August 2018

# Authors' responses to U.S. National Academy of Sciences, Engineering and Medicine (NASEM) Committee Review (April 2018)[1] of the Fourth Order Draft[2] of the 2nd State of the Carbon Cycle Report (SOCCR2)[3] (line-by-line and narrative)

**TABLE of CONTENTS (hyperlinked to specific chapter responses)**



[1] National Academies of Sciences, Engineering, and Medicine. 2018. Review of the Draft Second State of the Carbon Cycle Report (SOCCR2). Washington, DC: The National Academies Press. doi: https://org/10.17226/25045
[2] The NASEM review was conducted on the Fourth Order Draft of SOCCR2. The authors' responses and revisions on the subsequent Fifth Order Draft were based on those. Multiple other reviews followed, including final interagency review and clearance on that draft. Additional revisions led to the Sixth Order Draft and then the final SOCCR2 report months later.
[3] The final SOCCR2 report released in November 2018 is available via https://carbon2018.globalchange.gov.

RESPONSE to NAS comments on the PREFACE

***NAS Review Comment***

*The draft SOCCR2 Preface states that the report Summary is "designed for a broader, more general audience". The Committee appreciates the challenges involved in distilling such a long, technically-oriented assessment down to a short, readable overview. Nonetheless the Summary is quite lengthy (27 pages), and in many places contains language and statistics that will be difficult for a lay reader to grasp. The text should be carefully edited by someone with a "popular science" writing background, to help make it more concise and more readable for the intended audience.*

**Response**

Agreed. We have added a 2-page plain language overview. However, SOCCR2's intended audience is different from NCA4's. SOCCR2 focuses on the science and included technical language. NCA4 is written in a less technical language because of its general public audience focused scope.

***NAS Review Comment***

***BOX 1 RELATIONSHIP BETWEEN SOCCR2 AND THE NCA REPORTS.***

*In reviewing the draft SOCCR2, The Committee struggled with understanding the intended and the actual relationship between this report relative to that of the fourth National Climate Assessment, Volume 1 (Climate Science Special Report, CSSR) and Volume 2 (Climate Change Risks, Impacts, and Adaptation, NCA4). The draft SOCCR2 report Preface contains some discussion about the relationship among SOCCR2, CSSR, and NCA4 (see p.6) – including Figure P1 which illustrates the areas of overlap/separation among the reports, and Table P1 which illustrates the intended "crosswalk" between specific chapters of SOCCR2 and NCA4. This explanatory material is helpful, and it indicates that there were efforts at the outset to define clear relationships among these different reports. However, this appears to be based on the originally intended plans, wherein the SOCCR2 report is completed in time to serve as foundational technical input to NCA.* In reality, due to delays in the SOCCR2 production timeline, it was released at the same time that the CSSR was published in final form and the draft NCA4 was released for review. Thus, the value of SOCCR2 now rests more as a stand-alone document rather *than as a foundation piece for a broader assessment. The explanatory Preface material should be revisited and updated to better reflect the reality of how the different reports did or did not actually feed into one another. More generally, there is the need to consider how the SOCCR2 authors determined what topics to exclude from their scope in order to avoid overlap with the CSSR and NCA4 reports. The strategy appears to emphasize avoiding redundant efforts, which is reasonable, yet it also has the effect of constraining the discussion in ways that may be confusing to many readers. This is a reasonable strategy to avoid redundant efforts, yet it constrains the discussion in ways that may be confusing to many readers. This problem is reflected, for instance, in the very limited discussion in the draft SOCCR2 of "Consequences of Rising $CO_2$" (Chapter 17), which presumably resulted from trying to avoid overlap with NCA4 discussions about climate change impacts. This is also reflected in the narrow discussion in the draft SOCCR2 of strategies to better manage carbon sources and sinks, which presumably resulted from trying to avoid overlap with NCA4 discussions about mitigation strategies. It is worth careful consideration of how best to incorporate carbon cycle science into future USGCRP assessment efforts. A determination should be made about whether the carbon cycle should be more interwoven into other assessment products, or how to best structure future SOCCR reports to be more distinct from future NCA reports. The rationale for these scoping decisions could be more clearly articulated in both the SOCCR2 and NCA4 reports, so that the reader is not left to speculate about why certain topics are or are not covered in either report.*

