decisionmakers used energy forecasts to provide ostensibly scientific justification for decisions made for other reasons. One can assume that this continues to be the case. I would suggest it is a normal part of the science/policy process. Note that this is more easily done if the scenarios involved are claimed to be value neutral and objective. As I have argued in several papers, trying to make it more difficult for scenarios to be used in this way was one of the motivations behind developing explicitly normative backcasting approaches to scenario analysis. This leads to conclusions rather different from those in this report about the effect of explicitly normative scenario methods.

These points have been addressed in the revisions.

45. 106. For the reasons given earlier, I would challenge the arguments on this page. There does exist a literature on “self-altering prophecies” that suggests that forecasts can often give rise to countervailing affects. But there is no guarantee that this will happen. And one of the points of the bifurcation argument is that there may be turning points and irreversibilities that give rise to powerful positive feedbacks. As I recall, Jean Charles Hourcade has written about this with respect to nuclear power in France.

See response to comment #42 above. We have addressed these arguments in section 4.6.5.

46. 111: 2-13. While the use of scenarios to support climate change mitigation decisions may not yet have been frequent, there is a huge history and literature about the use of energy forecasts and scenarios in energy policy decisionmaking.

And in many other domains. Yet it is still striking how little scenarios have yet been used for concrete decisions related to climate change.

47. 112: 20-44. This is a very important discussion. Separating the decisions that can be affected from those which cannot is a large part of the art of scenario design and also the linkage between scenarios analysis and decisionmaking. In QUEST, for example, the user is asked to predict which global scenario is most likely to happen, precisely because local decisionmakers cannot influence global decisions. That choice being made, however, all subsequent decisions are choices as to what outcomes the user would prefer to see, since the regional variables in QUEST are amenable to policy choice at the regional level.

The comment does not explicitly advocate our adopting or endorsing the approach to uncertainty in QUEST, but if this is what is intended we have significant reservations about doing so. Because producers of the scenarios (or rather, the scenario system) cannot precisely specify probabilities of alternative global outcomes, they instead present only a list of potential global-scale futures with no likelihood information attached. Users are then asked to select one that they judge to be most likely, and all subsequent analysis is conditioned on the

assumption that this global scenario applies. This approach eliminates all uncertainty about global trends from subsequent decisions and consequences. This is a highly restrictive approach to informing decisions whose consequences will actually depend on those big uncertainties that are excluded.

48. 115: 31-42. This is an interesting point but not one that is discussed in the body of the report as I recall. It seems odd to have it suddenly appear in the conclusions.

The revised draft provides more background and support for this argument.

49. 116: 4-7. For reasons given above, I would disagree with this conclusion.

Addressed in responses to above comments.

50. 116: 32-35. For reasons given above, I would disagree with this conclusion.

Addressed in responses to above comments.

51. 118: 11-14. For reasons given above, I would disagree with this conclusion.

Addressed in responses to above comments.

52. 119: 2-10. For reasons given above, I would disagree with this conclusion.

Addressed in responses to above comments.

Conclusions

53. Overall, I think this is an important and extremely interesting report, with a lot of very valuable information and some powerful insights about scenario analysis. But I feel that it somewhat inappropriately takes a particular view on scenario analysis which is actually at odds with what I would guess would be the most prevalent position among creators of socio-economic scenarios. Whether or not that is true, it is certainly the case that many such analysts, from Shell onwards, and including many from the backcasting, GSG, SRES and PIA communities, would want to argue for an approach to scenario analysis that is more firmly grounded in an interpretive social science tradition which focuses more on feasibility, desirability, being explicit about normativity, the inherent value-ladenness of scholarship, contingency, etc. Of course this tradition cannot lay any more claim to being right than can any other. Omitting it, however, presents a somewhat one-sided picture of the field.

Addressed in responses to above comments.

Global-Change Scenarios: their Development and Use US CCSP Synthesis and Assessment Product 2.1b

Comments by Dr. Thomas J. Wilbanks, Oak Ridge National Laboratory

11 April 2006

Author team responses in italics:

Although I have the highest regard for the authors, I have somewhat mixed feelings about this draft report on “Global Change Scenarios: Their Development and Use.”

On the one hand, it is a highly professional piece of work: expert in every way, as well-informed about its topic as any such report could be, thorough, and well-written. What has been done here is very well-done indeed.

On the other hand, I have three general concerns:

1. It is not clear for what audience(s) this is intended. It comes across as a set of case study descriptions of scenario uses sandwiched between two tutorials. As an introduction to the art and science of climate change related scenarios, intended for graduate students and new-to-the-game technical staff people, it is very useful. Nothing else exists that is even remotely this good. But for people with some knowledge of the business already, it’s my guess that it is too long, discursive, and academically self-absorbed to be more than another document on the shelf – other than several of the case studies that people may not be familiar with.

