U.S. Climate Change Science Program
Synthesis and Assessment Product 3.2

Climate Projections Based on Emissions Scenarios for Long-Lived Radiatively Active Trace Gases and Future Climate Impacts of Short-Lived Radiatively Active Gases and Aerosols

Response of the Authors

to the

National Research Council of the National Academies'

Review of the Draft Report

The recommendations, comments, and issues raised by the NRC Review Committee are in italics **and the responses from the authors of Synthesis and Assessment Product 3.2 are in bold.**

Acknowledgement

The authors wish to first gratefully thank the NRC reviewers for their comprehensive, coherent, and consistent review. It was extremely helpful and has lead to what we hope is a much improved 2nd Draft. We particularly appreciate their providing specific written examples, many of which were directly included in the 2nd Draft, along with their comments and recommendations.

Summary

However, the current draft needs revision to make the document easier to read even by subject experts.

Response: The document has been completely revised and substantially re-written.

Many of the figures and captions do not convey the information intended and comparison of figures is difficult because different scales are employed.

Response: The figures and their captions have been clarified and all data is on the same scale in a given figure.

In addition, the document needs to distinguish between the types of models, especially for the benefit of non-specialists.

Response: Chapters 2 and 3 now begin with a non-specialist description of the models used in that particular chapter. A non-specialist description of the models encountered in this report is also provided in Chapter 1, Box 1.1.

Also, in the technical sections of the report, more details about the models used and statistical methods employed need to be included (see specific chapter reviews).

Response: Detailed discussions of the composition and climate models, uncertainty, and statistical methods are provided in two new appendices, Appendix 3.1 and 3.2, and in two new boxes, Box 2.2 and Box 3.3.

The draft does not address all of the specified audiences, particularly "policymakers, decision-makers, and members of the media and general public with an interest in developing a fundamental understanding of the issue."

Response: The Preface, Executive Summary, Chapters 1 and much of 4 and the Key Findings and Introduction of Chapters 2 and 3 are now written in a non-technical manner that should be accessible to "policymakers, decision-makers, and members of the media and general public with an interest in developing a fundamental understanding of the issue."

Chapter 3 does not describe the state-of-the-science, the problems in methodology adopted in the current models, and the most uncertain factors in the current research regarding the effect of short-lived species on climate.

Response: The state-of-the-science addressed in Chapter 3 is now summarized in 1. Introduction and in 3.1 Introduction in Chapter 3, while problems in methodology and the most uncertain factors in the current research are all addressed in Chapter 4. There is also considerable discussion in section 3.3.4.3

Key Issues

1. *The document is not accessible to all intended audiences. The committee finds that the draft is written largely for a technical audience. The intended audiences as outlined in the prospectus also include those people engaged in scientific research, the media, policymakers, and members of the public. Policy and decision-makers in the public sector (e.g., congressional staff) need to understand the implications of these scenarios, in contrast to the research science community, who may be more interested in the actual outcomes. The draft provides relatively little information for an audience of non-technical readers, particularly information that could be used as guidelines for effective communication techniques. In general, the draft would greatly benefit from revisions to make it easier to read.*

Response: The document has been completely revised and substantially re-written. The Preface, Executive Summary, Chapters 1 and most of 4, and the Key Findings and Introduction of Chapters 2 and 3 are now written in non-technical language.

Some specific suggestions follow.

- *The document should include a short executive summary for a non-technical reader, such as congressional staff, local and regional governmental decision makers. The summary should not be merely descriptive, but informative on the main points of the document.*

  Response: The Executive Summary is now written in non-technical language and contains a 300 word Abstract, 3 pages of Key Findings and Recommendations for Future research and a 1 paragraph Reader's Guide for Chapters 1-4.

  The summary should use plain language to describe the goals of the report, the principal findings and why it is critical to understand the impact of short-lived species on future climate.

  Response: We believe it now does so.

  The summary should point out that these types of studies encompass a realistic time frame over which available technological solutions can be employed, and *that this study in particular, focuses on those gas and aerosol species whose future atmospheric levels are also subject to mitigation to control air pollution.*

Response: This has been added to the introduction of 1. Key Findings and Results.

The summary should define briefly but clearly the line between "long-lived" and "short-lived", not just described as "(carbon dioxide)" and "(soot)". An alternative approach could be to add a box consisting of a chart with temporal vs. spatial scales of various species, added by general model resolutions used in such a practice as a reference.

Response: These definitions and the line between long-lived and short-lived have been added as footnotes. They are also included in Box 1.1 in the Introductory Chapter, Chapter 1.

• A technical summary written for an informed general scientific audience could be included. This could be written using clearly defined technical language (without acronyms) so that the general scientific community, not just atmospheric scientists can understand the goals, findings and relevance of the study.

Response: We decided not to write a technical Summary. We believe that the non-technical Executive Summary is sufficient.

• If some chapters are to use technical language, the introduction chapter should contain a section with advice on "How to read this document" – a paragraph that describes the intent of each chapter and its target audience.

For instance, the paragraph may state: Chapter 1 provides an introduction to the study and relevant findings from previous studies and is intended to provide all audiences with a general overview. Chapters 2 and 3 provide detailed technical information about specific models, model runs and trends and are intended primarily for the scientific community. Chapter 4, which is intended for all audiences, provides a summary of the major findings and identifies new opportunities for future research.

Response: We agree. "How to read this document" Sections have been added to the Preface [the last section] and the Executive Summary [3.] and is the last paragraph in Chapter 1.

• A clear concise description of the models employed in the study needs to be included. This description should clearly outline the strengths, weaknesses, and critical assumptions for each model. The models should be referred to by what they do, not necessarily by the name of the team that developed it. For example, explain what the GFDL, GISS, and the two NCAR models do when they are

referred to in the document. This could either go in the introduction or in Chapter 3.

Response: We added a new section 1.4 Methodology and Box 1.1 in Chapter 1. along with more detailed information in Chapter 3; 3.2.2 Composition Models, 3.3.2 Climate models and Appendix 3.1 and 3.2.

2. Introductory material is lacking. The draft would be improved if the introduction section provided a clear framework and context for the rest of the document. At present the scope of and motivation for the study are not well explained. The authors could clearly state what this study does and does not address in terms of responses, relative feedbacks and species. In its current form, the transition to technical material is far too abrupt. Specific ways to improve the introduction follow.

Response: We essentially completely rewrote the 1st Draft following the guidance of the NRC review.

• *The authors could define what a scenario is, describe the models used in the study and differentiate between the different types of models.*

Response: We have added two boxes, Box 1.2 and Box 1.2 that do so.

• *The introduction section could outline the charge to the authors as they perceived it, and clearly define the goals and objectives of the document.*

Response: We now outline the charge to authors and the goals and objectives in both the Preface and Chapter1, section 1.2.