*Further, because the National Academies reviews of SOCCR2 and of NCA4 were conducted by independent committees (who only focused on evaluating their respective report), the Committee recommends that leaders of the SOCCR2 and the NCA4 reports coordinate a careful look across both USGCRP reports as they work to finalize them, to assure consistency in the information, analyses, and messaging presented, and to look for important issues that may have slipped between the cracks of these two assessment efforts.*

**RESPONSE**

- We have included an expanded appendix in the final SOCCR2 report to describe the SOCCR2 process in detail, in response to your helpful comments. Thank you.
- SOCCR2 is indeed intended to inform NCA4. However, SOCCR2's timeline was not based on CSSR's and SOCCR2 was not intended to inform CSSR. SOCCR2 has always been intended as a stand-alone decadal assessment independent of the NCA, similar to SOCCR1, published in 2007. SOCCR2 is part of the Sustained Assessment Process, not the National Climate

Assessment. The Preface and pertinent sections of SOCCR2 makes this fact very clear. We have also further clarified this in response to the NAS review.

- CSSR, when it was started, was not named NCA4 Vol. 1. It was also developed as a self-standing document.
- CSSR had not be conceptualized in early 2015 when SOCRR2 was first initiated. CSSR was first planned as a scientific report to be incorporated and summarized as a foundational chapter for NCA4. This occurred months after SOCCR2 had already started its development, and as planning for starting the official development of the NCA4 process underwent delays on the federal side.
- It is incorrect to state that SOCCR2 was intended to inform CSSR. Both CSSR and SOCCR2 were/are intended to inform and contribute to the science of NCA4 Vol. II.
- On November 3, 2017, 3 documents were released to the public by the USGCRP: the final CSSR (which had already undergone public and expedited NAS reviews in December 2017- January 2018), and drafts of both SOCCR2 and NCA4 Vol. II were released for public comments simultaneously. It is incorrect to state that SOCCR2 was released at the same time as CSSR. This was the Fourth Order Draft of SOCCR2 which had already undergone multiple federal and author reviews and two prior SGCRP Principals Reviews. The NCA4 public draft was the Third Order Draft of NCA4 which had undergone one SGCRP Principals Review, among other prior federal reviews.
- Among current USGCRP sustained assessments scheduled for imminent release, SOCCR2 is a stand-alone, Highly Influential Scientific Assessment which informs NCA4 Vol. II.
- Yes, the NCA4 vol. I and vol. II should outline the process too.
- The figure P1 represent three concurrently developed USGCRP assessments (SOCCR2 started in 2015, CSSR started in 2016 and NCA4 started in 2017) which followed similar reviews and information quality guidelines, and which were planned to be released within a few months of each other and how their common elements overlap in terms of scientific findings. To not mislead the informed or uninformed reader, additional clarification has been added to the caption and the figure. The figure P1 has been updated with the requested. It is now entitled: 'Geographic scope and examples of overlapping topics and findings among 2017/2018 USGCRP interagency sustained assessment reports,' and contains the following description to better describe the development process for SOCCR2, CSSR and NCA4 vo. II: Expanded Caption: "**Geographic scope and examples of overlapping topics and findings among 2017/2018 USGCRP interagency sustained assessment reports.** Three concurrently developed USGCRP interagency assessments (SOCCR2 was initiated in 2015, CSSR or NCA4 Vol.I in 2016 and NCA4 Vol. II in 2017) followed common Information Quality Act guidelines, report development processes, review mechanisms (public review via Federal Register Notices, National Academy of Sciences, multiple sequentially coordinated federal reviews by the Interagency Sub-Committee on the Global

Change Research). While some topics are overlapping among the three assessments, the focii, scope and intended audience differ, resulting in deliberate complementary or supplementary content cross-referencing across related chapters of SOCCR2 and NCA4. The USGCRP published CSSR in Dec 2017, with planned release NCA4 vol. II in Dec 2018. Simultaneous Public and NAS reviews of Drafts of both SOCCR2 and NCA4 Vol. II occurred from November 2017 to March 2018. Both CSSR and SOCCR2 contribute to the robust scientific foundation of NCA4 Vol. II. NCA4 provides pertinent cross-references to both CSSR and SOCCR2, as well as earlier published USGCRP assessments (not shown in this figure) such as the U.S. Food System Report (Brown et al. 2015) and the Climate and Health Assessment (USGCRP 2016).'