The revised draft has attempted to state our two specific intended audiences more clearly and revised the text throughout to maintain consistency for these audiences. Among the resultant changes, the tutorial material has been greatly decreased in length.

2. In many cases, it addresses topics that are the subject of substantial research literatures without any references to those literatures whatsoever. Examples include decision support (2.6) and stakeholder interactions (4.3). As a general principle, it would appear that an SAP should at least provide links to such literatures where they exist.

These are vast areas of literature, which while relevant to scenarios also pertain to many other processes. The revised draft notes the existence of these literatures, and provides cites to a few of the major works in each field.

3. The report simply takes scenarios too seriously, as if they are almost always at the heart of climate science and policy analyses. To a degree not reflected in the cases described, scenario development often occurs in parallel with the analytic-deliberative process and is poorly integrated with it (although the resulting reports

may give a different impression). And then, too often, controversies about the scenarios end up undermining the credibility of assessment results that were not, in fact, very dependent on the scenarios. Frequently, at least in the world of today, the process of scenario development and refinement, especially when it is heavily quantitative, is a sidebar for decision support rather than a key building-block, which might raise questions about why it gets so much emphasis. Maybe there is a tendency to pretend to pay attention to scenarios because that seems to make an assessment process more structured and consistent, even if that is not what really happens; but there is a very real disconnect between the perspectives of scenario developers and the practices of climate change assessments. (More than two decades ago, I published a paper in an IIASA book which reported that policy analysis is used more often to support decisions already arrived at on other grounds than to decide what decisions should be.... Uses of scenarios might be yet another case of this familiar syndrome.) I'm not sure how to reflect this reality in the report, but it would help if scenarios were placed in a larger context, with a bit of humility.

All good points, although one is to some extent compelled to take scenarios seriously when tasked with writing a review of scenario methods. The revised draft has 1) more clearly distinguished scenarios from the models, analyses, and assessment in which they are used, and; 2) noted that scenarios are not the only, or in many cases even the most prominent or important parts, of these activities.

Further general comments:

4. By broadening the definition of scenarios to embrace entirely qualitative narratives, the report tends to lose focus, because so much of the discussion applies mainly to scenarios that are defined in quantitative terms. At the extreme of the use of narratives are “story lines,” like plots for a drama: e.g., the qualitative regional scenarios developed for the EU MedAction project in 2003 -- “Big is Beautiful”, “Convulsive Change”, and “Knowledge is King.” Are those sorts of things included here as “scenarios”? If not, it would be useful to indicate what the alternatives are to scenarios as ways to visualize possible futures.

The revised draft has elaborated on how we define scenarios and what activities are and are not included in our scope.

5. I would like to have seen a little more attention to challenges in developing scenarios of climate change impacts, which link knowledge bases about climate change with knowledge bases about exogenous change in impacted systems. There is a growing need for this, related to serious challenges with cascading uncertainties and – where human systems are concerned – serious challenges in developing scenarios for the human systems to go along with the scenarios of climate change (e.g., technological change, institutional change).

The discussion of scenarios for climate-change impacts assessment and decision-making, which are introduced in sections 2.6 and 2.7, have been expanded.

6. I think the report underplays the power of scenarios in “framing” climate change and other environmental discourses (as contrasted with supporting analyses) . SRES is a good example. This adds to the importance of considering who does the framing: -- i.e., how participative the process is – because the results can carry such weight.

We agree, and these points are made in the revised draft.

7. In IPCC and in most of the other SAPs, a central issue is indicating levels of confidence in the judgments being offered. Here, the conclusions summarized in Section 5 lack references back to their sources in the text (which are often judgments of the authors without other types of authority) and in general fail to differentiate, say, between (a) statements that are well-documented by case study experience, (b) statements about which the authors are highly confident even if evidence is lacking, and (c) statements that are probably true and worth thinking about – but which may not be true in all cases and/or still involve some uncertainty at this point.

In the revised draft, we have attempted to trace more clearly the foundations and support for our conclusions, and in many cases to delimit the conditions under which they apply.

A few more specific comments:

8. Page 3, lines 16-19: I think that impact scenarios will be at least as important and contentious, because discussions of emission stabilization will have to be set in a context of risks/costs of different stabilization levels.

*We agree. The treatment of impact scenarios (usually meaning scenarios **for** impacts, rather than scenarios **of** impacts – although we do consider the case of sea-level rise scenarios, which fall into the latter category) has been expanded – although it remains the case that scenarios of emissions and climate change have attracted the most prominence and controversy thus far.*

9. Page 9, lines 3-8: Also important as a stimulus for stakeholder participation.

We agree. This point has been added.

10. Page 19, Fig 2.4: I think the depiction of this process as being so linear is misleading; in fact, perspectives on impacts pay at least some attention to the kinds of feedbacks shown in Fig 2.6.

