Response of the authors for S&A Product 2.1A to the peer review of the draft report

Monday, July 23, 2006

Peer Reviewers:

Dr. Joseph Aldy, Resources for the Future
Dr. Bill Chameides, Dr. James Wang, Environmental Defense
Dr. Russell Jones, American Petroleum Institute
Dr. David Rind, NASA Goddard Institute for Space Studies
Professor Brent Sohngen, Ohio State University
Professor Richard Tol, Hamburg, Vrije and Carnegie Mellon Universities
Professor John Weyant, Stanford University

1. Introductory Remarks from Reviewers

This section presents introductory comments from the reviewers including specific answers to the questions in the charge to the reviewers. These comments are reproduced without response from the authors. Those comments that require responses are repeated in section 2 below with the response.

1.1 Answers to the Charge to Reviewers

1. Will this report be useful to its readers?

(Sohngen) Yes

(Weyant) The report is very clearly written and contains a great deal of information about where greenhouse gas emissions appear to be heading over the next century and what it would take, especially in terms of energy system transformations, to stabilize atmospheric concentrations of them over that period and beyond. I am sure a lot of readers would have liked more specific and realistic policy scenarios to be addressed, but my sense is that would not have been a good goal for this report because such scenarios would have been quite controversial and subject to being made irrelevant by actual short run policy decisions.

(Chameides, Wang) Yes. It is well-written and well-organized. There is some repetition that probably could be eliminated.

2. Is the charge clearly described in the report? Are the aspects of the charge fully addressed? Do the authors go beyond their charge or their expertise?

(Sohngen) Yes, it's clearly laid out. I do recommend reducing the words for this, particularly throughout the document. It's fairly well laid out initially what (the authors) are up to, although there is a sense that (the authors) keep trying to explain it later so people don't forget. That was a bit distracting, but it does make things very clear.

(Weyant) Paragraphs 2 and 3 of the executive summary succinctly express the charge. This is well within the expertise of the authors.

(Chameides, Wang) Yes, Yes. No.

3. Are the report's exposition and organization effective? The report stands heavily on the graphical presentation of the scenarios; is this an effective means of communicating the material?

(Sohngen) Organization: very good. Exposition: some areas where redundancy can be reduced.

(Weyant) The presentation is generally very clear for such a complex subject, and the many color graphs may be the only way to accomplish this objective.

(Chameides, Wang) Not entirely: A Conclusion chapter is needed to provide appropriate caveats and provide vision for the application of the report and research needed to improve future scenario building. Yes.

4. Are the conclusions adequately supported by evidence, analysis, and argument? How well is uncertainty recognized and discussed? Does the report effectively recognize and communicate the strengths and weaknesses of the scenarios and underlying models?

(Sohngen) For the most part, yes. Some suggestions, per the comments.

(Weyant) The conclusions are very well supported. My sense is the report needs an overarching discussion of the strengths and weaknesses of the scenarios/models in a concluding section. The sensitivities provided by the model comparisons are very well diagnosed and interpreted, but there is not enough discussion about how broader changes in model inputs and parameters would affect the results. In addition, there are some impacts not considered at all by this class of models as implemented here like macro-economic adjustments costs and the costs of imperfectly implemented policies.

(Chameides, Wang) Yes. No. No. Significant uncertainties arise from limitations in the models used (e.g., the inability to forecast how technological innovation might lead to significantly lower costs, the fact that the models may overestimate the CO_2 fertilization effect because of nutrient limitation), and some from the constraints mandated in the prospectus (e.g., the assumption that Annex I (AI) and Non-Annex I (NAI) countries all participate with equalized costs). These need to be acknowledged at the appropriate places in the main body of the text and expanded upon in a Conclusion.

5. Does the executive summary concisely and accurately describe the key findings and recommendations? Is it consistent with other sections of the report?

(Sohngen) Excellent, although some more caution may be warranted when describing land use components.

(Weyant) The executive summary summarizes well what is currently in the main body of the report, but would need some updating if changes are implemented.

(Chameides, Wang) For the most part: Yes; and Yes. However, the ES is a little dense and organizationally too similar to the report itself. It could be improved by providing a broader view of the findings with more integration and synthesis across the chapters, as opposed to a synopsis.

6. What other significant improvements, if any, might be made in the report?

(Sohngen) See attached suggestions, which except for the suggestion of some scenarios without land and ocean sinks, are probably relatively low cost to make.

(Weyant) Consistent with the comments above, I would recommend a concluding section that summarizes the insights from the first four sections and puts them in a broader context by discussing their likely robustness across sensitivities on key inputs, model parameters, and things left out but with the possibility of influencing results/insights.

(Chameides, Wang) See [comments] below.

1.2 Other Introductory Remarks

(Aldy) The authors of the report and the members of the three modeling teams should be commended for this effort. The U.S. government identified three modeling teams with substantial energy, economic, and climate change modeling experience who have developed models that have been used extensively to address both near-term and long-term emissions mitigation questions. The presentation of the results, especially the almost exhaustive set of figures on energy sector characteristics, will be useful for understanding the kinds of changes in the energy sector that may be necessary to achieve long-term emissions mitigation goals.

(Jones) Overall, the 3/1/06 draft does a very useful job of explaining the nature of the models used, the model reference cases, key similarities and differences and in many instances clearly explaining reasons for the differences. The report also does a reasonable job of identify overall strengths and weaknesses inherent in any effort to address stabilization scenarios.

The charge to the modeling group is … to provide useful information to analysts and policymakers on: 1) emission trajectories toward specified “stabilization” levels; 2) draw out key information on energy systems and how those are impacted by stabilization levels; and 3) economic implications of the stabilization scenarios.

The strength of the report is in the description of emission trajectories, assuming one likes the notion of “when” and “where” flexibility – which is key to a least cost approach to stabilization scenarios. The report seems somewhat weaker on the implications for energy systems. While there is significant discussion of changes in energy systems and differences across models in the stabilization scenarios, it was unclear if there were “key findings” regarding any consistent pattern (across models) of least cost changes in the energy system in those scenarios.

Additionally, it would be very useful to know if relatively modest changes in assumptions on energy costs or energy technologies would significantly alter model outcomes. Since the models each ran with a single set of their own technology cost/availability assumptions, no sensitivities were addressed. For example, would a 10% decline in the assumed cost of carbon capture and storage from coal fired electricity generation make any appreciable difference in future carbon prices and energy use? Information on such sensitivities would help understand model results and provide policymakers with added insights.

The greatest weakest of the report was on assessing the charge, "What are the possible economic implications of meeting the four alternative stabilization levels." (Executive Summary, page 2, lines 10-11). As noted on page 20 of Chapter 4, "No attempt has been made to report total cost of stabilization." And Section 4.6, Economic Consequences of Stabilization, provides virtually no information on changes in the economy under stabilization other than reporting various carbon prices.

(Rind) Overall, I found this a useful and interesting document. In many respects, however, I think it underplays the seriousness of the problem. The following comments are made in the spirit of expanding the discussion, or filling in gaps where they seem most obvious. I recognize that some of these aspects were not part of the study, and could not be included at this point; yet there should be more discussion concerning their absence to provide balance to this production. Other things could be better explained, or may well be able to be included.

(Sohngen) The overall objectives of this document are fairly well laid out and clear. The three models utilized are well established and have a long track record of analyzing climate change issues. The results of the scenario analysis provide many new insights to other modelers (e.g., climate models, other more specific sectoral models), and policy-makers considering how to develop greenhouse gas policy.

2. Responses to Comments

This section provides the detailed comments and responses from the authors. Comments are in *red italics* and are followed by the responses from the authors. This section begins with responses to general comments then provides responses to comments directed at individual chapters.

As a general note, a number of comments encouraged the authors to undertake substantial additional analyses, including sensitivity analysis on key assumptions, comparison to previous scenario exercises such as the IPCC's Special Report on Emissions Scenarios (SRES), and formal treatment of uncertainty. The authors strongly agree with the peer reviewers that these additional analyses would be valuable. However, these additional analyses were not called for in the Prospectus and would constitute a substantial expansion of the scope of the effort. In keeping with the spirit of the reviewers' comments, the authors have identified several of these expansions as important areas for future efforts.

2.1 Responses to General Comments

1. *General Comment (Weyant): The conclusions are very well supported. My sense is the report needs an overarching discussion of the strengths and weaknesses of the scenarios/models in a concluding section. The sensitivities provided by the model comparisons are very well diagnosed and interpreted, but there is not enough discussion about how broader changes in model inputs and parameters would affect the results. In addition, there are some impacts not considered at all by this class of models as implemented here like macro-economic adjustments costs and the costs of imperfectly implemented policies.*

   A concluding chapter has been added to the report. The concluding chapter considers broader insights from the analysis as well as limitations, many of which are considered areas for future research.

2. *General Comment (Weyant): Consistent with the comments above, I would recommend a concluding section that summarizes the insights from the first four sections and puts them in a broader context by discussing their likely robustness across sensitivities on key inputs, model parameters, and things left out but with the possibility of influencing results/insights.*

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3. *General Comment (Chameides, Wang): A Conclusion chapter is needed to provide appropriate caveats and provide vision for the application of the report and research needed to improve future scenario building.*

As mentioned above, a concluding chapter has been added to the report. The concluding chapter considers broader insights from the analysis as well as limitations, many of which are considered areas for future research.

4. *General Comment (Chameides, Wang): Significant uncertainties arise from limitations in the models used (e.g., the inability to forecast how technological innovation might lead to significantly lower costs, the fact that the models may overestimate the CO_2 fertilization effect because of nutrient limitation), and some from the constraints mandated in the prospectus (e.g., the assumption that Annex I (AI) and Non-Annex I (NAI) countries all participate with equalized costs). These need to be acknowledged at the appropriate places in the main body of the text and expanded upon in a Conclusion.*

   The authors agree that enormous uncertainty surrounds a multitude of assumptions in each of the scenarios. With respect to uncertainty, the Prospectus directed only that the authors use values for key drivers that they believed to be both “plausible” and “meaningful”. Formal consideration of uncertainty was not included in the Prospectus and was considered to be beyond the scope of the exercise. The authors concur with the reviewer, however, that readers would benefit from further analysis of the uncertainties. For this reason, the authors have identified formal sensitivity analysis as an important area for future efforts. In addition, the authors have attempted to explicitly acknowledge at several places in the text the uncertainty inherent in scenarios that look out one hundred years. However, the authors have not identified particular areas as more or less important, believing this enters the realm of formal uncertainty analysis, which is beyond the charge of the exercise.

5. *General Comment (Chameides, Wang): The ES is a little dense and organizationally too similar to the report itself. It could be improved by providing a broader view of the findings with more integration and synthesis across the chapters, as opposed to a synopsis.*

   In response to the comment, a section has been added to discuss possible uses of the scenarios and to put some of the discussion of limitations and cautions into the Executive Summary. At a number of places, the text was expanded to give a broader view of the findings. However, the basic structure remains that of a synopsis.

6. *General Comment (Sohngen): There are lots of areas where the writing can be tightened. The text is repetitive in many places. I would urge the authors to undertake a substantial edit of the document, taking a heavy hand at editing out repetitive statements and even repetitive paragraphs. I think the goals, and the limitations, of the models and documents are fairly well laid out in the executive summary and introduction. However, these seem to be repeated a*

lot throughout. If you can find ways to cut down on this even a little bit, I think it will help.

The authors agree and have made revisions to tighten the document.