- Since topics and content development NCA4 vol. I and II were both started much later than SOCCR2, the assumption of limited SOCCR2 discussions on certain chapters to avoid overlaps with related NCA4 chapters is incorrect. Both CSSR and NCA4 vol. II authors were granted access to early drafts for SOCCR2 in order to ensure consistency and appropriate cross-referencing early during the process of development of their chapters.
- Please see the detailed SOCCR2 timeline and compare it to both CSSR and NCA4 Vol. II for details.

----------------------

Line-by-line NAS comments

***Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?***
The audience for SOCCR2 is described in the Preface as *"a diverse audience that includes scientists, decision makers in the public and private sectors, and communities across the United States, North America, and the world".* This definition may be overly broad. That said, it is a standard audience definition for these sorts of assessment reports, thus refining further may not be a critical priority.

**Response:** We agree but have attempted to specify a bit more in the 2-page plain language overview which we developed in response to the NAS review.

P18, Line 16-17 [Preface]
$CO_2e$ is defined relative to a time horizon, typically 100 years or 25 years. Most usage in the chapters assumes 100 years, though some mentions a shorter time horizon. This has to be consistent throughout the report. Here, it would be useful to give actual numbers for methane and $N_2O$ for the two time horizons.
**Response:** We have added a section in the Preface to address this comment. Thank you.
- Added section: Global Warming Potential and Carbon Dioxide Equivalent

Natural and anthropogenically-mediated carbon cycling causes fluxes of the greenhouse gases (GHG) $CO_2$ and $CH_4$, and often of nitrous oxide ($N_2O$) as well due to the tight coupling of the carbon and nitrogen cycles in ecosystems. Comparing the climate impact of these gases of differing radiative efficiencies and atmospheric residence times requires a metric to inter-compare the relative climate effects. Radiative effects are compared using various techniques, including instantaneous impacts, such as with the Global Temperature Change Potential (GTP) metric, or integrated over time, such as with the Global Warming Potential (GWP) metric; the intricacies of the comparison techniques differ depending on metric. The GWP is the most widely used climate metric; it evaluates the cumulative forcing of a 1 kg pulse emission of a GHG over a specified analytical time horizon, and then normalizes against that of a 1 kg pulse emission of $CO_2$ evaluated over the same time horizon. Multiplying this value (the GWP) by the emissions of the GHG gas yields the $CO_2$-equivalent ($CO_2e$)—the amount of $CO_2$ that would have the same warming effect over that time period as the amount of the GHG emitted. The UN Intergovernmental Panel on Climate Change has evaluated GWP over 20- and/or 100-year analytical time horizons (denoted $GWP_{20}$, and $GWP_{100}$, respectively) (Myhre et al., 2013), which is a decent indicator of climate effects in the near- and long-term, respectively. It can be assumed that, wherever $CO_2e$ results are reported in Chapter 3 and elsewhere in the report, this refers to the IPCC $GWP_{100}$ values (without consideration of indirect effects and feedbacks) except where noted otherwise. This semi-arbitrary but common choice of the 100-year analytical time horizon tends to de-emphasize the near-term climate impacts of $CH_4$ and other short-lived climate forcers. While best practices call for reporting $GWP_{20}$ and $GWP_{100}$ values together as a pair (Ocko et al., 2017) or temporally-explicit climate impact accounting that avoids the issue of time horizon altogether (Alvarez et al., 2012), most of the previous studies available to inform this report evaluated climate impacts on a $GWP_{100}$ basis only. Also note that while these $CO_2e$ estimates reflect several of the most important GHGs related to global carbon cycling (see Executive Summary footnote 4), they stop short of a full climate impact accounting. Aerosols and black carbon emissions are significant climate forcers important in some natural processes and energy use pathways (e.g., traditional biomass combustion), though translating them to $CO_2e$ terms is very difficult due to short atmospheric residence times on the order of a week, and therefore high regional variability that is complicated by local interactions with clouds and surface snow and ice; this results in GWP values with high uncertainty ranges (Myhre et al., 2013) and makes a global value inappropriate. Similarly, albedo changes and other biophysical changes are significant in certain land management settings (Caiazzo et al., 2014), but are also challenging to express simply in GWP terms for similar reasons.