• *As an alternative, this material could be included in a foreword. The foreword or introduction could also state explicitly what the document does not address.*

Response: We have section 1.3 limitations that now explicitly addresses this issue.

• *There could also be a description of how adequate the adopted methods in the 3 models are in comparison to the current findings regarding the related processes.*

Response: We briefly touch on this in 1.4 Methodology, if we understand the reviewers' comment.

• *A discussion explaining the coupling between climate effects of long- and short-lived species is needed in the introduction. A reader with less technical background might wonder specifically about the relationship between the predictions for the well-mixed greenhouse gas scenarios and the predictions where these are combined with the short-lived species. Are effects adding? What* *changes in climate response, and on what time and space scales, when they are considered together?*

Response: We discuss this briefly in 1.4 Methodology and in 3.3.1 Experimental Design. However, one of our Key Findings is that the climate response is similar for radiative forcing by long-lived and short-lived radiatively active species.

3. *Details about the models used are lacking. In addition to a general description of model functions, many details about the models used in this assessment are not stated. Model resolution, inputs, reactive chemical mechanisms, emissions assumptions, and removal mechanisms, and residence times should be more clearly presented. In addition, there is insufficient detail about how the experiments were run. It is not possible to decipher what radiatively active species are predicted (emissions) vs. those prescribed (concentrations) and how they vary temporally and spatially. The technical detail could either be included in a table in Chapter 3 or described in the text of Chapter 3. The more general information about the models used could be included in the introduction (see reviews of specific chapters for suggestions).*

Response: We now provide the general information in Box 1.1 in Chapter 1 and much more technical detail in Appendix 3.1 and 3.2.

4. *Details about statistical methods employed are lacking. At present there is no discussion about how statistical significance was determined. The statistical significance of certain trends is discussed and judgments are made about the relative significance, yet there is no description of how this was calculated. This information could be provided in an appendix and should clearly describe the statistical approaches used to determine the relative significance of trends and explain the rationale behind why judgments were made.*

Response: We now provide a relatively non-technical box on uncertainty in Chapter 2, Box 2.2 and a mixed technical and non-technical box on statistical methods in Chapter 3, Box 3.3.

5. *Many of the figures and captions presented could be improved for ease of interpretation. The figures presented in the report do not have similar scales or projections, which makes comparison of the data difficult. In addition, key points that are made in the discussion are not necessarily obvious from the present figures. For example, it is not entirely clear that the pattern of temperature response to short-lived species is of similar magnitude and distribution as the pattern of long-lived species. A graphic comparison of the temperature response to short-lived species vs the response to long-lived species should be presented.*

Response: Each Figure should now have all panels with the same scale and projection. It was not feasible to keep similar scales and the same projections for all latitude-longitude Figures, but we did try. We also added Figures to clearly

demonstrate the similarity between temperature responses for the short-lived and long-lived forcings.

Stylistic Issues

The committee notes several stylistic issues, which, if addressed, could significantly improve the overall accessibility of the document for a wider audience and improve the coherence of the document. Specific instances will be noted in the sections of this report that provide reviews of individual chapters of the draft. Broadly, these issues are:

> • *Jargon and definitions: The language suffers from excessive use of jargon and a lack of definitions of terms that may have multiple meanings to multiple readers. For example, certain terms such as, "very likely", and "likely" are used with the "specific IPCC connotations". These connotations should be defined, or the text translated so that they are accessible to the non-technical reader, particularly in the "key findings". In addition, the authors should expand the glossary for less commonly understood terms and phrases.*
>
> **Response: We have attempted to remove as much jargon as possible, particularly from the non-technical portions of Synthesis and Assessment product 3.2. Very likely and likely are now defined in the box that uses them, Box 2.1, and they are discussed in more detail in the relatively non-technical box on uncertainty, Box 2.2. Both boxes are in Chapter 2 where the terms are actually used to discuss the IPCC results. We have also added a box in Chapter 1, Box 1.2 that contains the most critical definitions in a relatively non-technical language. We are particularly fond of the definition for Radiative Forcing that was pirated from the IPCC's 4th Assessment and then edited further.**
>
> • *Acronyms: There are many instances in which undefined acronyms are used or defined at a later point. In general acronyms are over used to the point that they interfere with the flow of the document.*
>
> **Response: We agree. Many acronyms have been removed and those that remain are defined. We consider IPCC to now be a non-technical part of the English language and use it throughout, after first defining it. We have tried to remove all other acronyms from the non-technical portions of this report [Preface, Executive Summary, Chapter 1, beginnings of Chapter 2 and 3, and Chapter 4. We have kept GFDL, GISS and NCAR, after first defining them, to save a tree or two.**
>
> • *Content arrangement: The document would be improved by relegating supporting text that is not central to the study to boxes. For example, the authors could consider revising Chapter 2 to focus more on the climate implications of emissions scenarios developed in SAP 2.1a, and move the summary of results of the IPCC WGI AR4 to a box.*

Response: We agree and have done just that – Box 2.1 and Appendix 2.1

Chapter 1 Introduction

General Remarks

Chapter 1 needs similar revision to the document as a whole to make it easier to read. The committee is concerned that this chapter is not written so that it can easily be understood by the non-specialist. In particular, readability is impaired by frequent use of acronyms and abbreviations. These concerns are especially relevant to this chapter, as it sets the stage for (and provides a summary of) the other chapters. For example, there are many instances in which undefined acronyms are used or defined at a later point. In general, acronyms are over used and detract from the flow of the material. Certain terms (for example, "very likely", "likely" etc) are used with the "specific IPCC connotations". These connotations should be defined, or the text translated so that they are accessible to the non-technical reader, particularly in the "key findings".

Response: Chapter 1 is now written for the non-technical reader. There are no undefined acronyms and very few acronyms at all. The definitions of likely and very likely are now provided in non-technical language in Box 2.1 where the terms are now first used. They are no longer in either Chapter 1 or the Executive Summary.

The document should be revised by including explanations and use plain language that make the results more easily interpretable to a non-technical audience. Some specific examples are to explain the following:
- *What is an integrated assessment model?*
- *What is a scenario?*
- *Explain how IAMs differ from climate models*
- *Explain why the IAMs differ from each other, and why it is important to use more than one*
- *Explain why Radiative Forcing is an important concept.*

Response: The first time any of the words listed above are used in Chapters 1-4, they are defined in footnotes using the MS Word footnote option. We also provide all of the above in Box 2.1: Model Descriptions and Box 2.1: Useful Definition.

The "Historical Overview" section is useful and well-written. The overview of the IPCC reports (beginning at line 414) could benefit from a very brief statement of what the IPCC is as well as the scope of IPCC assessments (i.e., review of current literature; there is a common misconception that IPCC performs research). At lines 434-441 it could be noted that the models are moving toward finer resolution that can include some topographic features that are important to U.S. climate. Finer resolution in the ocean now allows some important atmosphere-ocean coupling processes such as ENSO to be represented in some AOGCMs (see e.g., van Oldenborgh et al. 2005; Wittenberg et al. 2006).