7. *General Comment (Sohngen): There are several areas where the model names switch, for example IGSM and EPPA are I think the same model, but there are areas where IGSM is used and areas where EPPA is used to denote results from the MIT modeling group. I imagine these are for good reasons, perhaps because the authors are referring to specific components of the model rather than the whole system. But I found it to be a bit distracting. Can everything just be EPPA or IGSM and then if you need some additional text to describe a specific component, go ahead and use it?*

   The link between EPPA and IGSM has been clarified, and IGSM is used throughout the document to reduce confusion.

8. *General Comment (Sohngen): The use of CO_2 and C for carbon throughout is somewhat confusing. For instance, I assume where Gt C is used, it is gigatons (or petagrams) carbon and not CO_2, but this is sometimes used in close proximity to references to CO_2 in the text, so I'm not entirely sure. I would recommend clarifying terms up front, potentially with a text box that identifies the units used throughout the analysis. You could also define some terms used throughout the text in a text box at the beginning.*

   All emissions quantities for CO_2 are given in terms of carbon, whereas CO_2 is used to discuss the greenhouse gas itself. The authors are sympathetic to the suggestion for clarification of terms up front. This will be included in the next version of this report.

9. *General Comment (Jones): The strength of the report is in the description of emission trajectories, assuming one likes the notion of "when" and "where" flexibility – which is key to a least cost approach to stabilization scenarios. The report seems somewhat weaker on the implications for energy systems. While there is significant discussion of changes in energy systems and differences across models in the stabilization scenarios, it was unclear if there were "key findings" regarding any consistent pattern (across models) of least cost changes in the energy system in those scenarios.*

   Additionally, it would be very useful to know if relatively modest changes in assumptions on energy costs or energy technologies would significantly alter model outcomes. Since the models each ran with a single set of their own technology cost/availability assumptions, no sensitivities were addressed. For example, would a 10% decline in the assumed cost of carbon capture and storage from coal fired electricity generation make any appreciable difference in future carbon prices and energy use? Information on such sensitivities *would help understand model results and provide policymakers with added insights.*

The greatest weakest of the report was on assessing the charge, "What are the possible economic implications of meeting the four alternative stabilization levels." (Executive Summary, page 2, lines 10-11). As noted on page 20 of Chapter 4, "No attempt has been made to report total cost of stabilization." And Section 4.6, Economic Consequences of Stabilization, provides virtually no information on changes in the economy under stabilization other than reporting various carbon prices,

(a) With respect to the impacts on the energy system, adjustments have been made to the writing to better elucidate key points both in the sections and in the Executive Summary. (b) With respect to sensitivity analysis, please see the general comment on expansions to the scope of effort in the introduction to the responses to comments. (c) With respect to economic impacts, the report has been supplemented to include information on the GDP impacts of stabilization as well as a discussion of the fundamental challenges associated with estimating economic impacts and welfare impacts more generally.

10. *General Comments (Tol): www.mit.edu/globalchange does not exist; web.mit.edu/globalchange/www does; the technical description of the model seems adequate and is easy to find.*

    The website has been corrected.

11. *General Comment (Aldy): Participation Assumption: The prospectus notes that the stabilization scenarios are based on universal participation (chapter 1, page 10, lines 36-41). Short of doing a full uncertainty analysis (which I discuss below), it may be useful to policymakers to illustrate the effects of less than universal participation in the near term. For example, one could choose one of the stabilization goals (e.g., 550 ppm) and run the models again assuming that developing countries do not constrain their emissions until 2050 (or 2030 or some other arbitrary date). Presumably, this would show both an increase in costs and the more dramatic decline in emissions necessary later in this century to meet the stabilization target. It would also be useful to gain a sense of the emissions leakage associated with less than universal participation in the near term in these models.*

    Sensitivity analysis on the optimal policy regime constitutes an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

12. *General Comment (Aldy): Uncertainty Analysis: I encourage future work (either for this or subsequent reports) on uncertainty analysis. As an initial effort, a few cases with alternative assumptions (such as the participation*

constraint mentioned above) would be illustrative. These could include differing assumptions about carbon capture and storage availability and cost. Or the assumptions made in these models about the availability of alternative zero-carbon energy (e.g., nuclear, which is constrained in the IGSM model – chapter 3, page 9, line 44) or sinks (e.g., biological sequestration assumed to be neutral in MERGE). A full-blown uncertainty analysis may provide a better sense of what are the really critical technologies necessary to achieve various stabilization goals than evident in this analysis.

Sensitivity or uncertainty analysis would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

13. *General Comment (Aldy): Technological Change. Modeling technological change, especially over many decades and even centuries, is a very difficult task. It appears in the description of technological change in section 2.2.5 (chapter 2, pages 7-8), that all three models make assumptions about the exogenous improvement in the energy efficiency of the economy through an autonomous energy efficiency improvement (AEEI) parameter or some combination of parameters with such an effect. These models also characterize the effects of carbon prices on the deployment of technologies. It appears that these models do not, however, account for the effects of carbon prices on the R&D process. If one's interest is 2010 or 2020, that may not have much effect. If one's interest, as it is in this report, however, is 2050 and 2100, this can matter. Carbon prices up to $6,000 per ton of carbon in 2100 (Table 4.6) would likely affect innovation incentives. Do the models make any ad hoc assumptions about the availability and cost of low-carbon and zero-carbon technologies under the reference and stabilization scenarios? It appears that the very high carbon prices in the distant future under various stabilization scenarios (Figure 4.18) reflect in part the absence of innovation in these models. It should be pointed out that accounting for innovation may not substantially affect the total costs of an emissions mitigation policy – diverting capital from innovative activities in other parts of the economy may slow growth in those sectors. Accounting for innovation, however, should lower the marginal cost of abatement. In this case, the report should note that accounting for innovation could affect the estimated carbon prices.*

In keeping with the spirit of the comment with respect to the importance of technological change for costs, the authors have substantially revised the discussion of the economic impacts of stabilization. This section now explicitly identifies technological assumptions as a primary factor driving the difference between costs among models in the second half of the century.

The reviewer is correct that the models make exogenous assumptions about technological change throughout the model, including assumptions about end use technologies, energy supply technologies, agricultural productivity (in models with explicit land use components), and the productivity of labor throughout the economy. These assumptions include substantial improvements beyond the level of technology of today. The authors agree with the reviewer that accounting for innovation process should lead to some level of divergence in technological change between scenarios. However, the authors do not believe it appropriate to speculate on the degree of this effect. The authors also do not believe it appropriate to assert that carbon prices would be lower or higher than those presented, because the modelers have not associated the assumed levels of change with any particular stabilization level.

14. *General Comment (Aldy): What is the basis for the claim that "it is likely that the levels of exogenous change assumed in these three models would span the range of results from models imposing more structural detail in the change process" (chapter 2, page 8, lines 28-30)? This is potentially misleading. First, a detailed representation of the technological change process would illustrate how positive carbon prices affect the innovation focused on the carbon content of energy, which is clearly distinct from the energy content of economic output. Second, empirical estimates of the AEEI are based on analyses that control for the effect of energy prices on the energy intensity of output. The effects of carbon prices on innovation that does affect the energy intensity of output should be considered supplemental to the autonomous improvements.*

    The sentence has been removed.

15. *General Comment (Aldy): Comparing Reference Scenarios with SRES: The description of the reference scenarios (e.g., section 3.2 on socio-economic assumptions) would benefit by an explicit comparison with the SRES work. How do the population and economic growth forecasts used in these models compare to specific SRES scenarios? How do they compare, for example, with the assumptions in the SRES A1f and A2 suites of scenarios? (These are identified in chapter 3, page 17, lines 7-8 as those with comparable emissions in 2100 as the reference scenario emissions profiles for this report.) How do these assumptions match up with the various "storylines" used in the SRES (some of which I had difficulty understanding how the SRES operationalized them)? What are the primary reasons why the reference scenarios for this report yield results comparable to the upper end of the SRES scenarios and higher than IS92a?*

    Comparison with previous scenarios would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments. At the same time, the authors note that the aggregate fossil and other industrial emissions trajectories from the SRES analyses are included in Chapter 3 as a point of reference.

16. *General Comment (Aldy): Growth in U.S. Energy Per Capita: Figure 3.4 shows that energy use per capita increasing by two-thirds or more over the next 100 years in the United States. In what sectors of the economy do these models show the growth in energy consumption? With the assumption of higher energy prices in the reference case, this appears to be quite substantial increases in energy consumption per capita. For example, energy consumption per capita in the U.S. was only 5% greater in 2000 than it was in 1970 (based on total energy consumption, Table 1.1, EIA's 2004 Annual Energy Review and Table 1 of the 2006 Statistical Abstract). This slow growth obviously reflected the effects of the high oil price period from 1973-1986. It would at least be informative to understand the sectors of the economy with the more substantial increases in energy consumption in the reference scenarios to illustrate the potential opportunities for policies to target these energy growth sectors in efforts to achieve future emissions mitigation goals.*

    Sectoral decomposition of the energy sector by end-uses has not been pursued in part because the approaches to sectoral breakdowns and the detail at which sectors are modeled differ among the participating models.

17. *General Comment (Aldy): The Effects of Energy Price Increases on Reference Case: The report notes that oil and gas prices are projected to increase, but given the nature of these models, these increases should be considered long-term average trends. Short-term energy price shocks are not characterized in these models with time-steps on the order of 5 to 15 years. Are the energy price profiles used in these reference scenarios significantly higher than those used by these modeling teams in previous analyses, such as the IPCC SRES effort? The Energy Information Administration's Annual Energy Outlook 2006 (AEO2006) includes a rather substantial upward adjustment in oil and gas prices over the next 25 years. Although this is a shorter timeframe than considered in this report, it illustrates the effect of changing energy price assumptions over time. In 2025, the EIA reference case total carbon dioxide emissions from fossil fuel combustion for the United States is 7,587 $MMTCO_2$. This is 5.9% less than the emissions forecast for 2025 in the previous year's Annual Energy Outlook (AEO2005: 8,062) and 7.5% less than emissions forecast in the 2003 forecast (AEO2003: 8,202). Other factors also influence the decline in emissions forecast over the next two decades, but expected increases in energy prices clearly play an important role. Further details about the energy price assumptions employed in the reference case scenarios, with comparisons to previous scenarios work with these models, would improve the description of the reference scenarios.*

    It is important to emphasize that prices are model outputs. Thus, they reflect interactions through markets within the models. For example, assumptions about relative availability of different fossil fuel resources are reflected in their prices. Of course, these abundances along with the different carbon-to-energy ratios of the different fossil fuels lead to different fossil fuel carbon intensities in the three models. This is discussed in section 3.5.1. Because of the importance of energy prices, Chapters 3 and 4 both provide extensive discussions of the energy price trajectories and their underlying drivers – these same drivers apply to the energy system more generally.

Comparisons to previous scenarios would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

18. *General Comment (Aldy): The models vary quite significantly with respect to oil consumption in their reference scenarios (Figure 3.3). MERGE shows global oil consumption declining between 2020 and 2030, while IGSM shows global oil consumption increasing throughout the century and MiniCAM peaks around 2080. Although this is not the focus of this report, these results do bear on an ongoing debate in the energy and policy communities: when will oil production peak? It would be useful to explicitly address this issue, perhaps in a box, for those with such an interest. It would also be beneficial if the figure or the discussion in the text could discern conventional oil from unconventional oil production. For example, does the increase in oil consumption in IGSM reflect different assumptions about the economic availability of unconventional oil sources? It would also be very informative to discuss the carbon implications of peak oil. Some may believe that peak oil would result in a lower carbon future. Is this the case? If cellulosic ethanol substitutes for petroleum, that may hold. In contrast, if coal-to-liquids substitutes for petroleum, this may not be the case. Providing some insights on the reasons for and carbon implications of peak oil would be very welcome.*

The authors agree with the spirit of the comment regarding the importance of the transition from conventional oil. For this reason, the implications of a transition away from conventional fuel sources serve as the primary theme of Section 3.1.1. The basis for differences between the models with respect to oil consumption is a function of the increasing costs of extraction as cheaper resources are exhausted, the availability of alternative fuels such as synthetic fuels from coal and biofuels, and the demand for transportation fuels more generally. This material is discussed in Section 3.1.1, but a box on the peak oil issue might be informative and potentially valuable in the final version of this document.