Executive Summary

Narrative Response to NAS Comments on the Executive Summary

The NAS reviewers provided many constructive comments about how to improve the ES. In response, we have thoroughly revised the ES to make it significantly shorter by about half, and to make it more readable by focusing on the main highlights revealed by the chapters, as well as attempting to synthesize results across chapters. We carefully reviewed all of the numerical estimates in the ES as the chapters submitted their final versions in order to ensure consistency and traceability across the whole document. We also tried to respond to all of the detailed line-by-line suggestions, though in the end, by removing half of the original material, many of the comments became less relevant.

Here are brief responses to the main overview and general NAS comments on the ES. We have also prepared more detailed explanations of our responses in a separate document.

- In the ES and throughout the report, we strengthened discussions about implications of the findings for management and policy decisions regarding all main approaches for reducing greenhouse gases.

- We have carefully edited the ES to make it shorter and more understandable to a lay reader, including cutting some of the more detailed and dense sections in the middle.

- We have much more carefully arranged the ES and distilled our key messages to offer more context and guidance on why it is important to know more about carbon cycle dynamics.

- We attempted to improve integration of the physical and biological aspects of carbon cycle science and to integrate the natural science with social science, decision making, and actionable items, particularly in the updated section of the ES entitled "Carbon Management: Challenges and Opportunities."

- All numbers in the ES have been carefully reviewed for consistency with estimates reported in the chapters, and main points checked to ensure that connection to the material in the chapters is clear.

- In revising the main findings, we started with a statement of finding and then, if appropriate, reported on what was learned since SOCCR-1 or highlighted research needs.

- We decided not to include confidence statement in the key findings though some remain in the body of the ES where they are directly relevant. We did include an indication of uncertainty associated with the estimates that appear in the summary key findings.

- We added an initial key finding about global trends in carbon gases from Chapter 1 and carefully followed other recommendations about content and structure of the key findings.

- We made significant revisions to several of the figures, replaced ES-5 with a new figure showing fluxes and lateral flows in inland waters, while moving information from ES-5 to an enhanced ES-2 showing the main fluxes and flows. Also, table ES1 was simplified and made more comprehensive of all carbon management opportunities.

Detailed Responses to NAS Review Comments on the Executive Summary

ES-related comments from the overview comments

The draft report discussion of management decisions that affect carbon dynamics is uneven. For instance, there is discussion of how urban-scale actions can affect carbon emissions but little comparable discussion of actions at state, federal, and international levels. There is extensive coverage of decision-making regarding Agriculture, Forestry, and other Land Uses (AFOLU), but little discussion of how this integrates with other components of the carbon cycle to support decisions about $CO_2$ mitigation. There is very limited explanation of the opportunities that exist for more effective management of carbon sources and sinks. The Committee strongly encourages the authors address such gaps, as this would greatly enhance the usefulness of the assessment for informing governance and management decisions that affect carbon sources and sinks.

RESPONSE: Thank you for your comments. Box ES-3 (formerly ES-5) now better addresses state, federal, and international actions. We did not feel that AFOLU was not overly emphasized, since there is extensive discussion around Table ES.1 dealing with fossil fuel emissions reductions. We simplified and expanded Table ES.1 that integrates all of the options from the chapters compared with impacts on the carbon cycle. Please refer to figure ES-6 which presents an integrated view of all drivers (including management) and their interactions. We also strengthened details about management decisions in the chapters which are reflected (summarized) in the ES.