Response: We added the brief statement about the scope of the IPCC along with some editorial comments of our own. We did not discuss issues of increasing

resolution since it did not seem to be appropriate for a non-technical introductory chapter.

The three AOGCMs and modeling groups should be briefly introduced in this chapter. Care should be taken to distinguish the AOGCMs from the IAMs, especially for the benefit of non-specialists. Text (perhaps a box) describing these types of models and functions as an introduction to a non-technical reader should be included.

Response: We added Box 1.1 to do just that.

An indication should also be given as to whether the AOGCMs used in the study are appropriate to the task at hand. This need not be a detailed performance evaluation; it would be adequate simply to state that intercomparison studies have shown that the performance of these models is comparable to other state of the art climate models. Finally, the methodology and its limitations should be made clearer at the outset and should also explain why new emissions scenarios are needed. . .

Response: We added the performance statement and addressed the new emission scenarios from SAP 2.1 in section 1.5 of Chapter 1.

Minor Issues

L428-429: "model" should be plural, "models"

Response: That has been corrected.

L506: Methane is reactive not only in the troposphere but also in the stratosphere (being a main source of water vapor in the upper stratosphere).

Response: All of that discussion has been remove along with the whole Section 2. Chapter 1 was completely re-written.

Chapter 2 Climate Projections From Well-Mixed Greenhouse Gas Stabilization Scenarios

General Remarks

The committee feels that the chapter contains much useful material that serves to fulfill the mandates of the prospectus. It also feels that the chapter can be improved in several regards.

First, the chapter needs revisions to make it easier to read. It also assumes the reader to be a technical expert, and should either have a summary for non-technical reader, or be identified as such at the beginning.

Response: Chapter 2 has been re-written and a non-technical summary of key findings and a non-technical description of the Chapter are now provided in 2.1 Introduction.

The chapter notes that all of the scenarios are contained within the range of the scenarios examined by IPCC Working Group I (WGI) in the Fourth Assessment Report (AR4). (Though, one of the attributes of the SAP 2.1a is that it contains stabilization scenarios that fall outside of the range of the SRES scenarios on the low side.)

Response: The fact that the Level 4 scenarios with stabilization levels of 450ppm carbon dioxide and their resulting global average radiative forcings and surface temperatures all fall below the three standard IPCC scenarios is now noted and discussed.

The committee recommends that the authors consider revising the chapter to focus more on the first material, moving the summary of results of the IPCC WGI AR4 to a box, and adding a section that identifies the role of the short-lived species that could serve as motivation for and transition to Chapter 3.

Response: This has been done along with the addition of Appendix 2.1.

The motivation for and important conclusions arising from the section on regional climate models needs to be clarified. The committee speculated that the intent was to show the similarities in surface temperature change and ozone change between the global and regional models, but was left wondering if there was more to the section. The committee agrees that more research is clearly needed to assess if downscaled RCM simulations improve our ability to characterize climate change (lines 1132-1145), but this statement might be better suited for Chapter 4.

Response: The section on regional climate models has been moved to section 3.4, Regional emission sector perturbations and regional models, in Chapter 3 and to Chapter 4.

Minor Issues

Figures 2.1, 2.2, 2.3, 2.4: Show the three SAP 2.1a reference cases as well as the stabilization cases. Also note that in addition to the SRES cases that a commitment run is also shown. (The latter is what allows the assertion that the IPCC work contains the SAP 2.1a.)

Response: We now acknowledge that Level 1 is below the B1 scenario. We did not add the reference scenarios because the figures are already very crowded and we did not think that the reference scenarios were relevant to the points being made with the Figures. We did, however, add a discussion of the reference scenarios and their meaning in sections 2.1 and 2.2.

Figures 2.1, 2.2, 2.3, 2.4: In general it would be good to provide a table for year 2100 values. This would make it easier for the reader to get a sense of the absolute differences between cases.

Response: The Table has been provided.

L27642-643: How can the lower bound of the 5-95% range (i.e., 0.19 m) be less than the minimum of the entire range (0.28 m)? Please clarify.

Response: We changed the text to read "By 2100, global-mean sea level is projected across the 3 SRES scenarios to rise by 0.28m to 37m for the three multi-model averages with an overall 5-95% range of 0.19 to 0.50 m."

L28655-658: ENSO and the AMOC are two different phenomena. Lumping them together risks creating a misleading association in the minds of non-experts.

Response: They have been separated.

L29688-690: *This is right for one model, MERGE. It is a formal optimization model. The other two models are recursive. However, the two recursive models adopted two assumptions that resulted in results being similar to that of a formal optimization model. Both assumed that all regions of the world and all economic activities faced the same price of carbon, though each model adopted their own treatment of the non-CO2 greenhouse gases. Only MERGE was a true optimization frame. The MiniCAM adopted the assumption that the price of carbon rose at the rate of interest plus the rate of net loss of carbon from the atmosphere to the ocean-terrestrial system. This is consistent with intertemporal cost optimization for carbon. The IGSM used a similar assumption, namely that the price of carbon rises at the rate of interest. For the purposes of this report it should be adequate to simply state that, "All of the groups developed pathways to stabilization targets designed around economic principles. However, each group used somewhat different approaches to stabilization scenario construction."*

Response: We have added the suggested language in both 2.1 and 2.2.

L30694-695*: "...trajectories ... were produced...". Would a non-expert know what this phrase means?*

Response: No. We changed it to "Consistent time series for the emissions of short-lived…"

L30696: *Change "optimization process" to "scenario definitions".*

Response: The change has been made.

L31724: *What are "F-gases?"*

Response: F-gases have been changed to halocarbons.

L32739: *Note that the MiniCAM uses MAGICC as its representation of carbon cycle and the atmosphere.*

Response: This is now noted.