A precise delineation between conventional and unconventional sources would be valuable. However, although all the models represent fossil fuel resources in terms of graded resource bases, not all models make such a precise delineation between conventional and unconventional oil, instead representing increasingly unconventional sources exclusively in terms of increasing extraction costs. For this reason, breaking out these sources was not feasible.

19. *General Comment (Aldy): Constrained Nuclear: The IGSM reference scenario assumes that there are political limits on nuclear power (Chapter 3, page 9, lines 43-45). What is the basis for this assumption? Are there common political constraints worldwide, or only in some countries/regions? How is this political constraint operationalized in the model?*

    The assumption of constraints on nuclear power is a scenario assumption. In keeping with the spirit of the exercise, the authors have focused on understanding the implications of this and other key scenario assumptions and have minimized text devoted to justifying these assumptions.

20. *General Comments (Aldy): Biomass Fuels: The IGSM reference scenario assumes no commercial biofuels in the United States. Given the current use of ethanol and (generous) policies that promote the use of ethanol, this appears to be a peculiar outcome of the model. This result is in sharp contrast to the positive and growing use of biofuels in the other two models. With the policy interest in biofuels, the report should explain the zero level of biofuels over the next 100 years.*

    Adjustments have been made to the IGSM scenarios to better reflect the demand and production of biomass in the U.S.

21. *General Comment (Aldy): 2095 or 2100: Table 2.1 indicates that MiniCAM operates on 15-year intervals through 2095. Every figure and virtually every reference in the text apparently represents modeling outputs for the year 2100. This is important in the context of stabilization concentrations (chapter 4), in which some of the radiative forcing levels are lower than necessary for the target. It appears that these below-target levels reflect the assumption of stabilization at the target level at some point in the future. How are these estimated if that point in the future is beyond the time horizon of the model? Are the figures showing 2100 results for MiniCAM correct, and Table 2.1 is wrong, or vice versa? If it is the former, then Table 2.1 should be corrected. If the latter, then this should be explicitly addressed in the report and the figures modified accordingly.*

    For any model, results for time steps that are not explicitly modeled must be represented either through interpolation or extrapolation. This is the case for MiniCAM results in 2100. However, the fact that MiniCAM is below the target radiative forcing levels for Levels 2 through 4, is not a function of interpolation. It arises because stabilization occurs in the next century. In 2100, the globe is on a trajectory toward stabilization, but has not yet reached stabilization. For Level 1, stabilization occurs in this century for all three models. For Level 2, stabilization occurs somewhat beyond 2100. For Levels

3 and 4, stabilization occurs well into the next century. As stated in the text, the modeling teams using models that run only to the end of the century (MiniCAM and IGSM) had to make judgments as to the appropriate levels and rates of change of radiative forcing at the end of the century.

22. *General Comment (Aldy): Electricity Generation Variation: These models yield very different reference scenario estimates for electricity generation in 2100: 228 EJ/year in IGSM to 459 EJ/year in MiniCAM (chapter 4, page 11, lines 30-31). What explains this very substantial range? Economic growth? More generation of power outside of the electricity sector and thus captured in other parts of the IGSM model? Is electricity generated in MiniCAM to produce hydrogen for the transportation sector? This wide range raises questions that additional text should address.*

    The discussion of variation in electricity production has been enhanced to better explain the key forces.

23. *General Comment (Aldy): Safety Valve in MERGE: The carbon price results discussion indicates that MERGE carbon prices level off at $1,000 per ton of carbon because of "an assumption in MERGE that at this price actors in the economy can purchase emissions rights in lieu of reducing their emissions further" (chapter 4, page 17, lines 31-33). First, this is a rather opaque reference to what appears to be a modeling assumption of a safety valve. Given the policy prominence of this tool, it should receive more attention than this. Second, and more importantly, it is not clear why this safety valve is triggered. The end of this paragraph notes that the Level 1 RF target is still met with this assumption. If global emissions are sufficiently low to meet this target, why do economic agents purchase additional emissions rights?*

    The reviewer is correct that the approach in MERGE is equivalent to a safety valve. The safety valve is triggered whenever the price of carbon reaches $1000/tonne C. This only occurs in the Level 1 scenarios. As the text points out, the safety valve does not keep MERGE from meeting the Level 1 target. This is because emissions reductions in other periods of the model are sufficient for stabilization.

24. *General Comment (Rind): It would be useful to emphasize what this report is not: it is not a replacement for the SRES scenarios, in which estimates are made of likely changes in trace gas releases under different economic or environmental assumptions. By having either a 'business as usual" scenario, or targeted levels to be achieved, it manages to avoid providing best guess estimates entirely. Since that might well have been what the readers were expecting, it would be useful to make the point explicitly.*

    The authors agree that it would be valuable to compare and or contrast these scenarios with previous scenarios, such as the SRES scenarios. However, such comparisons would constitute a substantial expansion of the scope of effort (please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.) For this reason, the authors have chosen to minimize discussion of previous scenarios throughout the report.

25. *General Comment (Rind): I understand that the focus of this exercise was greenhouse gases, and that aerosols were not to be considered. However, as shown in Figure 1.1, there are potentially large cooling affects, both direct and indirect, associated with aerosols. While the increased use of coal would have the potential to increase sulfates (and soot), experience shows that societies are much more willing to remove these from the atmosphere to limit their visibility and health effects. If CCS occurs to the extent anticipated in this report (and most likely even without it), the cooling associated with sulfate aerosols is undoubtedly going to decrease, adding to the net radiative forcing; at the very least, the implications of this effect should be indicated.*

    The authors agree with the reviewer about the importance of aerosols. Furthermore, the modeling teams have observed precisely the types of feedback effects mentioned by the reviewer in their own modeling work. However, consideration of aerosols was not called for in the Prospectus, and would constitute a substantial expansion of the scope of effort. Furthermore, the authors note that if the exercise were to expand beyond the six-gas bundle to gases that cause indirect effects, the problem grows in both complexity and uncertainty.

26. *General Comment (Rind): As noted, the technology and possibility for CCS is highly uncertain, yet it plays a prominent role in the future scenarios; this is an example of how things may well be worse than this report implies. As long as we're talking about speculative technologies that would make a difference, I wonder why fusion reactors, which really fall in much the same category, are not afforded the same treatment? As is well-known, an initial small-scale fusion reactor is to be built in France, and very possibly a larger-scale project subsequently in Japan (I know of no similar relevant CCS plan.) There is thus some reality to this as a possible energy source, if not by 2050 than post-2050 when the largest greenhouse gas reductions are envisioned. The use of 'nuclear technology' in the report does not specify what is implied, but my guess is that it is a continuation of the same technology we have today. A statement concerning this omission would be useful to provide perspective. It would also be nice to have a comment about the prominent (and somewhat dubious) role CSS plays in producing these results right up front, rather than having to wait until chapter 4.*

    On several points. (a) The authors have followed the directions in the Prospectus, which called for each team to construct a single set of underlying assumptions that they considered both plausible and meaningful. In keeping to the letter and spirit of the Prospectus, the authors have chosen not to discuss the many other possible futures that could have been considered in this exercise. (b) It should also be noted that although carbon capture and storage is important in all three sets of scenarios, it is not the only technology that sees substantial expansion and that is critical for stabilization. Other technologies, including bioenergy, nuclear fission power, other renewables such as solar and wind, and energy use reduction are all critical factors in stabilization and experience deployment levels well above those of today. Because the scale of deployment is substantial for multiple technologies in the scenarios, the authors have chosen not to focus on the deployment of any single technology. (c) Nuclear technology in the report refers to nuclear fission power.

27. *General Comment (Rind): Outside of some implied climate-change impacts on biospheric CO_2 emissions, there is nothing in this report that recognizes the potential for climate warming to alter anything. In particular, the energy demand/electricity demand scenarios will be highly sensitive to global warming. The report should note this important omission. (EPRI has done numerous studies on this and could undoubtedly provide details - this brings to mind that the report is pretty weak on references concerning any of the subjects not covered directly.)*

    A paragraph has been added at the end of Chapter 2 that discusses that the models are not fully closed and specifically mentions temperature feedbacks on heating and cooling.

28. *General Comment (Rind): As noted, the minimalist assumption is made that all countries will participate in activities to limit greenhouse gases. As a first guess that's o.k. However, subsequent work should look into what would happen if specific countries did not 'get on-board' - in particular, the U.S., or China and India. This would seem to better reflect (at least current) real-world conditions.*

    Sensitivity analysis on the optimal policy regime would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

29. *General Comment (Rind): An 'ocean saturation' effect is noted for CO_2 uptake - however, that is not explained. In fact, numerous studies have now been made indicating that as climate warms, the ability of the ocean to remove CO_2 decreases. Has that effect been included? If not, it should be mentioned.*

    This effect is included in the IGSM and MiniCAM models, and this is now explicitly noted in the text. MERGE does not include this effect, but it could be partially balanced by the assumption of a neutral biosphere.

30. *General Comment (Rind): The terrestrial land system is indicated to be a continual sink for additional CO_2. There would seem to be several implausible assumptions associated with this. It is noted that the world population is estimated to increase by all the modeling groups by some 40-66%; exactly how much additional arable land will this require, both to feed them and house them? Will this allow for any expansion of natural vegetation (the only kind that would act as such a sink)? Furthermore, apparently a CO_2 fertilization assumption is being used; what value is that exactly, because real world field studies indicate the effect does not persist in the real world, probably due to other limitations (water, nitrogen, sunlight).*

    The discussion of the land use components of the models has been expanded and revised substantially in the text. The limitations of the models in this regard have been more clearly highlighted. Furthermore, the land-use components of the models have specifically been called out as an area for future research.

31. *General Comment (Rind): Methane projections are made; yet methane (as noted) is part of the global atmospheric chemistry cycle, which includes many other trace gases which do not appear to be projected here (e.g., CO, O_3). How can atmospheric concentrations (as opposed to emissions) for this gas really be calculated? (Also, this is another example of how global warming and likely changes in both temperature and water vapor will affect the response.)*

    The discussion of the Earth System components of the models has been expanded in Chapter 2. As discussed in Chapter 2, The IGSM models atmospheric chemistry, resolved separately for urban (i.e., heavily polluted) and background conditions. In MiniCAM, Reactive gases and their interactions are modeled on a global-mean basis using equations derived from results of global atmospheric chemistry models. MERGE does not explicitly represent atmospheric chemistry interactions in its reduced-form earth-systems model. It models the radiative effects of GHGs using relationships consistent with summaries by the IPCC, and applies the median aerosol forcing from Wigley and Raper.

32. *General Comment (Rind): The report is only concerned with greenhouse gas and radiative forcing stabilization, not with global temperature change, but clearly the only reason for stabilizing the radiative forcing is to limit global warming. The approach adapted by all the modeling efforts - to reach stabilization with the least cost to the economy - results, as noted, in the major reductions in emissions occurring in the last half of the century. Obviously, discounting makes that work. However, from the climate point of view, that is exactly the worst approach, for by waiting to reduce emissions (or increasing sinks), the atmosphere is allowed to maintain higher*

concentrations of greenhouse gases for a longer period of time, which amplifies the warming. It is the integrated radiative forcing over time that is the issue, not the value at any particular year. This aspect should be noted.