A major concern of the Committee is that the report does not also provide adequate explicit discussion of carbon management issues— that is, the actions that can be taken to reduce carbon emissions (e.g., through new energy policies and technologies) and to enhance carbon sinks (e.g., through effectively managing certain terrestrial or aquatic coastal habitats). The draft report's Executive Summary has some discussion about carbon management needs and challenges, but this does not seem to be based on material found in the body of the report. These shortcomings are important because a central reason why scientists study the carbon cycle is to help inform efforts to manage carbon source and sinks, and to identify the "levers" where effective changes can be made. The lack of coherent discussion of such issues limits the usefulness of the report for people who are making governance and management decisions that can affect carbon sources and sinks.

RESPONSE: Individual chapters enhanced their content about carbon management, and in the shortened ES (based on other review comments) we improved on the integration and consistency of the carbon management discussion. Now the ES has about 3.5 (formerly 6) pages dedicated to carbon management, all carefully linked to the content from different chapters and edited to ensure a more coherent presentation of this issue.

Comments from NAS specific to the ES: The draft SOCCR2 Preface states that the report Summary is "designed for a broader, more general audience."The Committee appreciates the challenges involved in distilling such a long, technically-oriented assessment down to a short, readable overview. Nonetheless the Summary is quite lengthy (27 pages), and in many places contains language and statistics that will be difficult for a lay reader to grasp. The text should be carefully edited by someone with a "popular science" writing background, to help make it more concise and more readable for the intended audience.

RESPONSE: We have carefully edited the ES to make it shorter and more understandable to a lay reader, including cutting some of the more detailed and dense sections in the middle.

In many places, the Summary reads like an accounting exercise, which may be reasonable given its focus on characterizing carbon stocks and flows. But it would be helpful to readers to offer more context and guidance on why it is important to know more about carbon cycle dynamics. There is some good text to this effect in report Chapter 1 that could be adapted for use in the Summary.

RESPONSE: We have much more carefully arranged the Summary and distilled our key messages. As mentioned above, some of the more dense and detailed sections in the middle were removed, which should enhance focus on higher-level issues, and decrease the appearance of too much "accounting."

Also, the Executive Summary misses some opportunities to integrate the physical and biological aspects of carbon cycle science and to integrate the natural science with social science, decision making, and actionable items. It would also be helpful to distinguish more clearly between carbon sources and sinks that one can control (e.g., urban emissions) and those that one cannot control directly (e.g., climate feedbacks).

RESPONSE: To some extent, this general comment about the whole report has been addressed in the updated section of the ES entitled "Carbon Management: Challenges and Opportunities." We have asked chapters to re-consider their carbon management sections to highlight those sources and sinks that can be managed. We tried to more closely link the ES section on "C management challenges and opportunities" with the section "a systems approach."

The Summary should also be carefully reviewed to ensure consistency within the body of the SOCCR2 report. There are a few places where numerical values presented in the Summary do not match the related numbers presented in later report chapters (examples are provided below). There are a number of points raised in the Summary that seem to bear no clear connection to material in the later report chapters. This should be carefully checked. Also, a 2017 carbon budget has been released by the Global Carbon Project, and a summary on U.S. greenhouse gases up to 2015 has been released by the US EPA (EPA, 2017), and so the SOCCR2 numbers should be consistent with these reports where appropriate.

RESPONSE: All numbers in the ES have been carefully reviewed for consistency with estimates reported in the chapters and main points checked to ensure that connection to the material in the chapters is clear. We also asked chapter authors to consult the latest releases from the GCP and EPA.

Add the following to the "main Findings:"

- a statement to convey some sense of what carbon sources/sinks we can and cannot control (i.e., what are the levers in the carbon cycle that we have opportunities to better manage?).

RESPONSE: We have modified main finding #6 (social systems) and #8 (attribution) to reflect this comment.

General Suggestions for the Summary Key Findings

Overall the Summary key findings come across as rather bland, in part because many of them focus primarily on the message that "we have learned a lot" about some aspect of carbon cycle science. It would be more informative to focus on articulating what has actually been learned (i.e., focus more on the actual outcomes of the research than on the research process itself).

