

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.