The authors have followed the directions given in the Prospectus. Critique of these directions or comparison to other possible approaches to scenario development is viewed as beyond the scope of this exercise.

33. *General Comment (Rind): The use of Global Warming Potentials seems to be included only in recognizing the cost; but from the climate point of view, they make a big difference to the climate post-2100. This point should be emphasized - different trace gas reductions are not equal, regardless of their immediate impact on radiative balance.*

Global warming potentials are only used in this report in determining the relative prices of non-CO_2 gases in MiniCAM. Beyond this, the relative roles of different greenhouse gases in stabilization, including the relative timing of emissions abatement as a result of differing atmospheric lifetimes, is a primary focus of the report and receives extensive discussion. Nonetheless, the authors have attempted to sharpen this discussion as appropriate.

34. *General Comment (Rind): By not providing more commentary on the economic impact of the necessary steps to achieve each level of stabilization, the report seems to back away from the most important result. I'm sure the models produce the GDP changes; why not show them as the last part of the report (on p. 20)? In addition, on p. 14 of part IV, the report lists the mix of responses necessary to reduce energy consumption - why not go into detail on how the different models handled them? It is very unsatisfying not to see this.*

The authors agree with the reviewer that more is needed on the economic implications of stabilization. The report has therefore been expanded to include more discussion of the GDP implications of stabilization among the models along with a discussion of the difficulty in developing effect welfare metrics more generally.

As discussed in Chapter 4, the changes in energy demand patterns are a function of three factors: (a) Substitution of technologies that produce the same energy service with lower direct-plus-indirect carbon emissions, (b) changes in the composition of final goods and services, shifting toward consumption of goods and services with lower direct-plus-indirect carbon emissions, and (c) reductions in the consumption of energy services. The authors agree that it would be a valuable follow-on research area to begin to better quantify these different elements. However, this is not feasible within the scope of this exercise because each of the models has a different set of technology options, different technology performance assumptions, and different model structures. Furthermore, no well defined protocol exists that can provide a unique attribution among these three general processes. This point has been clarified in the text.

35. *General Comment (Chameides, Wang): Stabilization Scenarios and Overshoot: The committee has chosen to interpret a stabilization target as a target that cannot be exceeded (at least not significantly) at any time during the simulation period. However, because of the inertia in the climate system, it is possible for CO_2/GHG and its corresponding radiative forcing (RF) to exceed the stabilization level for some brief period of time and still avoid dangerous temperature increases provided the RF returns below the stabilization level promptly enough. This phenomenon is commonly referred to as overshoot. It would be useful for the committee to consider some alternative scenarios that allow for overshoot but still meet the specified target by the end of the 21st century. At the very least there ought to be some discussion of overshooting scenarios and their implications (perhaps in a Conclusion). Even though the prospectus did not require an analysis of overshooting scenarios, such pathways may be necessary to avert dangerous and irreversible climatic changes in the long term. It would be helpful to place the report's stabilization pathways in the context of a wider variety of options for preventing dangerous anthropogenic interference, including overshooting pathways.*

    The authors agree that overshoot pathways are a potentially valuable avenue for future research. Overshoot pathways were not considered in this exercise because they were not prescribed in the Prospectus. The authors have added a sentence in Chapter 4 that indicates that the stabilization pathways used in this exercise do not exhaust all the possibilities.

36. *General Comment (Chameides, Wang): Technological Innovation: There is essentially no discussion of technological innovation: how, if at all, this is treated in the models and how such innovation might affect the projected costs of meeting the scenario targets. For example how might the projected $/ton C illustrated in Figure 4.18 be affected by a possible breakthrough in PV technology. At the very least this would be an appropriate topic for a Concluding chapter. There is also little explicit discussion of changes in vehicle fuel economy, consumer preferences as to vehicle types and sizes, and gasoline prices. These would seem to warrant more attention, since transportation is an important and fast-growing contributor to GHG emissions.*

    (a) The report explicitly states that the models all use exogenous assumptions regarding the rates of technological advance. The authors believe that a discussion of the process of innovation is beyond the scope of this effort, and have therefore not included such a discussion. (b) With respect to sensitivity analysis on assumptions of technological change, this would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments. (c) Finally, the authors agree that greater information on underlying technology assumptions would be valuable. However, the participating models employ very different approaches for representing technology, so it was not feasible to present technology assumptions across models in a methodologically consistent fashion. This point is now explicitly made in Chapter 2.

37. *General Comment (Chameides, Wang): Land-Use and Deforestation: Consideration of the role of land-use changes and deforestation in hindering or helping to meet the stabilization targets is not adequately addressed. Do the models assume that adoption of C caps will have any impact on deforestation rates? Is it not possible that compensation to tropical rainforest countries for slowing their rates of deforestation could reduce the costs of meeting the targets? Has this been considered? Has there been any consideration of land-management/C-offset projects as a vehicle to reduce RF as well as the costs of meeting the targets? If not, why not? The apparent decision to exclude land-use and deforestation from the policy framework is a weakness that even leads to the unintended consequence of increased deforestation in the MiniCAM model for the stabilization scenarios due to increased demand for biofuels. Is it likely that such an outcome would come to pass in a global framework for capping GHG emissions?*

    The authors agree with the reviewers. The material on land use and land use change in both Chapters 2 and 4 has been extensively revised to better represent limitations and strengths of approaches used in these scenarios. With respect to the MiniCAM, the analysis now includes a price for terrestrial carbon resources just as it does for fossil fuel carbon. This in turn leads to explicit trade-offs between maintaining and/or expanding carbon stocks and using those lands for agriculture, pasture, commercial forestry, urbanization and commercial bioenergy. The text now notes the importance of valuing terrestrial carbon in the MiniCAM results and cites a sensitivity analysis in which terrestrial carbon is not valued for comparison. The failure to value carbon leads directly to accelerated rates of deforestation, while in the case in which terrestrial carbon is valued leads to restrained deforestation rates.

38. *General Comment (Chameides, Wang): AI vs. NAI countries: The assumption that all nations would participate in a regime of emissions limitations by 2015 is probably unrealistic (certainly too optimistic for developing nations). Some discussion, even sensitivity analysis of how this assumption affects the results is needed. At the very least, the report should mention that a delay in action is possible (even probable) and that this would increase the stringency of the required emissions reductions in the out-years.*

    Sensitivity analysis on the optimal policy regime would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments. While formal sensitivity analysis is not feasible for this exercise, the authors have chosen to better highlight the issue of underlying policy regime. The point that the assumed policy approach may substantially underestimate both the global and regional impacts of mitigation has been raised at several points in the text, including references to other work.

39. *General Comment (Chameides, Wang): Not meeting Level 1 target: It is noted in the text in Ch. 4 that the models are not quite able to meet the Level 1 target. However, there is no discussion of the implications. Presumably the models are so close to the target that it is not a "big deal." Nevertheless some discussion of why the models were "unable" to meet the target and what that says about the viability of the Level 1 scenario is called for.*

    A sentence has been added in Chapter 4 that notes that the implication of the slightly higher radiative forcing in IGSM Level 1 is that this scenario has less non-emitting technology and lower economic costs than would be the case if the constraint were met precisely. However, the reviewers are correct that the variation is not a "big deal", and that all the models met the radiative forcing levels within an appropriate degree of precision.

40. *General Comment (Weyant): In several places economically efficient achievement of the radiative forcing targets is mentioned but not put in a broader cost effectiveness/cost benefit context. In addition, it says this might give you a useful lower bound on costs, but there is no discussion of what could make the costs much higher or lower and roughly how much higher or lower (e.g., 10%, factor of 2, order of magnitude). Things like revenue recycling, co-benefits, command and control, limited scope, etc. come to mind. Finally, there is not much discussion of different cost metrics and what they mean.*

    The authors agree and have attempted to better explain the impacts of the choice of policy approach prescribed in the Prospectus. The point that the assumed policy approach may substantially underestimate both the global and regional impacts of mitigation has been raised at several points in the text, including references to other work. However, the authors believe it is beyond the scope of this work to speculate regarding the differences among various policy approaches.

41. *General Comment (Weyant): I cannot figure out how oil markets work in the models. The write up makes it seem like all fuel markets are perfectly competitive and marginal cost pricing prevails, but I suspect something different might be going on for OPEC supply where market power is probably being exercised and either prices or quantities are assumed exogenously or there is some kind of market response or supply function (sloped more*

steeply than marginal costs). Also, I think many people would be interested in oil and gas import/export numbers as relevant to energy security concerns.

The authors have attempted to sharpen the discussion in this area. As the reviewer suggests, the models all assume efficient oil markets. However, as the text indicates, the models do not address oil markets at the level of detail that would be necessary to explore such issues as the implications of OPEC market power.

42. *General Comment (Weyant): It is not abundantly clear exactly clear why these scenarios are chosen and how they relate to sequential decision making under uncertainty. Rather than avoid this issue I would prefer a Mort Webster like discussion of synergies and complementarities between the two approaches.*

    The need for consideration of decision-making under uncertainty has been added as an area for future research in the new concluding chapter to the report.

43. *General Comment (Weyant): Technology assumptions may be conservative for latter part of century. Suggest adding sensitivities if possible.*

    Sensitivity analysis would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

Executive Summary Comments

1. *ES, Page 5, Lines 33-38 (Sohngen): The terrestrial changes included are fairly specific to a single ecological adjustment. This could be rewritten here and later in the text as discussed below, just to recognize some of the uncertainties associated with the fluxes. There seem to be a couple of issues:*

   The initial baseline net sink of .5 Gt C seems to be based largely on inversion model results, which show a range of .7 - 1.3 Gt C annual sequestration in recent past. The results seem to ignore evidence from statistical sampling protocols, which suggest the opposite, in the range of 0.9 - 2 Gt C emissions from forests in the recent past. I'm not sure which I believe, but it seems to me that the results of the baseline and stabilization scenarios are sensitive to these effects, and thus I would suggest that some discussion of the uncertainty here would be warranted.

   There is also uncertainty about the growing net sink as climate changes. These results are all based on one type of ecological model (well one different type for each of the two modeling groups that use them). There are other models and there is little discussion about the sensitivity of the results. I realize there are millions of uncertainties in any modeling exercise like this (a point you already make in the document), and you may view other uncertainties are more pressing or important (hence the discussion about uncertainty in the untested CCS technology), but I think it would be useful to point out the implied sink uncertainty in particular, since at least one of the three models (MERGE) assumes no net change in sinks.

   The paragraph was rewritten to clarify the difference between MERGE and the two other models. Also it is made clear that the numbers quoted include the emissions from deforestation which implies net natural uptake closer to the reviewer's range. Also, a sentence was added to emphasize the uncertainty in the carbon fertilization effect.

2. *ES, Page 6, Line 37 (Sohngen): The point about biomass energy and land use is interesting, but there really is not enough supporting information in the text for readers to figure out what it means. I would recommend either more detail in the text on the land use changes implied, or that the authors modify this point. For example, you don't really talk at all about land use patterns in this document.*

   Rather than add a more complete explanation here, the point was simplified to say that biomass crops are important and limited by competition with agriculture and forestry.

3. *ES, Page 7, Line 5 (Sohngen): I'm not at all certain about what you mean about terrestrial systems saturating. I would suggest eliminating this term throughout the text and modifying the discussion. First, there are no payments for carbon sequestration as far as I can tell, so I imagine that there are opportunities to continue increasing C sequestration beyond the levels implied in these models if landowners were paid. Second, the two models (IGSM and Minicam) both have endogenous land use as far as I can tell. Thus, the overall landscape probably isn't saturated because a change in policy or prices would lead to a different level of carbon stored in land. The use of the term saturated is very confusing.*

   The paragraph was rewritten to avoid use of the term "saturation" and instead to use the concept of equilibrium.