L34786: *Some discussion of the methodology employed to link MERGE output to MAGICC, particularly in the carbon cycle is needed. The MERGE model appears to have adjusted its ocean to reproduce essentially the same behavior as the other two models' combined ocean and terrestrial system models. This sparks the question of how was this case run so that it is true to the underlying MERGE approach?*

Response: The CO2 concentration results presented are not those given by the original models, but by the MAGICC carbon cycle model using the emissions given in CCSP2.1a. The reason for this is to provide a "level playing field" that facilitates comparisons between the different integrated assessment models. MERGE has no terrestrial biosphere and instead assumes a neutral biosphere where net terrestrial emissions are balanced by fertilization and other feedbacks. The MERGE ocean model has not been tuned to any other models. Instead, it employs convolution parameters given years ago by Maier-Reimer et al. It is largely a coincidence, therefore, that MERGE gives reasonable concentration projections for the cases considered. There is, of course, an inconsistency in using optimized emissions from any of the models, which are dependent on the model's own carbon cycle model, with the MAGICC model (except for MiniCAM which uses MAGICC). However, there is no single "right" way to do these analyses – either one uses each model's CO2 concentration directly, which would be inconsistent with the use of MAGICC elsewhere and would make interpretation more difficult; or one uses the method that we chose to employ with the same carbon cycle model applied in all cases. We have thought about this issue and chosen the latter strategy – although we should point out that the differences in results of the two approaches are very small. We did add "While the three integrated assessment models used in Synthesis and Assessment Product 2.1a each treated the cycling of carbon dioxide between the

land, ocean and atmosphere in their own way, in this study we use the carbon cycling treatment employed by MAGICC for all of the stabilization emission scenarios. This provides a level playing field for all of the scenarios (see Wigley et al., 2007b for a detailed discussion of this issue). We find that there is little difference between the two approaches."

L35805-806: *committee is skeptical that EPPA runs a 200-year trajectory to stabilization. This assertion should be checked.*

Response: EPPA did not run out to a specific stabilization point 200 years in the future. Our text says "a 200 year trajectory to stabilization", but this cannot be taken literally. The point is that in the optimization calculations it is not possible to stabilize by 2100. If the calculations were carried in detail beyond 2100, then stabilization would occur at some later date. The higher the stabilization level, the later the date – as in the standard WRE profiles (Wigley, T.M.L., Richels, R. and Edmonds, J.A., 1996: Economic and environmental choices in the stabilization of atmospheric CO_2 concentrations. *Nature* 379, 240–243). Clearly, in the Level 4 case, stabilization could not be achieved by 2100 because this would lead to CO2 forcings above the reference case. Although the CCSP2.1a scenarios were not determined beyond 2100, our understanding is that the optimization algorithm employed by EPPA requires a target stabilization date to be specified. Different dates may have been used for different stabilization levels. This would have to be checked with the MIT group. However, this is a point of no consequence for the calculations given here.

We changed the sentence to read "on a trajectory where radiative forcing stabilizes some time after 2100 (although emissions were calculated only to 2100) (SAP 2.1a, 2007). For the Level 2, 3 and 4 stabilization cases it is not possible to stabilize as early as 2100 (c.f. Wigley et al., 1996)."

We also removed the text "All three IAM … CH4 scenarios." since this is a complex issue of little significance that cannot be explained briefly.

L35800-816: *It is important to note that in general all of the models hit their targets with their own carbon cycle and atmosphere models. Thus, failure to hit targets in MAGICC is not the same thing as failing to meet the stabilization target for the model. The methodology employed to get the radiative forcing and transient temperature changes were to have each model use the MAGICC atmosphere. However, the models did hit the target using their own atmospheric representations. Thus, when results differed when all used the MAGICC atmosphere, this would seem to be a reflection of underlying uncertainty in the carbon cycle models, which, as we all know, is substantial.*

Response: This paragraph, with some editing, has been added to the end of 2.4.

L36823-825: *It seems odd to replace the BC and OC from the MiniCAM and MERGE models with arbitrary trajectories*.

Response: Actually, it is the MiniCAM and EPPA BC and OC results that we chose not to use. The text explains why this is so – simply to provide a more level playing field for comparison of the three IA models. To use BC and OC data would require additional choices to be made regarding their 1990 or 2000 forcing levels, so it is not a trivial matter to use these data. With reasonable choices here, the effects of their use versus the method currently employed are minor.

L36836: *What does "later for MiniCAM" mean? The comment is unclear.*

Response: It was not an important point and has been dropped.

L37842-843: *This statement should be checked. Is it really true that SO2 emissions are invariant across the scenarios as indicated? How could SO2 emissions not vary with dramatic reductions in fossil fuel use? EPPA assumes lots and lots of CO2 capture and storage, which means almost complete clean up of S, so SO2 emissions should decline as the stabilization level tightens.*

Response: In EPPA and MinCAM, SO2 emissions are not "invariant across the scenarios". They are in MERGE because MERGE does not specifically consider SO2 emissions and does not give any results for SO2 emissions. Future SO2 emissions cannot be ignored, of course. We therefore chose to use B2 emissions for all MERGE cases. While using a common scenario in all cases is clearly incorrect, there is no way that we could generate scenarios that differed. Our choice here was endorsed by the integrated assessment modeling teams. For EPPA and MiniCAM, future SO2 emissions are driven largely by the way responses to pollution are modeled (see, e.g., Smith, S. J., Pitcher, H. and Wigley, T.M.L., 2005: Future sulfur dioxide emissions. *Climatic Change* 73, 267–318), and partly by "knock on" effects from policy-derived CO2 emissions reductions. The latter is a secondary effect, and is significant only in the MiniCAM Level 1 case where the optimized emissions trajectory requires large and virtually immediate reductions in high-sulfur Chinese coal. There are considerable uncertainties in modeling future SO2 emissions, and these are manifest in the differences between the EPPA and MinCAM results.
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L 371132-1145: *Downscaled information is necessary for more than air quality; in particular, it is needed for hydrologic and agricultural uses (among many others).* **We agree and the**

Response: Statement has been expanded. It is now in Chapter 3, section 3.4

Chapter 3 Climate Change From Short-Lived Emissions Due to Human Activities

General Remarks

The committee thought that Chapter 3 was the most substantive. They believed that this chapter should more clearly identify what the major take-home messages are and should also consider including additional analysis of the mechanisms involved.

Response: Key findings from this chapter are stated more clearly in the chapter opening. Both the summary of the findings and the Introduction to the Chapter are now written in non-technical language.

The authors' main point, that the short-lived greenhouse gases are important factors in projections of future climate, is well supported. However, the climate models do not use consistent forcing scenarios for the short-lived species, nor do they use consistent natural emissions of primary aerosols and ozone and aerosol precursors or consistent removal mechanisms for the short-lived species. This makes comparison of the model results challenging.

Response: We concur. More consistency among the models would make comparison of the results more straightforward. In contrast, having the models use their own forcing, emissions and removal mechanisms provides a more realistic picture of the range of possible future behavior of the short-lived species and of the current level of confusion regarding projections of short-lived emissions.

Additional discussion of the difference between uncertainties in processes and uncertainties in future emissions is needed. Uncertainties in chemical and physical processes represent the state of our current knowledge. The fact that one modeling group chooses to include a process while another group chooses not to shows that our knowledge about short-lived species is still evolving. Eventually, with further research, uncertainties in chemical and physical processes can be ironed out. Uncertainties in future emissions, however, will never be completely erased. What modelers can do is choose consistent emission scenarios to bracket possible future outcomes.

