

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

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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 that 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.