4. *ES, Page 3, Lines 1-4 (Jones): This notes that "current policies" assumes that the Kyoto Protocol and the "President's carbon intensity target" (actually, it is a GHG intensity target) are assumed to end in 2012. These are reasonable but critical assumptions and it would be helpful to highlight the assumptions – perhaps simply with bullets.*

   No change here. The assumption is clear.

5. *ES, Page 3, Lines 1-4 (Jones): This is a US analysis and "President" is understood. However, since this product is likely to be read internationally, for clarity why not say "US" instead of "President"?*

   This change was made.

6. *ES, Page 4, line 11 (Jones): This says that energy demand growth in developing countries "may be even larger" than in developed countries. If energy demand growth in developing countries in the reference case is larger than in developed countries, then say so, don't just hint that it might be.*

   The sentence was rewritten to clarify the projected levels.

7. *ES, Page 4, Line 43-44 (Jones): While acknowledging that the growth of nuclear energy depends in part on "non-climate policy considerations," the role of fossil fuels in the overall energy system is described on this page in percentage terms. The role of nuclear should be described similarly in the Executive Summary. Here, like in many parts of the analysis, there is no "right" answer. But people need to know what is in the reference case. The same goes for renewables. There is a huge difference in nuclear generation across the three models in the reference case (see Figure 4-11) that should be noted in the text. And the responsiveness of nuclear energy, or lack of it, to rising carbon prices across the three models may go a long way to*

explaining differences across models in the energy system response to stabilization scenarios.

A sentence was added to describe the nuclear difference in percentage terms.

8. *ES, Page 5, line 33 (Jones): Here and in other places, I found the description of land use or a "responsive terrestrial land system" generally too sparse to be understand what the models include and how that might change in a stabilization scenario.*

   This paragraph was rewritten to clarify this concept and clarify the difference between the models.

9. *ES, Page 7, Line 29 (Jones): Given the challenge of this effort, there may be no choice but to assume "universal participation by the world's nations" starting in 2015. But this is not a minor assumption -- it is a huge assumption and needs to be highlighted. And because of this, the geographic distribution of emission reductions from the reference case needs to be clearly presented – as opposed to being totally avoided.*

   A paragraph was added right at the start of this section to emphasize the importance of these assumptions to ALL of the results.

10. *ES, Page 7, lines 34-36 (Jones): It is useful to know that a common finding is that the value of carbon starts with a low carbon price that gradually rises over time. This is fine, but what is "low"? People will ask and not providing that information initially will cause more problems than it will avoid. Not only is this not covered in the Executive Summary, even in Section 4.6 there is virtually nothing in the text giving carbon price results – only a figure (4.18) where most prices over the next 50 years are so close to the horizontal axis that it is impossible to know what they are.*

    The paragraph was rewritten to insert the actual numbers.

11. *ES, General (Aldy): Context for Scenarios: The description of the various scenarios would benefit from additional context provided in the existing literature. For example, the bullet point on page 5, lines 40-44 provides the concentrations of the various greenhouse gases in 2100 under the reference scenarios developed with these three models. How do the emissions and concentrations profiles compare with the scenarios used to estimate the climate impacts presented in the Summary for Policymakers of Working Group I's contribution to the IPCC Third Assessment Report? How do these profiles compare with the scenarios presented in the IPCC Special Report on Emissions Scenarios? Some readers will approach this report with priors based on these two IPCC reports (among other literature), and this*

comparison will give them a better sense of how this work differs from the literature. This would also be of value for the stabilization scenarios. How are these results different from Wigley, Richels, and Edmonds 1996? How are they different from the Stanford Energy Modeling Forum's EMF-14 work? What explains the differences?

Comparisons to previous scenarios would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

12. *ES, General (Aldy): Economic Costs: The discussion of the economic effects of the various stabilization scenarios was too vague and provided little value-added. Anyone familiar with the literature on cost-effective long-term emissions pathways could easily write the first and third bullet points on page 7 (lines 34-36 and 42-45), and could probably write the second bullet point as well (page 7, lines 38-40), simply by being told that three different models were used to construct cost-effective emissions pathways to a long-term stabilization goal. Since some of the audience for this report will read little beyond the executive summary, additional detail on the economic effects of stabilization policy would benefit this section. First, some of the difference in carbon prices across models reflects the differences in the reference scenarios. This is not clear from the second bullet point. Second, some measure of the cost of emissions mitigation should be included. Even if this focused on just the U.S. costs, that would be fine, just to provide a sense of magnitude for the reader. Various measures of cost have been provided before in the literature: present discounted value of foregone consumption, present discounted value of GDP change, etc. The report could compare U.S. per capita income under the reference scenario and the various stabilization scenarios in 2025, 2050, and 2100 to illustrate the economic effects of stabilization. Of course, these would reflect simply the economic effects of emissions mitigation, and exclude both market and non-market benefits associated with climate change mitigation. Such a caveat should be included when discussing the economic burden of the stabilization scenarios. Third, a comparison of costs across the various stabilization scenarios would be useful. How much more expensive is it to move from 650 ppm to 550 ppm to 450 ppm? There is no discussion of this at all in the economic effects section of the executive summary. Additional discussion of the economic effects of stabilization policies should also be presented in section 4.*

    (a) The paragraph on carbon prices has been expanded to give actual numbers. (b) The second bullet point has been rewritten to make more clear what underlying assumptions, including the reference case differences, contribute most to the inter-model difference. (c) Results for economic impacts in terms of Gross Global Product have been added, with an additional table and figure in Chapter 4, and a summary bullet paragraph on these results has been added here.

13. *ES, General (Aldy): What Flexibility: The executive summary describes the assumptions employed to ensure cost-effective implementation of the stabilization scenarios, including "where" and "when" flexibility (page 7, lines 26-32). The executive summary is silent on the so-called "what" flexibility – that opportunities to effectively trade across gases and sources and sinks of a given greenhouse gas – can also reduce costs. After reading the rest of the report, it appears that the models did not fully incorporate all of the cost-saving flexibility that one might associate with intergas source and sink trading. For example, the MERGE model is neutral with respect to biological sequestration while the other models are not. The IGSM model employs quantitative limits on methane emissions instead of attempting to allow for a clearing price across gases. Granted, this is very challenging to do. Some analyses have modeled intergas trading based on global warming potentials (GWPs), but these are time-sensitive and may result in biased results relative to the preferred approach of accounting for the climate damages of the various gases. (The MIT modeling team has shown this very effectively in some of their prior modeling work.) It would be useful to at least address this point briefly in the introduction and more transparently in the report (section 4.6.2 was not very clear about the assumptions made underlying the pricing of non-CO_2 gases). It is not necessary that these models retool to do such intergas trading in a different manner than was undertaken for this effort, but it would be valuable to address this issue explicitly in the report.*

    The discussion of "what" flexibility has been sharpened in the Executive Summary. In addition, the approaches to tradeoffs between gases are discussed primarily in Chapter 4, and the discussion there has been sharpened. The discussion in Chapter 4 provides information on the relative prices of the non-CO_2 gases and CO_2, which provides insight into the tradeoffs raised by the reviewer.

Chapter 1 Comments

1. *Chapter 1, Page 1, lines 31-32 (Jones): The commitment to providing scenario details in a separate data archive is critical to the usefulness of this effort. Presumably this will be part of the public record.*

   The dataset will be provided as part of the final public record.

2. *Chapter 1, Page 4, lines 22 (Jones): Yes, "humans also alter the land surface…." Perhaps I read too fast, but I am still uncertain exactly what the models include (and how) and exclude. If the inclusion of factors is limited, or our understanding limited (with more and more research apparently indicating humans may have substantial impacts on climate via non-GHG impacts), then it is important to be clear what the models include and what they don't. Claiming that "land-use change" is "included" in the models has great potential for overstating what we really know and can model about the many aspects of land use by humans.*

   Qualifications applicable to the land surface models have been added and strengthened in Chapter 2.

3. *Chapter 1, Page 8, line 41-42 (Jones): The text notes that past 2012, all climate polices were removed and described this as "one possible, albeit extreme, assumption." This is a critically key definition of the reference case. One the one hand, it seems totally implausible that all climate policies will be removed. On the other hand, any other assumption requires a level of specificity that would be difficult to justify. My suggestion is to highlight this assumption with a text box, combined with a short explanation why it was appropriate and/or necessary.*

   The text has been expanded to clarify the role of the reference scenario and emphasize that it is not a prediction of a most likely outcome.

4. *Chapter 1, page 3, line 42 (Aldy): This should refer to section 1.5, not 1.6.*

   The correction has been made.

5. *Chapter 1, page 6, lines 1-4 (Aldy): What does it mean that "computer codes are available that calculate the net forcing of a group of gases"? Is this intended to simply say that one can model the net effect of atmospheric concentrations of various gases on radiative forcing? The term "computer codes" seems odd.*

   These sentences were rewritten to clarify the multi-gas issues.

6. *Chapter 1, page 4 line 10 (Chameides, Wang): A change in temperature of 55 deg F is equivalent to a change of ~30 deg C, not 13 deg C.*

   The correction has been made.

7. *Chapter 1, page 11, lines 30-32 (Chameides, Wang): This makes it sound like the SRES didn't consider non-CO_2 gases at all. What is actually meant is that the IPCC didn't consider non-CO_2 gases in its stabilization scenarios.*

   The sentence was removed.

8. *Chapter 1, page 4, line 10 (Rind): 55°F is 31°C.*

   The correction has been made.

Chapter 2 Comments

1. *Chapter 2: General (Sohngen): For IGSM, what is a multi-box approach? (p3)*

   This is a reference to a description of MiniCam. The model is explained more fully later, and this descriptor is not needed at this point, so it was removed.

2. *Chapter 2: General (Sohngen): Page 9, lines 20 -21: The term neutral here should be defined and put into context. It becomes clearer later in chapter 4, but here you should mention that no changes in the biosphere are assumed in any of the scenarios, although bioenergy options are modeled.*

   The term "neutral" in this context has been better defined in the text, and description of each of the models has been expanded to make more clear what kinds of simplifications are included. Also, a stronger statement of humility has been included at the start of the section.

3. *Chapter 2, Page 3, lines 3-6 (Jones): This caveat on the "differences among their [model] results" is an important caveat that needs to be included in the overall CCSP Product Executive Summary.*

   The authors agree regarding the importance of this point.

4. *Chapter 2, Page 3, lines 6-9 (Jones): Is there a analytic basis for the phrase "is likely within the range…"? If not, does this sentence belong here? If there is a basis, it should be provided.*

   A citation to uncertainty analysis of the IGSM has been added as justification.

5. *Chapter 2, Section 2.2.2, Page 3-4 (Jones): While this section is useful, it is very short and **does not address a key issue** – in each model, what if any impact does much higher delivered energy prices (fuel price plus carbon permit cost) have on economic growth in each model? Similarly, Chapter 4 (see for example section 4.6 – Economic Consequences of Stabilization) – says virtually nothing about any impacts on GDP or economic activity. Is the reader to assume that all three models indicate that there are no economic consequences to GHG concentration stabilization? Given that under the level 1 scenario in 2100, the ISGM carbon price has a carbon price of $6,053/ MtC, a no-impact result simply does not seem plausible.*

   A sentence was added to Chapter 2 to make clear that stabilization measures cause changes in economic activity. In addition, the discussion of economic impacts has been expanded substantially in Chapter 4, and now includes information on GDP losses from stabilization.