Response: We agree and think it is an excellent point. We have added part of this discussion to the Executive Summary "However, uncertainties in future emissions will always be with us. What we can do is develop a set of internally consistent emission scenarios that include all of the important radiative species and bracket the full range of possible future outcomes."

We also use it to lead off section 4.3 in Chapter 4 "It is important to recognize the difference between uncertainties in processes and uncertainties in future emissions. Uncertainties in chemical and physical processes, which are discussed in 4.3.2-3, represent the state of our current knowledge. The fact that one modeling group chooses to include a process such as indirect forcing of climate by particles, while

another group chooses not to, shows that our knowledge about short-lived species and their interactions with climate is still evolving. Eventually, with further research, uncertainties in chemical and physical processes can be reduced if not completely removed. However, uncertainties in future emissions, which are discussed in 4.3.1, will always be with us. What we can do is develop a set of internally consistent emission scenarios that include all of the important radiative species and bracket the full range of possible future outcomes."

The authors need to emphasize that the magnitude and signs of effects of the short-lived species on climate may be totally different using different projected emissions in the same models.

Response: We agree with this point, and believe it is now clear from the discussion of the dominant impact of emissions uncertainties in the key findings and from an explicit discussion added to the section discussing the climate response to short-lived species (section 3.3.4.3).

Also, the committee thinks that following the A1B emission scenarios for the precursors of short-lived species all the way out to 2100 may result in unrealistically large surface concentrations of pollutants. The committee recommends inclusion of a figure showing the monthly mean surface ozone, BC, and OC at 2100. A caveat should then be added that such large abundances are not likely to be tolerated.

Response: We agree that emission projections out to 2100 are problematical and not yet even done in a logically consistent manner. In fact that is one the major findings and major research needs listed in both the executive Summary and in Chapter 4. However, we are not sure which is less likely for the year 2100, elemental carbon particle levels doubling relative to 2000 levels or sulfate particle levels halving. Admittedly, less sulfate reduction and less elemental carbon growth would both reduce the impact that we currently find for 2100. Those were the two major drivers of the 2100 impact of short-lived species on warming. We do add at the end of 3.3.5, "The integrated assessment model projections for A1B assume that SO_2 emissions will be reduced in the future in order to improve air quality, but did not explicitly project carbonaceous aerosol emissions. Scaling future carbonaceous emissions according to carbon monoxide emissions projections does not lead to similar reductions in emissions of these particulates, so that there is an issue of consistency in projecting the influence of future air quality decisions that deserves further study."

The committee recommends the addition of a table that includes descriptions of each of the models, including resolution, inputs, reactive chemical mechanisms, emissions assumptions, removal mechanisms, and residence times. In the accompanying discussion, sufficient detail should be provided for each experiment regarding what radiatively active species are predicted (emissions) vs. those prescribed (concentrations) and how they vary temporally and spatially so that the reader can understand exactly what was *done. Discussion of this table should include some analysis of the differences between model results produced by the different parameterizations.*

Response: We have provided most of this information in Appendix 3.1 and Appendix 3.2. However, as we state throughout Chapter 3, the major differences between the model responses result from the different emission scenarios chosen for the short-lived species. Next to those issues, model parameterizations are generally less important. We do address some of the model parameterization issues in the discussions of the radiative forcing and surface temperature differences among the models.

A graphic comparison of the temperature response to short-lived species vs the response to long-lived species should be presented. In this way, readers can appreciate 1) the contribution of the short-lived species to future climate change and 2) the similarities (or differences) of the responses to the short-lived vs long-lived species. Past work investigating the climate response to heterogeneous forcing should be discussed. Many of the plots showing future changes provide only the annual mean. Because the short-lived species have large seasonal trends, plots showing seasonal forcings and temperature responses are essential. Much information could be lost in the mean. This is true of course for the surface temperature response, but also especially important for the response of precipitation to changes in aerosol. It could be that the seasonal precipitation response would have much greater statistical significance than the annual mean precipitation response.

Response: Those have been added to the temperature and radiative forcing Figures, 3.7 and 3.8. We have examined seasonal precipitation, but still do not see statistical significance. The second paragraph of section 3.3.4.2 in fact is almost entirely a discussion of seasonal precipitation responses, and we have clarified that the seasonal changes, like the annual ones, have limited statistical significance (as well as diverging across the models).

Regional changes, particularly surface temperature, appear to be important, and the committee recommends that considerably more attention, discussion, and analysis be paid to this, including a comprehensive treatment of uncertainty. For example, a summertime 2oC increase over the central United States by 2100 would have large consequences for both human health and the economy.

Response: Statistical significance has been added to the maps of the spatial pattern of surface temperature change in response to short-lived species (Figure 3.7). Box 3.3 explicitly discusses the methods of calculating statistical significance for the 2100 results and the meaning of a 95% confidence level. The 2 degree C increase over the summer US is at the 99% confidence level for the version of the A1B scenario used. It is not the charge of SAP 3.2 to go any farther down the policy slope [we stop at presenting policy relevant results], though we agree that policy implications are pretty obvious. However, we also should and do note that "The integrated assessment model projections for A1B assume that SO_2 emissions will be reduced in

the future in order to improve air quality, but did not explicitly project carbonaceous aerosol emissions. Scaling future carbonaceous emissions according to carbon monoxide emissions projections does not lead to similar reductions in emissions of these particulates, so that there is an issue of consistency in projecting the influence of future air quality decisions that deserves further study."

Results on how temperature responded to changes in short-lived species would be greatly strengthened by additional sensitivity studies that could help to establish causes and mechanisms. For example, in the GISS model, how much warming did the declining trend in the indirect effect contribute to the climate response and where? How would the GISS results differ if dust had not been permitted to take up sulfur dioxide? Determining the relative importance of these and other processes to the climate response would help prioritize the gaps in our knowledge.

Response: While we do not have resources to undertake additional studies, we have added a discussion of the global mean forcing effect of these processes to section 3.3.4.3. We have not characterized the regional forcing due to the aerosol indirect effect in the GISS model, but given the conclusions of this chapter, believe that the global value is nevertheless a useful parameter. We now point out that the influence of these uncertain processes (aerosol indirect effects, aerosol internal mixing) have a comparable impact on temperature to the entire trend in the GISS model out to 2050.

In addition, a discussion of how the system might respond to controls on short-lived species and the possible feedbacks, and what the impact of climate changes might be on short-lived species would be helpful. At this point, there is sufficient information from present study and previous ones to get an approximate idea of what the feedbacks and control sensitivities are on the system to get a first order estimate of what controls on short-lived species and their precursors might do to climate.

Response: We are not sure we understand the comment. We do note in the report that the various A1B scenarios for short-lived species involve different visions of future controls. We also discuss, in the context of radiative forcing, the magnitude of some of the possible feedbacks, using previous results from studies by the GISS group, in section 3.3.3.4.