The simple linear depiction is explicitly presented as a simplified representation showing how scenarios are used, not showing all important causal linkages in the climate-change issue. In addition, Section 2.7 (and Figure 2.6) shows the more complicated causal linkages necessary for constructing comprehensive scenarios to assess impacts and vulnerability.

11. Page 29, lines 6 ff: Shouldn't this discussion reference the SAPs concerned with decision support?

We will add references to these other SAPs if citable drafts become available in time for further revisions of this report.

12. Pages 39-45: There are other issues as well (ref. F. Toth and T. Wilbanks, "Considering the Technical and Socioeconomic Assumptions Embedded in the SRES Scenario Families," *IPCC Working Group II Guidance Papers*, Fourth Assessment Report, September 2004), including:
- (1) Is this general approach to incorporating technological and socioeconomic uncertainties into GHG emission projections the best alternative? Are there other possible approaches that should be considered?
 - (2) Do the four scenario families, as described above, satisfactorily capture the range of possible global futures over the next century? Do they reflect current thinking about paths toward (and away from) sustainable development? A number of more recent efforts at narrative story-telling, some of them including quantitative projections, might be consulted about qualitative attributes, especially if any of them paint a substantially different picture than the SRES families. Examples include the U.S. National Academy of Science's sustainability transition study (1999) and multiscale scenario development at ICIS to support the MedAction project in the Mediterranean region (2003).
 - (3) Are there other ways to incorporate qualitative propositions in quantitative projections, more sensitive to fundamental differences between scenarios?
 - (4) What are the best current knowledge bases for forecasting global and regional demographic and economic change over the next century? It is not clear, for instance, that current socioeconomic assumptions are based on sound theory and data, e.g. regarding feedbacks among model elements (such as interactions between fertility and changing economic conditions and age distributions, and possible responses of consumption and income distribution to changes in trade patterns).
 - (5) How does one handle the challenge of looking at technological change well into the future, not only in energy technologies but also in other technology areas related to GHG emissions, such as materials supply and

consumer choice? For instance, how does one allow for the likelihood of significant technology breakthroughs over a one hundred year period?

- (6) Are there more systematic ways for considering and incorporating scenarios of institutional change and land-use change than were used in these SRES scenarios? For instance, how might developments in information technology change how institutions work, including across national and regional boundaries?
- (7) Are there additional ways for addressing technological and socioeconomic uncertainties that should be considered, beyond creating additional scenarios?

Several of these issues (e.g., points 2, 3, 5, and 6) are considered in the report, in particular in the discussion of the SRES exercise and in the discussion of uncertainties in scenarios. The other points (e.g., points 1, 4, and 7) either concern limitations in the underlying knowledge used to generate scenarios, or pose open-ended questions regarding whether any preferable approaches are available relative to those used in SRES. These are addressed implicitly in our criticisms of SRES, but beyond that we do not have any useful insights to offer on these.

- 13. Pages 45 ff: Regarding the National Assessment, I think this is an accurate representation of how the process looked from the top down, but it is not all that close to how it worked from the bottom up. A new NAS/NRC committee looking at assessment experiences heard several presentations a couple of weeks ago, including one from me, about lessons learned which might be considered in revisiting this section. In particular, I would encourage including in the concluding paragraph (page 54) the Dave Schimel concept of an “inverse” approach to scenario development, starting with end user questions and then developing scenarios that answer those questions.

We agree with this characterization of the National Assessment process as a whole, but this report focused only on the development and use of scenarios within the assessment, which was (and had to be) a more centralized process than the totality of the assessment activity. The report does discuss the unsuccessful attempt to use inverse scenarios in the assessment, although we suspect that the inverse approach proposed in the assessment is different from the inverse approach that the comment refers to. The inverse approach proposed in the comment refers to a more extensive process of involving users in the early development of scenarios. The report extensively discusses and endorses such involvement, but does not use the term “inverse” to describe it.

- 14. Page 62 re conclusions about MEA scenarios: In fact, the scenarios got a lot of attention from the people in that particular working group, but they were very

unevenly used elsewhere in the assessment: e.g., the subglobal component, where there was very little use of them – an example of my third general concern above.

The report does make this observation about MEA scenarios. In view of our primary mission to provide advice to inform future scenario exercises, however, we do not find it necessary to expand further upon this criticism.

15. Pages 66-68: Very useful case of uses of scenarios by decision-makers. It sort of stands out as an exception in this regard.

We agree. The revisions have retained this example, and noted more directly that the cases of effective use of climate-change scenarios in practical decision-making remain uncommon.

16. Pages 75-79: Also very useful as a demonstration that scenarios are used in the private sector by people who find them useful tools for financial risk assessment.

We agree. Same response as for point 15 above.

17. Pages 84ff: Re uncertainty in simple quantitative projections: I question the relevance of a lot of this to the broader scenario efforts that are described in the case studies – and that are in fact the norm. An example of my first general concern above.

This section has been substantially cut, in response to this and other reviewers' concerns that it was unnecessary for our intended audience.