6. *Chapter 2, Sections 2.2.3 through 2.2.5, Pages 5-8 (Jones): While the material in these sections are useful general descriptions, it also is a "trust-us black box" description of the guts of the three models. Lacking in the report is any sense of the relative cost of resource and especially technology options and how they change over a century. This is the type of information that drives the fuel use trajectory in the reference case as well as the substitution away from GHG emitting technologies under the stabilization scenarios. Yet there are no tables or figures to illustrate these key underlying assumptions. In the interest of credibility, such information is needed.*

   The authors agree that greater information on underlying technology assumptions would be valuable. However, the participating models employ very different approaches for representing technology, so it was not feasible to present technology assumptions across models in a methodologically consistent fashion. This section has been rewritten to better describe the different ways technology is handled in the three models, to explain why detailed comparisons among them of specific technologies are problematic, and to highlight technology assumptions that play an important role in the results shown in Chapters 3 and 4.

7. *Chapter 2, Section 2.2.6, Page 8-9 (Jones): This section describes in somewhat abbreviated terms what the models cover. This area of research likely will turn out to become increasingly important over time as knowledge improves. In the mean time, it is probably important not to overstate our understanding or knowledge in this area. Unless care is taken with the text, at some point a press release will probably say the models "include land use change," as if we really understood this area well.*

   This section has been extensively revised to clarify the simplifications imposed and to stress the fact that land-use is much less well modeled than the energy aspects of the analysis.

8. *Chapter 2, Page 10, line 18 (Jones): This says, "In MERGE, methane production from natural gas use is tied directly to the level of natural gas consumption." Should "methane production" really be "methane emissions"? If not, I don't know what this sentence means. And if it means methane emissions, it ignores significant improvements in emissions control technology that have occurred and are occurring through programs like EPA's Natural Gas Star, Methane to Markets, and the World Bank's Global Gas Flaring Reduction program. To assume that technology here is standing still is seriously unrealistic.*

   This sentence has been corrected to refer to emissions, and the discussion has been revised to make clear that methane emissions as a function of natural gas consumption do indeed improve over time in MERGE.

9. *Chapter 2, Box 2.2, Page 11 (Jones): This starts out by saying that the concept of atmospheric lifetime for CO_2 may be "potentially misleading". So I started reading the Box thinking I would be told how not to misunderstand the concept. I'm not sure this box helps.*

   This box has been heavily edited to clarify why the concept of lifetime is misleading and better describe the element of the carbon cycle.

10. *Chapter 2, Page 1, line 43 (Chameides, Wang): The correct URL is http://web.mit.edu/globalchange*

    The web address has been corrected.

11. *Chapter 2, Page 9, lines 17-18 (Chameides, Wang): This statement seems incorrect. It is reported later on that deforestation occurs in the IGSM model, although at a decreasing rate. If so, how could the land-use pattern be unchanged over the simulation period? Perhaps the authors meant to say that the land-use pattern is unchanged among scenarios?*

    The text has been extended to clarify the fact that the biogeography of the terrestrial model is not now revised to reflect the land-use change implied by assumed deforestataion.

12. *Chapter 2, Page 12, line 4 (Chameides, Wang): Does the MERGE model really account for damages related to climate change in this analysis? If so, please provide details.*

    A sentence has been added to clarify that this feature was not used in the current study.

13. *Chapter 2, Page 3, end of first paragraph (Rind): How does one know this?*

    A citation to uncertainty analysis of the IGSM model has been added as justification.

Chapter 3 Comments

1. *Chapter 3, General (Sohngen): I was surprised that the modelers did not try to come up with at least 1 set of consistent assumptions about population they could use across all the models. It would help in making comparisons of the reference case.*

   The modeling teams have followed the approach to scenario development prescribed in the Prospectus, which called for independent development of scenario assumptions. However, as noted in the text, the scenarios were not entirely independent due to the fact that the modeling teams met regularly to discuss progress and to share preliminary results.

2. *Chapter 3, General (Sohngen): It would be useful to get a sense for the scale of biomass energy and land use. That is, can you relate exajoules to land use (hectares/exajoule/yr) so that readers can get a sense for the scale of production in the world and in the US. This also would be helpful for analyzing the land use assertions made in the executive summary.*

   This information was not provided because only the EPPA and MiniCAM models include such information.

3. *Chapter 3, General (Sohngen): More discussion about the role of sinks would be useful. Figure 3.12 shows a net sink for world forests currently, with an increasing sink due to CO_2 fertilization. The authors have rightly pointed out uncertainty with respect to sinks, but IGSM and Minicam suggest sinks on the order of 500 - 700 Tg C/yr initially. This is consistent with the inversion models, but it ignores other published literature suggesting that forests in temperate regions do not approach this and that forests in tropical regions are going the other direction in the order of 600 - 1200 Tg C/yr. The net effects appear to be somewhere around 400 - 900 Tg C/yr of additional emissions, not sequestration (with some authors having results as high as 2100 Tg C/yr). This ought to be mentioned as a major uncertainty associated with the sink estimates. Also the IGSM and Minicam models should provide slightly more detail on how the CO_2 fertilization effects are calculated - i.e., do they calculate NPP in ecosystems and base the CO_2 fertilization effects on NPP changes, or do they use some other measures of ecosystem productivity changes over time. Finally, the authors should acknowledge continued uncertainty in the effects of CO_2 fertilization and climate change on net ecosystem productivity (same point as above).*

   In the spirit of this comment, the discussion of the land use and terrestrial biosphere components of the models more generally has been expanded in Chapter 2. The discussion better acknowledges the limitations of these components of the participating models.

4. *Chapter 3, General (Sohngen): A related point is that assumptions about the effects of CO_2 fertilization on the biomass sector should be made clear. Do the authors assume that the biomass sector is becoming more productive due to technology change and CO_2 fertilization, or one or the other, or neither? My reading of the entire text is that the biomass option is important at the margin. Thus, I think some more detail on it would be useful.*

   This point is now addressed in Chapter 2. A paragraph has been added at the end of the chapter that discusses that the models are not fully closed in a number of ways.

5. *Chapter 3, General (Sohngen) Can you provide an estimate of biofuel prices?*

   The market price dynamics for biofuels in all three models is linked tightly with the price dynamics of oil, as biofuels serve as a substitute for many petroleum products, including gasoline. Hence, the dynamics of the oil price should be used as a proxy for the dynamics of the biofuels prices.

6. *Chapter 3, pages 3-7 (Jones): The text regarding the reference cases sort of acknowledges past debates over long-run projections GDP and per capita GDP for different countries/regions. However, the lack of specificity about regional GDP and per capita GDP projections between now and 2100 in the three models' reference cases simply implies that the CCSP doesn't want us to know the details of the reference cases. It is probably far better to clearly identify how the models work and what their results are (e.g., PPP or MER based data, etc.) and acknowledge that there is not a good clean answer here, than to not provide the results you know people will want to see.*

   The authors agree and have provided regional population and GDP numbers from the scenarios.

7. *Similarly, there should be graphical presentations of these results by country/region – not just for GDP and per capita GDP, but also for carbon emissions (although Figure 3-15 does that to a limited extent). The text on page 7 (lines 2-10) already acknowledges uncertainty here. The nature of that uncertainty is useful information for policymakers.*

   Reporting regionally disaggregated information would dramatically increase the length of this report and the associated reporting requirements from the participating modeling teams. The authors have therefore chosen to focus primarily on the world and the USA.

8. *Chapter 3, page 7, line 2 (Jones): Are these "forecasts" or "projections"?*

   Good point. The authors have changed the term "forecasts" to "scenarios".

9. *Chapter 3, pages 7-13 (Jones): Overall, the text here contains useful descriptive material about trends and differences between the models. However, Section 3.3.2. Trends in Fuel Prices could be strengthened by addressing any links between relative energy prices and energy use across the models. Why is it, for example, that IGSM (figure 3.7) has the highest world oil price but has more oil consumption (Figure 3.3) then either MERGE or MiniCAM? Is it energy prices driving choices that lead to that result, or differences in assumptions about technology change. If there was more information on the underlying technology options presented (see comment on Pages 5-8, Sections 2.2.3 through 2.2.5), then one could better understand this apparent inconsistency.*

   The results reported here are the consequences of the entire suite of assumptions that constitute the models' scenarios. Where the modelers were able to clearly understand the comparative behavior of the models, this has been reported. However, this example does not have an obvious explanation. In addition to the explanations suggested by this comment one could add differences in fossil fuel and non-fossil fuel resource assumptions. However, without a much more complete model intercomparison, it is not possible to attribute many results to specific differences across models. The authors have therefore not been able to provide more explanation on this point. Wherever possible, the authors do try to provide explanations of scenario differences.

10. *Chapter 3, page 11-12 (Jones): It is probably too late for this, but as crude oil prices increase in the reference case, it frequently becomes profitably to use CO_2 enhanced recovery to boost oil production. However, it does not appear the petroleum with CCS is in any of the reference cases. Was this technology trend considered in developing oil price and production trends, or is it simply a victim of the assumption of "no climate policies" after 2012?*

   While the models assume that it is possible to get to increasingly less attractive grades of oil resources, which implicitly includes the use of CO_2 for enhanced oil recovery, none explicitly link oil production and CO_2 storage. There could be potentially an inconsistency between model assumptions about tertiary oil production and CO_2 storage. Some of the models do distinguish CO_2 reservoir types with EOR representing the cheapest opportunities. However, as noted there is no direct linkage to oil production. On the other hand, the potential for this to affect aggregate model outputs is relatively small. The models assume that the oil can be recovered; they merely neglect to directly associate the CO_2. Similarly, the models assume that CO_2 can be captured and stored in geologic reservoirs, including in oil and gas fields. Give the limited volumes of CO_2 that are at issue—at least relative to total CO_2 capture and storage—the potential inconsistency is relatively minor. The models have not been modified to eliminate this potential inconsistency.

11. *Chapter 3, Page 2, Figure 3.3 (Jones): There is a remarkable divergence in the projections of energy mix (especially coal versus petroleum) for the US across the three models. Hopefully this leads to some useful understanding of how emission reductions occur in Chapter 4.*

    The authors agree and appreciate the comment.

12. *Chapter 3, Page 3, Figure 3.4 (Jones): This references growth of global per capita energy use as being half US. In fairness, a comparison also should be made for GDP per capita and CO_2 emissions per unit of GDP.*

    Good point. The authors have added CO_2 emissions per capita in the two regions to the document. The authors have also provided GDP and population by region, which can used to develop GDP per capita. Because GDP per capita is inherently difficult to compare (see box in Chapter 3) the authors have decided not to specifically report GDP per capita.

13. *Chapter 3, Page 5, Figure 3.7 (Jones): There are huge differences in price projections. The implications for energy use and carbon emissions across the models should be addressed more extensively.*

    Energy prices are model outputs. They do not drive carbon emissions, but are, instead, a result of the same interactions that govern carbon emissions. Thus, although these prices have implications for model outputs, more importantly they are a reflection of model inputs. For example, assumptions about relative availability of different fossil fuel resources and demands for different forms of energy over time are reflected in both energy prices and carbon emissions. The discussion in section 3.5.1 therefore focuses less on energy prices than on the forces that drive both energy prices and carbon emissions.

14. *Chapter 3, Page 13, Figure 3.16 (Jones): It would be useful if the three CCSP reference scenarios could be noted on this figure.*

    The authors may add the CCSP reference scenarios to the figure in the next version of this report if they can obtain the original data.

15. *Chapter 3, Page 19, Table 3.1 & 3.2 (Jones): New tables need to be added giving country/region GDP and per capita GDP levels for key dates. This information drives energy use, so you might as well show it now and not take grief later for not showing it.*

    This has been done.