While the committee notes that the present document would benefit from these additional analyses [2 paragraphs above], it may not be feasible given time and potential monetary constraints; in such a case, a recommendation for future analyses should be included in Chapter 4.

Response: This was added to Chapter 4.

Discussion and citation of previous studies is insufficient. The authors need to show that their work builds on what has already been done. Also, citing previous work will enrich *the study by making clear where various model agreements and disagreements lie, and will help clarify how robust the current findings are.*

Response: We've added citation of several other studies examining the climate response to inhomogeneous forcings, including the work of Mitchell et al., Boer and Yu, Berntsen et al., and Hansen et al.,) as well as citations of earlier work on the climate impact of short-lived species (e.g. Hansen et al., 2000; Brasseur and Roekner, 2005; Delworth et al., 2005).

All of the methods used to calculate statistical significance should be described in detail either within this chapter or in an appendix.

Response: We have added Box 3.3 on statistical methods to Chapter 3.

The authors should emphasize that the impacts of climate change on the short-lived species were not included in this exercise, except for the methane/isoprene simulations. The authors should refer to other studies that show the relative importance of these climate impacts, and briefly describe how including such impacts might affect their results.

Response: Sections 3.3.3.3 and 3.3.3.4 discuss the potential importance of climate-related impacts on short-lived species. That these were not included is reiterated in the discussion section at the end of the climate response portion of the text (section 3.3.4.3). The text does include citations to prior work such as Stevenson et al and Gauss et al that looked at the response of ozone to climate change across a large number of models. It should be noted that, in the simulations proceeding out to 2100, ozone played a minor role.

It is not clear how the model simulations were set up, and why the authors made the choices they did. How were the time-slice monthly chemical fields of ozone and aerosol implemented in the transient climate simulations? Were the future composition simulations performed with present-day climate? How were the effects of long-lived species implemented in the models, as forcings or concentrations? The description of ensemble runs needs clarification for the lay audience.

Response: The second sentence of section 3.3.1 defines ensembles for the lay audience. This section and 3.2.2 have been revised to clarify the issues mentioned above. We have also added extensive technical discussions and detailed references in Appendix 3.1 and Appendix 3.2.

The methane text leads to many questions. Was three years a sufficient length of time to calculate the methane response to changing climate and chemistry? How much did OH concentrations further decline when the biogenic emissions of methane and isoprene were permitted to interact with the changing climate? What chemical mechanism was used for isoprene oxidation? (The choice of mechanism could make a difference in the outcome. Given that the fate of isoprene oxidation products is a major issue among air *quality modelers, this has importance. See Wu et al., 2007.) Was OH also allowed to respond to changing water vapor concentrations? How did changes in NOx emissions impact OH? This section also neglects much previous work looking at the effect of changing emissions and/or climate on methane abundances, e.g., Wild et al., 2001; Wigley et al., 2002; Stevenson et al., 2006.*

Response: The discussion of methane has been revised and made more comprehensive. A table showing the methane lifetime in the various emissions and climate change runs is now included (Table 3.9). This demonstrates how OH responded to biogenic emissions, climate, and to anthropogenic emissions (e.g. NOx). The latter two responses are now compared with those in Stevenson et al's multi-model assessment. We've added an explicit statement that in the climate runs, methane oxidation (OH) is affected by changing water vapor. We believe that discussion of the chemical mechanism used for isoprene is too much detail for the purposes of this CCSP report, but note that the reference given for the GISS model [Shindell et al., 2003] describes this fully.

The methane text should not be a box, but a separate section. The result here, that including biogenic chemistry-climate impacts increases methane concentrations and thus climate forcing, has importance and should be included in the chapter summary.

Response: This is now in its own section (3.3.3.3). Comment added to A1.

The methane section could end with a brief description of other chemistry-climate feedbacks that could play a major role in the future climate. Processes involved in the feedbacks include: lightning NOx emissions, land cover change, changes in convection and transport, and changes in absolute humidity.

Response: We've added a paragraph at the end of this section (3.3.3.3) discussing some of these potential additional feedbacks. Changes in transport and humidity are already included in standard climate change simulations such as those described in section 3.3.3.4, which addresses these and similar issues of internal 'atmospheric' responses.

The current practice to include tropospheric chemistry in global models has a common problem in methods, i.e., the simplified representation of subgrid-scale processes (e.g., fast chemistry affecting species from nitrogen oxides and isoprene to ozone, nucleation of aerosols). The authors are encouraged to make a comment on the consequences of using coarse-grid models to describe fine-scale chemistry.

Response: The question of the spatial scales for chemical reactions vs. the coarse spatial representations of any numerical model, global or regional, has been an issue for atmospheric chemistry and chemistry transport modeling for more than 30 years. We do not have any answers. We do discuss a range of chemical transport issues in Chapter 4, section 4.4.3.

Minor Issues

The bullets at the beginning of the chapter could be revised to ensure that key points are highlighted. The first bullet in the Introduction and Key Findings section, line 1328, is awkward. Bullet 2, line 1333, infers that short-lived species are emitted when actually some of the most important (ozone and sulfate) are formed in the atmosphere.

Response: We do not use bullets anymore. The uncertainties in precursor emissions play the largest role in the intermodel differences in aerosols. We do not feel that the current wording implies the short-lived species are necessarily directly emitted.

Line 1500. It's more appropriate to use "amount of sulfate and ammonia" instead of "amount of sulfate". Note that the added detail that a lognormal distribution is assumed for all aerosols is not needed for this audience.

Response: This section is now in 3.4.2. We've revised the phrase as suggested and removed the sentence about the lognormal distribution.

Line 1572: Why would the treatment of natural and biomass burning emissions affect sulfur dioxide emissions to such a large extent?

Response: This sentence in section 3.2.1.1 has been revised to indicate that the SO_2 emissions are affected by the overall present-day inventory. This depends in part on the natural and biomass burning emissions, but the implication was unclear before.

Lines 1606-1608. Nitrate can be a dominant component of aerosol during the winter, and may therefore play an important role in climate at that time of year. Therefore, the reviewers are not convinced that nitrate has a "minimal effect" and that it doesn't matter that only GISS includes nitrate aerosol. Further, as sulfate concentrations decline, and ammonia increases (as estimates suggest will be the case), nitrate may become an even bigger player.

Response: Nitrate may indeed be important seasonally and locally, and its importance may change with time. In the GISS model used here, nitrate's influence was quite small however, even at the regional scale. We've clarified this text.

Many of the Figure captions are not clear. E.g., in Figure 3.2, what is being shown here in what units, and for what time period? "Other" in the NCAR bar should be defined differently in the labels. Most of the captions are not "stand-alone." The reader needs to burrow through the text to know what is going on in this plot.