RESPONSE: Good points, we have addressed these concerns in revising the main findings in the ES. In all cases, we started with a statement of finding and then, if appropriate, reported on what was learned since SOCCR-1 or highlighted research needs.

Further, statements such as "we have improved understanding of …." or "knowledge gaps remain" are vague. Does improved understanding mean improved accuracy, or new mechanisms, or something else? The Committee suggests providing quantitative information, including uncertainties, where possible.

RESPONSE: We have tried to be more specific when findings involve "improved understanding" or highlight "knowledge gaps." We have also tried to minimize the presentation of these kinds of findings, both in the Executive Summary and throughout the document.

Where possible, social science and decision-making concepts that are discussed in the body of the report could be woven into the key findings.

RESPONSE: Thanks a lot for this comment. Even though the chapters present this information largely independently, we tried to enhance integration throughout the ES and in the summary of key findings.

It is suggested that confidence levels should reflect the insights and not the methodology. For example, it is not helpful to show high confidence that data and models do not converge on some estimate.

RESPONSE: Thank you. We decided not to include confidence statement in the key findings though some remain in the body of the ES where they are directly relevant. We did include an indication of uncertainty associated with the estimates that appear in the summary key findings.

The time period covered by the assessment should be stated clearly at the outset of the Summary.

RESPONSE: We specified the time period as 2004-2013 early in the Executive Summary and recommended that authors specify the timeframe over which their assessment occurs if it differs from the recommendation.

It would be helpful to start with a finding that provides context about global $CO_2$ and $CH_4$ trends (i.e., $CO_2$ now exceeds 400ppm; atmospheric $CH_4$ has more than doubled), including some explanation of how we know that these trends are driven by anthropogenic emissions. These can link back to the National Climate Assessment report.

RESPONSE: Although the focus of this report is North America, we agree that an initial key finding about global trends in carbon gases is warranted, which we developed from chapter 1.

This finding can be followed by a clear statement about North American carbon budgets, that identifies the most important processes contributing to sources and sinks (e.g., for $CO_2$, biogenic versus energy-related sources; for $CH_4$, the importance of wetlands and estuaries), and that conveys the totality of changes in North American carbon flux (i.e., what is the net balance in terms of sources/sinks?). Figures ES1 and ES5 in principle provide this information, but they are not consistent and not described clearly in the text. There should be a separate key finding about methane.

RESPONSE: This seems to duplicate the material in the first few main findings. Rather than a separate finding, the first two were edited to accomplish this purpose. Note that figure ES-5 is deleted, and ES-2 (not ES-1 which is the map) is enhanced to show the totality and net balance. ES-2 is now referred to in the key findings. Also note that there is already a key finding about methane, which we updated to give it more visibility.

Many topics discussed in the body of the report are not reflected anywhere in these key findings. While of course hard choices must be made regarding what is and is not critical to include in key findings, the following topics are worth reconsidering to add somewhere in this list:

- the point that terrestrial land sinks play a critical role in helping to offset anthropogenic emissions;
- the issue of decreasing capacity for land and ocean carbon sinks (i.e., describe the current sink capacity, the possible degradation of these sinks by land use changes and disturbances, and the levers available to protect these sinks);
- a statement reflecting the report's discussion of tribal lands;
- a statement about the importance of greenhouse gas emissions from agriculture;
- a clear explanation of $CO_2$ and $CH_4$ contributions from the energy sector;

- a statement to convey some sense of what carbon sources/sinks we can and cannot control (i.e., what are the levers in the carbon cycle that we have opportunities to better manage?).

RESPONSE: These statements were integrated into existing or new main findings and in the main text of the Summary, as appropriate.

<u>Comments on Specific Summary Key Findings (Note that in revision the numbering of findings has changed because of adding a new #1)</u>

***FINDING 1.*** *Emissions from fossil fuels have declined slightly over the last decade, largely a result of decreasing reliance on coal, increasing reliance on natural gas, the global recession, and increased vehicle fuel efficiency standards. Economic productivity has continued to increase, demonstrating that $CO_2$ emissions can be decoupled at least partly from economic activity.* ● Insert "North America" to avoid confusion with the global emissions, which have grown.