16. *Chapter 3, Page 1, Lines 23-24 (Aldy): The text should note the pre-industrial level of radiative forcing to provide context for these results.*

    By definition, the preindustrial level of radiative forcing is zero.

17. *Chapter 3, page 17, lines 13-21 (Aldy): The Framework Convention on Climate Change references to Annex I and non-Annex I countries is neither entirely transparent nor exact. For example, some of the countries in the Eastern Europe and former Soviet Union modules in these models are not Annex I countries. It would probably be better to simply refer to these as industrialized and developing countries.*

    The authors have added a footnote to make this relationship clearer.

18. *Chapter 3, Figures 3.1-15 (Tol): Please extend the graphs to include 1900-2000.*

    Development and inclusion of historical data in all the figures was not called for in the Prospectus and might constitute substantial additional effort. It has therefore not been attempted here. However, the authors agree on the value of such historical information and will consider the possibility of including it in the next version of the report if time and resources permit.

19. *Chapter 3, Figure 3.7 (Tol): Please add the growth rates, perhaps in the legend. Some historical comparison would be good.*

    The authors are not convinced that calculating average annual growth rates for variables that are not essentially log linear would be of much help. The authors are, however, sympathetic with the idea of adding historical data as for example, from Table 3.6.

20. *Chapter 3, Figure 3.14 (Tol): Please add a graph for the USA.*

    The figure has been added.

21. *Chapter 3, General (Tol): Please add a graph with income per capita. IGSM has $190,000 in 2100, MERGE only $115,000. This disparity is somewhat hidden in the text; the difference is larger than twice the current average income. The numbers are mind-boggling.*

    Income and population are now both provided in tables. Per capita income can be determined from these two tables. However, because income per capita is inherently difficult to compare (see box in Chapter 3) the authors have decided not to explicitly report income per capita.

22. *Chapter 3, Table 3.2 (Tol): a PPP method > a PPP method (Geary-Khamis)*

The change has been made.

23. *Chapter 3, Page 3, Lines 28-29 (Chameides, Wang): This sentence is out of place, since the rest of the paragraph discusses population, not GDP.*

    The authors have broken the paragraph into two pieces to reflect the two different topics.

24. *Chapter 3, Page 4, Lines 17-37 (Chameides, Wang): The writing in this section contains much repetition.*

    The repetition is with the summary paragraph. The authors think this is acceptable.

25. *Chapter 3, Page 7, Line 14 and Line 40 (Chameides, Wang): Not true. Reword to something like "projected to be largely tied to…"*

    The sentence has been reworded to read as follows: "In the three reference scenarios energy production is closely associated with emissions of GHGs, particularly CO_2."

26. *Chapter 3, Page 7, Line 31 (Chameides, Wang): Replace "these" with "non-fossil"*

    The change has been made.

27. *Chapter 3, Page 7, Line 34 (Chameides, Wang): Should there be a "not" right after "does"?*

    The paragraph has been changed.

28. *Chapter 3, Page 14, Lines 27-28 (Chameides, Wang): How is the net land-use change prescribed? Please provide details. Is deforestation/land-use rates used for the base case adopted from the EDGAR inventory? This probably underestimated present-day rates and over-estimates the rate of decrease in deforestation in the coming decades.*

    The discussion of land use throughout the document has been substantially revised to better reflect the approaches taken by the models and to better acknowledge the limitations of these components of the participating models. For more details than are provided in this report, readers are encouraged to review the technical documentation for each of the participating models.

29. *Chapter 3, Page 14, Line 30 (Chameides, Wang): Add the phrase "from scenario to scenario" at the end of the sentence.*

The change has been made.

30. *Chapter 3, Page 18, Lines 16-25 (Chameides, Wang): This section is vague, needs more explanation.*

    It is hard to know what was vague and in need of further explanation. The authors have not changed the paragraph.

31. *Chapter 3, Page 21, Lines 19-21 (Chameides, Wang): Specify that these values are for a 100-year time horizon.*

    The change has been made.

32. *Chapter 3, Figure 3.18 (Chameides, Wang): State why only 2 model results are shown.*

    Results from all three models are now included.

33. *Chapter 3, Table 3.1 and 3.2 (Chameides, Wang): Indicate units.*

    The change has been made.

34. *Chapter 3, Figure 3.12 (Chameides, Wang): Is the assumed decrease in deforestation over time unrealistic?*

    The figure shows net emissions of CO_2 from terrestrial systems. Thus, it is an aggregation of land-use change emissions, forest re-growth, and other natural processes such as the CO_2 fertilization effect. It is impossible to tell from this figure what is happening with individual components.

35. *Chapter 3, Figure 3.16, line 1 (Chameides, Wang): Insert "Fossil" between "Global" and "Carbon"*

    The change has been made.

36. *Chapter 3, Page 5, first main paragraph (Rind): Considering the historical growth rates since the 1970s for Africa and Latin America in particular, the assumed growth rates seem overly optimistic.*

    As directed by the Prospectus, the modeling teams have used assumptions that they believe are plausible and meaningful. Assigning likelihoods to assumptions would be part of a formal uncertainty analysis, which would constitute a substantial expansion of the scope of effort

37. *Chapter 3, Page 7, Line 40 (Rind): "CO_2" looks like it is subscripted.*

The change has been made.

38. *Chapter 3, Page 13, First Paragraph (Rind): has hydrogen-powered or hybrid vehicles been considered in these projections?*

Technology detail varies between models. Both are explicitly considered in MiniCAM, for example.

Chapter 4 Comments

1. *Chapter 4, Page 3 (Sohngen): The notion of saturation in the sinks needs to be clarified. As I read these scenarios, there probably is not a point where saturation would occur. To me, "saturation" in the aggregate sense described here occurs either when all land has been converted to forests and climate change has stopped affecting forests, or when all land has been converted to biomass and we are running as much C through the landscape every year that we can, or something else. But since not all land is forests, or not all land is biomass energy crops, then I imagine that some additional changes are possible and things aren't quite saturated. More land could still be converted to biomass and therefore land could be an even larger "sink," even if it's not economically efficient given the prices under the different stabilization scenarios? Anyways, my recommendation is to drop the discussion about saturation because I think it confuses people.*

   Saturation in this sentence refers not to a cessation of uptake in sinks, but to a slowing of uptake.

2. *Chapter 4, General (Sohngen): The oceans and forests are two key components that are essentially left uncontrolled, although they enter the models in different ways. However, they would seem to be fairly important. They also are both highly uncertain, as noted by the authors. One of the models ignores forests/sinks. Could all the authors run some scenarios where they turn off the sinks and oceans, and see what the implications on the scenarios are of not having these apparently important model components?*

   For instance, figure 4.6 suggests that in 2050, for level 3, IGSM, Merge and Minicam would require 6, 2, and 2 (respectively) fewer Gt C/yr to meet the stabilization target. According to figure 4.17, for IGSM CO_2 fertilization is providing 2 of these Gt C (or a 1/3), and for Minicam, it is providing around 1.5 Gt C or so (or ¾). This seems relatively important, particularly when compared to MERGE which ignores sinks but has a backstop technology at $1000/t C. I would encourage the IGSM and Minicam modelers to test the sensitivity of their baselines and stabilization scenarios against the MERGE assumption of a neutral sink, which would provide some indication about the importance of CO_2 fertilization, and potentially sink enhancements in the context of these stabilization scenarios.

   Sensitivity analysis would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

3. *Chapter 4, General (Sohngen): It would be useful to present figure 4.6 like figure 4.17, i.e. the reference and deviations from the reference on the same figure for each model.*

The authors have discussed at length the appropriate forms for the different figures that support this report, including those just mentioned. There are benefits to both approaches to presenting the information. The authors will revisit this question in the next version of the report with these comments in mind.

4. *Chapter 4, General (Sohngen): The result from Minicam with large deforestation losses in level 1 needs to be better explained. First, the deforestation effects seem astoundingly large. There is some discussion of this, but it could be better described about where this occurs since it's so big. Second, and more importantly, I'm not sure I understand the logic of modeling deforestation for biomass energy without also modeling deforestation for food consumption (i.e., baseline deforestation). Perhaps other deforestation flows are monitored or modeled? But as currently described, it looks to me like the biosphere in the baseline is treated mainly as a sink due to CO_2 fertilization, except when land serves as an input into biomass energy. How can deforestation be ignored in the baseline (when lots of studies have already pointed out the potential importance of deforestation in the C cycle), but be important in one of the policy scenarios?*

   Results are now changed as MiniCAM now places an economic value on terrestrial carbon emissions just as it does on fossil fuel carbon. The authors have also attempted to do a better job describing the MiniCAM treatment of land-use change emissions in the text. All land-uses compete for land in MiniCAM and that land use directly affects the distribution of stocks of terrestrial carbon. Thus, in MiniCAM there is net deforestation in the reference case caused by the increasing demands for land for agricultural and other uses. This peaks and then declines in the latter half of the century as agricultural productivity catches up with the slowing rate of population growth and saturation of food demands. However, when an additional demand for land is added, that demand can only be satisfied if it can successfully compete against other land uses. That increased demand for land leads to dramatic increased pressure on unmanaged ecosystems in the stabilization cases.

5. *Chapter 4, Page 12 (Sohngen): The authors make a point about uncertainty with geological storage, which is untested. Yet, the numbers shown in tables 4.4 - 4.6 suggest that for roughly $14 - $97/t C, 5 - 8 Gt C could be stored by 2050, or for $37 - $245/t C, 13 - 19 Gt C could be stored. Forest sink enhancements are probably equally uncertain and untested, but suggest far larger source potential, on the order of 35 Gt C by 2050 for $97/t C or 80 Gt C for $245/t C. To some extent, the free sink provided by CO_2 fertilization accounts for this in IGSM and Minicam. The only point here is that I think the paper would benefit from some additional discussion about the uncertainties and unknowns in the abatement technology sector. Maybe a table, and*

accompanying discussion, that tries to highlight what we know about the different technologies or mitigation options (just the ones that show up in the figures) and their certainty (or uncertainty) at this stage would be very useful to put the numbers in context.

The authors agree that formal consideration of uncertainty would be a valuable follow-on to this work. However, the Prospectus for this product called only for the modeling teams to use assumptions for key drivers that they believed to be “meaningful” and “plausible” and did not request uncertainty analysis more generally. Consideration of the relative uncertainties associated with different technological options would constitute a substantial expansion of the scope of the effort. Please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

6. *Chapter 4, Section 4.2.3, Page 3 (Jones): Policy Instrument Assumptions in Stabilization Scenarios. This briefly mentions when/where flexibility. Results later in the chapter cover “when” flexibility, but no results are shown for “where” flexibility. Without that information, the credibility of the results is not great. If a huge percentage of the avoided emissions are, for example, in China or India, policymakers need to know that. This is sort of referenced on page 8, lines 26-28, but there are no tables/figures that comprehensively address the “where” flexibility aspect of the scenarios.*

   Although the report does not provide country-by-country detail, the United States and the world are both provided and it is clear that most emissions mitigation occurs outside of the United States. Since each of the models has a different composition of emissions between Annex I and non-Annex I nations, the relative importance of emissions mitigation between these two groups will vary from model to model.

7. *Chapter 4, Page 10, Lines 25-46 plus (Jones): Paragraphs like this are very useful in understanding why the results occur. No single model is probably “right” or “wrong”, but understanding the “why” of different results helps advance understanding of options.*

   The authors appreciate the comment.