Response: Both the figure and its caption were revised as suggested, as were caption for other figures to make them 'stand-alone'.

Table 3.3. The term "model production efficiency" is confusing, since it resembles the well-known but differently defined term ozone production efficiency. The reviewers

suggest employing a different term, such as burden-emission ratio (BER). Alternatively, what would be lost if the authors instead just looked at species lifetimes?

Response: We've added a new table (3.3) which shows the residence times for the aerosols. This eliminates the need for 'production' information for BC and OC. For sulfate however, we feel that it's useful to include the sulfate burden change per SO_2 emission as this diagnoses the intermodel differences in conversion of SO_2 into sulfate.

Also in Table 3.3, the GFDL ozone production efficiency declines dramatically between 2000 and 2030 (7.19 to 2.24). Why is this? In the A1B future, volatile organic compounds go up and NOx goes down, which would typically lead to a higher OPE. The authors need to explain this large jump.

Response: The ratio of Ozone to NOx declines because ozone is being produce by NOx, but it is also being transported in from the stratosphere. Since the stratospheric level of ozone is held constant and tropospheric ozone is increasing, the transport of ozone from the stratosphere is decreasing while the production by NOx is increasing. Just looking at the net change and assigning it only to increasing NOx is misleading. We have partially corrected the language in the last paragraph of 3.2.3

Line 1650. This paragraph describes several trends in aerosol production efficiencies. The authors need to attempt to explain the reasons for these trends. Could trends in wet and dry deposition of sulfate, nitrate, BC, OC and other aerosols and precursor gases matter?

Response: We have replaced analysis of carbonaceous aerosols in terms of 'production efficiency' with their residence time, so this discussion is now for residence times. We've revised this to indicate that this is most likely attributable to the shift in emission source regions, which affects wet and dry removal. For sulfate, chemical conversion rates may also change with time, which is also noted.

Line 1651 What does "are more effective" mean? Please clarify.

Response: This has been removed in the revision of this paragraph.

Paragraph beginning with Line 1720. The reviewers are surprised that OC is not considered a more major player. Could the authors comment on this small contribution?

Response: The small contribution of OC is common to all 3 models, and to most models (as in the AeroCom project studies). We expect that OC is underestimated in the current generation of models, for reasons not fully understood.

A better figure and table of the radiative forcing of each of the different short-lived greenhouse gases for each model would be helpful (Figure 3.3)

Response: The revised Figure 3.3 and Table 3.6 give the forcings.

Color-coding the tables to emphasize the sign and magnitude of the differences in burden and emissions would be helpful.

Response: In many tables, all values are positive (e.g. Tables 3.1-3.5). In those showing radiative forcing, Table 3.6, 3.7, 3.10 and 3.11, we have color coded positive values red and negative blue.

Consistent scales and map projections would be helpful in Figures 3.4 and 3.7.

Response: These are now consistent.

Figure 3.4. The reviewers suggest including maps of radiative forcing from both long-lived and short-lived greenhouse gases for the same time periods.

Response: Radiative forcing from long-lived species is nearly uniform spatially. We now note this in the figure caption.

Lines 2176-2177. The authors state that uncertainties in socio-economics dominate "uncertainties in physical sciences." Here chemical mechanisms should also be mentioned because of important chemical reactions for sulfate and ozone formation.

Response: Chemistry added here.

Line 2248-2250. This sentence concerning the possible impact of future air-policy decisions in Asia on U.S. climate change is loaded with importance and needs more discussion.

Response: We have moderated the last paragraph and noted the inconsistency between sulfur dioxide emissions going way down while elemental carbon particle emissions continue to increase. In the 3rd draft we will revise further and note that the main issue for the US is the predicted strong climate response of the summertime central US to any increased radiative forcing in the Northern Hemisphere, be it reduced sulfur dioxide emissions in Asia or the lack of reduced carbon dioxide emissions anywhere in the Northern Hemisphere including the US.

Table 3.8 The table of radiative forcing impacts from regional sector perturbations is interesting, but needs more explanation in the caption. What are the perturbations? Give them in the table caption or footnote.

Response: Added to Table (now 3.10) caption. We've added further analysis to this section of the paper as well to better compare the NCAR and GISS models.

Lines 2276-80: use 0.01 W rather than 10 mW (and not 10 MW!)

Response: We prefer to use the milliwatt notation, as this is common in the literature on the response to sector and/or regional emissions. We have corrected the MW values.

Paragraph beginning with line 2293. This paragraph assumes that reducing surface transportation emissions of short-lived species and their precursors is done by reduced fuel consumption. This is not necessarily the case. Indeed, some controls might increase fuel consumption.

Response: We've clarified what was studied in these simulations, and the implications. The new section 3.4.1 introduces these experiments and points out that they assess the potential impacts of reduced usage rather than the mix of sources or emissions control technologies. This is discussed in more detail, including the specific point raised above, in the second to last paragraph of section 3.4.3.

Chapter 4 Issues, Opportunities, and Recommendations

General Remarks

The title provides a nice paradigm for the chapter as it suggests that issues, opportunities and recommendations will be discussed. However, few issues, opportunities or recommendations are apparent in the organization or presentation of the chapter material. The committee offers the following suggestions for reorganization within the chapter:

> 1. *Introduce this chapter with a restatement of what the scope of this SAP is, why the scope has been so defined (what was seen to have highest priority and why; what it was possible to do at the time), and what is not being addressed in this SAP;*
>
> **Response: We thought that stating this in the Preface and in Chapter 1 was enough. We preferred to start the last chapter with Findings. For those who read all the way to Chapter 4, we thought it was better to reward them with our punchlines.**
>
> 2. *Avoid jargon and acronyms; use more functional descriptions of models (with model names in parentheses). Otherwise, the text is seen as inaccessible and thus detail-dense information is largely lost;*
>
> **Response: We agree and have done our best to eliminate acronyms. Jargon is harder since there is significant science content, some of it technical in nature, in 4.3 and 4.4, but we have tried our best.**
>
> 3. *Refer back to new table(s) added to chapter 1 (Introduction) in which the different model configurations are described; possibly add figures to clarify steps taken/model process and use examples from chapter 3 (perhaps even show one of the figures (e.g. 3.1 bottom) again in this chapter) to highlight findings/conclusions drawn in this SAP;*
>
> **Response: We used extensive footnotes that are relatively non-technical, but we forgot about Box 1.1. We will add it in the 3rd Draft.**

In addition the chapter would be improved by the addition of a section recapitulating the highpoints of the study. Some suggestions are.

Response: We agree and chose to start with them. With some editing and rearranging, we have adopted much of 1.-7.

> 1. The SAP model scenarios for long-lived species produce projections that are within the IPCC range, although it should be noted that the SAP response range tends to be lower than all but the IPCC "commitment scenario."

2. The most important uncertainties in characterization in short lived species were found to be emissions and the indirect effect.

3. Part of the reason for the different emission inventories used here and in the IPCC studies was that the Integrated Assessment Models did not recognize that these species were necessarily important when the scenarios were first constructed. Clarification of the challenges associated with emissions projections (not a simple matter of improving quantitative skill, as these are a function of difficult-to-anticipate socioeconomic choices) should also be included;

4. Natural aerosols are also important and their emissions differed greatly between the models, with consequences to the role of anthropogenic emissions. The inconsistencies between models should be reconciled in future studies.

5. Calculation of the indirect effect is potentially the single most important deficiency in the study. The modeling community as a whole cannot as yet produce a credible characterization of the climate response to aerosol/cloud interactions. All models (including those participating in this study) are currently either ignoring it, or strongly constraining the model response. Using a box to highlight this issue is recommended. Additional referencing of work on aerosol indirect effects is also recommended.

6. The results suggest that the short-lived species do matter to the climate in the long term (e.g. out to 2100). The presence of radiatively active short-lived species can significantly change the regional surface temperature response (for example over the continental US). It is noteworthy and surprising however that the response location is not local to the forcing.

7. The 3 model frameworks participating in the study produced different outcomes. Each model represents a thoughtful, but incomplete characterization of the driving forces and processes that are believed to be important to the climate. Much work remains to be done before there can be confidence that the climate response to short-lived species is well understood.

At present, a list of the priorities and opportunities for future work is not presented in the chapter. This is unfortunate; because the result of this report clearly illustrated that there are many needs for future research.

Response: We agree and have added a complete section 4.4 Research Opportunities and Recommendations.

Additional regional modeling studies could provide information on local effects of short-lived species on regional climate. For example, the local impacts of aerosol forcing on surface temperature and precipitation could be significant. In addition, there is a need for modeling studies with finer resolution models, both at regional and global scales, to determine the resulting impacts on derived effects from short-lived emissions

Response: We agree and have included a revised 4.4.4 Recommendations for Regional Downscaling.

This SAP examines only a subset of processes controlling short-lived species and their interactions with clouds. Other processes might be important but have not been addressed such as ice clouds and their interactions with short lived species and the climate system. There is evidence that future biomass burning and land cover change could have a large effect on the climate response.

Response: We added a partial list of what is not addressed in this report in 4.4.5 Expanded Analysis and Sensitivity Studies. We also addressed much of this earlier in 1.3. Limitations

To conclude the document, a reflective assessment of the product would be useful. For example, – what lessons were learned during these experiments, what would be done differently if the experiments were to be repeated? How should other experiments be set up to answer the key questions generated by this study?

Response: We now conclude with "With all the issues discussed in Chapter 4, the net result is that at this time we can not find a consensus on the duration, magnitude or even sign (warming or cooling) of the climate change due to future levels of the short-lived species. However, we have presented a plausible case for enhanced climate warming due to air quality policies that focus primarily on sulfate aerosol reduction and permit the emission of soot to continue to increase as realized in a version of the IPCC's A1B scenario. Alternative versions of this scenario for short-lived species that follow different pollution control storylines could have less impact. While we do not have definitive answers to the second goal of this report "to assess the sign, magnitude and duration of future climate impacts due to changing levels of short-lived radiative species of anthropogenic origin", we do provide plausible estimates that begin to characterize the range of possibilities and we identify key areas of uncertainty and provide motivation for addressing them."
This along with section 4.4 provides a pretty extensive laundry list of future research. We do identify our key topics at the beginning of 4.4: the future emission scenarios for sulfur dioxide, elemental carbon particles and nitrogen oxides; the indirect radiative forcing by aerosols; and a number of ambiguities in current treatments of transport, deposition, and chemistry.

Minor Issues

Page 15 of the executive summary introduces a different chapter 4 than exists and raises expectations regarding a long list of other potentially important short-lived species and anthropogenic impacts (land use change, reactive nitrogen deposition and ecosystem responses, changing VOC emissions, changing oxidant and SOA formation).

Response: Chapter 4 has been completely re-written, and it now agrees with the Executive Summary.

A more-detailed discussion of the impact of fire (biomass burning) on aerosols and hydroxyl is needed. A concern was raised that 2-3 year runs are not long enough to capture the impact of ENSO-related fires in the tropics (like the 1997-1998 Indonesian fires). This may be an issue where current capabilities restrict a thorough treatment as part of this effort. If so, this should be stated in Chapter 3, and this issue should be raised as a future need in Chapter 4;

Response: We have a general discussion of what is not covered in 1.3. Limitations and a more focused one in 4.4.3 Improvements in Transport, Deposition, and Chemistry.

L2642: *This line seems to all of a sudden pop in here, yet is potentially rather important in regards to results in Chapter 3. Given that NH3 comes from a rather large number of processes, some rather disconnected to N2O production, this seems a stretch.*

Response: This qualification has been added and the discussion expanded: "Ammonia emissions present a similar problem. They are sometimes scaled by default to follow nitrous oxide, which is projected to increase significantly. Given the number of ammonia sources that are disconnected from nitrous oxide production, this may be questionable. Moreover newer projections for ammonia emission have a much slower rate of increase."

L2750 *"five different RCMs" should be "six different RCMs…"*

Response: This was corrected.

L 2755-2756 *The sentence beginning "Future IPCC…" should be corrected to "The IPCC A1B scenario is used in this intercomparison study." (i.e., only A1B will be used and not A2)*

Response: This has been corrected.

L 2790: *"The future sources of most of*

Response: Whoops, missed that one. We will correct it in the 3rd Draft.

L 2800: *They need to be more specific as to what is meant by biofuel, and also need to show it is CO2 neutral. In many cases, it is not.*

Response: This discussion has been clarified: "A major source of soot is the burning of biofuel, the sources of which are primarily animal and human waste as well as crop residue, all of which are considered carbon dioxide neutral. Current suggested replacements result in the release of fossil carbon dioxide. Therefore this reduction

in biofuel burning, while reducing the emission of soot, will increase the emission of carbon dioxide. The actual net climate response from reduced use of biofuel is not clear."

L 2841: *This last sentence is rather weak and equivocating. From what is presented, reducing NOx will reduce tropospheric ozone. Reducing tropospheric ozone should reduce radiative forcing. The report should have a strong ending.*

Response: I wish to thank the reviewers for reading to the end of Chapter 4. We certainly hope that someone else besides the authors does too.

Is this any stronger?
"While we do not have definitive answers to the second goal of this report "to assess the sign, magnitude and duration of future climate impacts due to changing levels of short-lived radiative species of anthropogenic origin", we do provide plausible estimates that begin to characterize the range of possibilities and we identify key areas of uncertainty and provide motivation for addressing them."