8. *Chapter 4, Page 12 (Jones): Carbon Capture and Storage Discussion. Lines 42-43 say that “if CCS were unavailable, the effect on cost would be adverse.” That only hints at the real issue. A key issue is at what price CCS “is available”, and that information is never provided in this report. As noted earlier, choices across technologies depend on the fuel they use, the carbon price, and the cost of that technology. The end results discussed here are interesting, but they depend on information not provided in this report.*

It would be useful for a policy maker to know what technologies could make a big difference, but was not used in a scenario because it/they cost "10%" too much. That information could be used to promote R&D or provide incentives to the private sector to under take R&D that might lower a technology cost.

In that respect, it would be very useful to undertake sensitivity scenarios that alter technology costs. This would provide a better indication of the importance of each technology, and its potential to reduce emissions.

(a) The discussion in Chapter 2 on technologies has been expanded to consider this issue. The revised discussion in Chapter 2 notes that the models are of such fundamentally different character that it is impossible to develop a meaningful side-by-side comparison. Thus, the report attempts to explain differences when they are important for illuminating differences in the scenarios. (b) Sensitivity analysis would constitute an expansion of the scope of effort; please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

9. *Chapter 4, Page 3/Table 4.6 (Jones): Some of the prices in this table are so large is to strain credibility. What is the value of the energy service being provided with a carbon price of $6,053/MtC? Is there a way to put these prices into a useful perspective? It would be useful to see stacked bar graphs of energy prices and carbon prices. Figure 4.21 has energy prices but without the carbon price component. This is interesting but it does not tell you what consumers pay.*

    The information needed to make that calculation is provided in Table 4.7.

10. *Chapter 4, Page 12/Figure 4.9 (Jones): These types of charts are a useful way to portray information. I regret that I haven't be able to focus on them yet. Figure 4.14 is particularly interesting because of its US focus.*

    The authors appreciate the comment.

11. *Chapter 4, Page 28/Figure 4.18 (Jones): Because of the high IGSM carbon prices, the scale of the graphs is such that it makes the MERGE and MiniCAM prices for most years impossible to follow. Similarly for Figure 4.19: the 2050 and 2100 figures have the same scale, but that makes most of the 2050 results difficult to follow. Try different scales.*

    Leaving the same scale allows for a relative visual comparison of the prices among scenarios. The details are provided in Table 4.6. The authors have added more detail to Table 4.6 with additional data for the years 2020 and 2030 to augment existing values for 2050 and 2100.

12. *Chapter 4, Page 3, Lines 23-27 (Aldy): This text provides the reasons for modest abatement in the near term and more substantial abatement in the longer term to meet various stabilization targets. I have a vague recollection that the Wigley, Richels, and Edmonds work also notes that such a pathway can take advantage of the carbon cycle – some of the carbon emitted today will not be in the atmosphere in 2100, so there is no climate benefit (at least in the distant future) associated with mitigating those carbon dioxide tons today.*

    The authors agree with the assessment. Over 1000 years cumulative emissions are fixed and a tonne of carbon in any period counts the same as a tonne of carbon in any other period. But, over shorter time scales removal rates play a role. That is why there is an additive term on the Hotelling rate to reflect the average annual removal rate of carbon from the atmosphere. Obviously that must asymptotically approach zero. But, at present it is about one-half percent per year. However, for the purposes of this document, the authors considered this to be too subtle to present.

13. *Chapter 4, Page 5, Lines 11-19 (Aldy): It is good that the text refers to the drawbacks in using GWPs for intergas trading. It would also be useful if the text could explain how the independent stabilization levels were set for CH_4 and N_2O.*

    A paragraph has been added to section 4.6.2 to explain that stabilization levels for CH_4 and N_2O are not determined by the modelers directly, but are derivative of the approach employed by the modelers. Furthermore, the concentrations of N_2O and CH_4 are not necessarily stabilized in the 21st century.

14. *Chapter 4, Page 8, Lines 16-18 and Lines 26-28 (Aldy): These two sentences, in successive paragraphs, do not appear to be consistent: "For Level 1… more than 75 percent of the emissions mitigation occurs in the second half of the 21st century." "So, when RF is restricted to Level I [sic] all three models find that more than half of the emissions mitigation occurs in Non-Annex I regions by 2050."*

    The first statement refers to the distribution of global emissions mitigation over time. The second statement refers to the distribution of emissions mitigation at a particular point in time across space. There is no obvious inconsistency.

15. *Chapter 4, Page 11, Lines 25-26 (Aldy): What does it mean that electricity can move to the carbon-free source of terrestrial sequestration?*

    The text has been changed to read: "Also, the long-term cost of transitioning to low and non-carbon emitting sources is relatively smaller than in the economy on average."

16. *Chapter 4, Page 16, Lines 34-36 (Aldy): What does it mean that the carbon price path is neither arbitrary nor the function of cost-effectiveness considerations? Earlier in the report, one gets the impression that emissions pathways to stabilization targets are constructed to minimize costs (e.g., Executive Summary, page 7 and chapter 4, section 4.2.3).*

    Only the MERGE model employs a pure intertemporal optimization modeling system. While the MiniCAM uses the Peck-Wan result that produces a price path that is consistent with a pure intertemporal optimization framework, the non-CO_2 GHG's are not priced using the same algorithm—it uses GWPs for non-CO_2 GHG prices. Thus, the resulting cost calculation is not precisely optimal. The EPPA model, like MiniCAM, does not use pure intertemporal optimization to set the prices of non-CO_2 GHG's. Furthermore, it uses a Hotelling path for CO_2 prices, but starts the first period with preexisting market distortions. Thus, it is not clear that an idealized cost-minimizing rule from a first-best world still produces a first-best solution in a world with preexisting market distortions. This reality makes it somewhat more difficult to craft an unqualified statement about cost-effectiveness.

    The following sentence has been removed: "However, the emissions path and resulting economic costs that emerge are not completely arbitrary nor are they the result solely of economic cost effectiveness considerations." The preceding sentence has been rewritten to read: "The similarity of the price paths, rising over time, reflects the similarity of economic approach employed by the three modeling teams as discussed in Section 4.2."

17. *Chapter 4, Page 4 (Tol): "The MiniCAM team employed … " This paragraph is too vague. What did they do?*

    The authors agree. This section has been completely rewritten.

18. *Chapter 4, Tables 4.2-3 (Tol): Please also specify the annual change in concentration in 2100.*

    This may be added in the final version.

19. *Chapter 4, Table 4.7 (Tol): Great information. Please add the same for the three models, or say that they use the same numbers.*

    The authors appreciate the comment. That is why the information was included. However, not only would each model give a different value, the models' values would be different in every period. Unfortunately this information can only be generated by sensitivity analysis. Sensitivity analysis was not prescribed in the Prospectus and would constitute a substantial expansion of this exercise. Please see the general comment on expansions to the scope of effort in the introduction to the responses to comments.

20. *Chapter 4, Figure 4.2 (Tol): It is probably better to give the differences from reference; or to put the five scenarios in one graph per model.*

    The two suggested alternatives are very different approaches to displaying the data. In any case all of the information is presented. Organizing it in different ways would doubtless shed light, but there is a limit to how much space can be allocated in this document to that task. The authors note however, that these data will be made available and that individuals will be able to construct alternative plots when the report is released.

21. *Chapter 4, Figures 4.3/6 (Tol): I like the format of Figure 4.4 much better.*

    There is discussion among the authors as to which presentation style is best. On the one hand Figure 4.4 has more information, but it is also more cluttered. This is helpful input. The authors will revisit this question for the next version of this report with these comments in mind.

22. *Chapter 4, Figure 4.4 (Tol): I don't understand IGSM. The text is not clear either, referring to a paper of Sarofim without further explanation. Why does methane have to be stabilized right away?*

    Absent a full specification of the damage function over time consistent with the emissions forecast and climate parameterization, any index is essentially arbitrary as shown in Reilly and Richards (1993) or Reilly, Babiker, and Mayer (2001), or as discussed in Reilly et al. (1999) or also in many other economic analyses of the GWP index issue. Optimizing based exclusively on the ultimate stabilization target ignores the potential for damage or benefits at levels approaching the stabilization goal. The cited article by Sarofim et al. (2005), acknowledges these various issues, and demonstrates that in the longer run stabilization of all substances individually becomes a practical requirement. Near term stabilization of methane is consistent with a view that near term climate change has significant risks, and therefore actions that have a relatively strong effect on climate in the near term have some value. A contrary view is that through 2050 or 2075 climate benefits are possible, and so controlling methane early would only avoid benefits, and thus, delay of methane control as is indicated by the Manne Richels approach is preferred. These two different approaches provide something close to bounds on how one might treat methane, and thus the IGSM approach is a useful alternative to the MERGE result.

23. *Chapter 4, Figure 4.21 (Tol): I don't understand the coal market in MERGE, or the gas market in MiniCAM and MERGE. Do you really think the price elasticity is near zero?*

The authors believe that the reviewer means to say that the price elasticity is near infinity. In fact the MiniCAM price elasticity of supply of natural gas in around 3. And, this is a direct reflection of the high availability of gas in the costlier grades of the resource. The same is true of MERGE.

24. *Chapter 4, Page 8, Line 10 (Chameides, Wang): The challenge grows in terms of cost or what?*

The mitigation challenge means literally the challenge of mitigation measured in terms of tonnes of carbon per year. The sentence can be read literally.

25. *Chapter 4, Page 8, Lines 24-28 (Chameides, Wang): There could be more in-depth discussion here. For example, does more emissions mitigation occur in non-Annex I regions due to the lower marginal cost of reducing emissions there (energy intensity is typically higher in developing economies)? And what is the final distribution of per capita emissions and income between Annex I and non-Annex I countries? We suggest including plots showing the breakdown of emissions reductions among regions.*

The authors have changed the text to make clear that the relative level of emissions reductions in Annex 1 and non-Annex 1 countries is tightly linked to the relative emissions in these regions in the reference cases. Consideration of the reference case emissions in Chapter 3 should therefore provide substantial insight into the relative emissions reductions. The authors have added the following phrase at the end: "Because the stabilization scenarios are based on the assumption that all regions of the world face the same price of greenhouse gas emissions and have access to the same technologies for mitigation, the resulting distribution of emissions mitigation between Annex I and Non-Annex I regions generally reflects the distribution of reference scenario emissions among them. So, when radiative forcing is restricted to Level I, all three models find that more than half of the emissions mitigation occurs in Non-Annex I regions by 2050 because more than half of reference case emissions occur in Non-Annex I regions."

26. *Chapter 4, Page 16, Lines 9-12 (Chameides, Wang): Should point out that this is a weakness in the model.*

The MiniCAM results now place a value on terrestrial carbon emissions. Thus the accelerated land-use change emissions are now described as a sensitivity case.

27. *Chapter 4, Figure 4.16 (Chameides, Wang). Why do the MERGE results oscillate? Is this ringing brought about from the assumption of intertemporal equilibrium?*

The MERGE results oscillate due to a combination of factors associated with the intertemporal equilibrium solution. This issue is being explored in preparation for the next version of this document.

28. *Chapter 4, Page 6, Line 14 (Rind): Cumulative 'net' annual emissions.*

That works too. However, the authors wanted to emphasize that it was cumulative, NOT annual emissions that were being stabilized. We inserted a comma to make the contrast more apparent.

29. *p.6, line 36: Fig. 1 and Fig. 2*

The change has been made.

30. *Chapter 4, Page 9, Figure 4.10 (Rind): perhaps this would work better by showing the changes for the different stabilization levels as % reductions for each individual energy source; the changes don't stand out very clearly this way, although with effort one can calculate their importance. This is true for some of the other figures in this section as well.*

This issue will be revisited in the next version of this report.