- The reference to economic "productivity" would be better worded as "activity", assuming the authors are really talking about economic output measures such as GDP.
- Suggest dropping the reference to the recession here; as that was too short-term of a change to have a major effect on global atmospheric concentrations.
- Add to this finding or add a new finding: North American fraction of global fossil emissions, and the trend since SOCCR1. $CO_2$ and $CH_4$ contribution from the energy and agricultural sectors. RESPONSE: These edits are completed.

***FINDING 2.*** *The results from top down and bottom up approaches to estimating the magnitude of the land carbon sink are converging because of improvements in data and methodology, though significant uncertainties remain in both approaches. The land sink appears to be persistent, but future impacts from land use change and disturbances, both natural and human induced, may diminish this sink.*

- Starting with the "top down versus bottom up" message overemphasizes scientific process, as opposed to the actual information that people need to know. Suggest deleting this first sentence.
- This Finding should give quantitative estimates (including uncertainties) of the North American carbon budget and updates since SOCCR1.
- . Acknowledge here the fact that the estimates for Mexican forest fluxes have changed sign since SOCCR1, including discussion about the uncertainties in this finding.
- 'Future impacts' should include climate change.

RESPONSE: We revised so that second sentence is first but kept part of the first sentence because we also want to highlight advances based on research since SOCCR-2. Other edits accepted, except that the Mexican reference is rather specific and does not significantly affect the North America total sink. Will include this detail later in the ES under a forests section.

***FINDING 3***. *There have been marked improvements in the understanding of North America's carbon sources and sinks and the partitioning of carbon forms in water environments, as well as the importance of carbon transfers in inland water environments and across land water interfaces. Significant emissions from inland waters and a large carbon sink in the coastal ocean have been quantified.*

- This is another example of focusing on the process ("we've had improvements in understanding") rather than focusing on what has actually been learned. Give quantitative estimates of fluxes and inventories in water environments, including uncertainties.
- Need to mention that fluxes associated with inland waters and coastal ocean include pre-industrial or background fluxes, and that there is significant lateral transport.

RESPONSE: Similar to the previous finding, put the second sentence first and added quantitative estimates. Added additional detail as recommended.

***FINDING 4.*** *Understanding of the $CH_4$ budget has much improved, although there are important knowledge gaps. Overall, observations indicate that global atmospheric concentrations of $CH_4$ are increasing, while North American $CH_4$ emissions are relatively stable.* ● It is vague to say, "understanding of the budget has improved" and "there are knowledge gaps." Offer some concrete sense of what things we have learned, and what still needs to be learned. Be quantitative where possible.

- It is confusing to conflate the global and national emission trends. Better to focus on global emission numbers at the start of the key findings list, and then focus just on North American emission trends.
  It is not really clear what the point of this finding is. What do these stated trends mean? Can we link the North American $CH_4$ emission trend to mechanisms/drivers?
- Need to discuss the findings of significant methane emissions from oil/gas producing regions, and the existing discrepancies between emission observations with the apparently decreasing overall atmospheric methane trends.
- RESPONSE: In revision, we started this one with a quantitative statement of what is known, though we were unable to provide clear linkages to all different drivers because of uncertainties of understanding.

***FINDING 5.*** *Analyses of social systems and how carbon is embedded in them demonstrate the relevance of carbon cycle changes to people's everyday lives and reveal feasible pathways to reduce GHGs.*

- The Committee questions whether this sort of statement really needs to be highlighted as a "key finding", as it seems rather hollow.
- While explicit policy recommendations are not part of the mandate for the SOCCR2 assessment, it is reasonable to discuss the implications of policy decisions on the carbon cycle. Perhaps this is something that can be better addressed in the next SOCCR assessment.

RESPONSE: Thanks for the comment. We have significantly expanded this finding and added details about pathways. We think it is important to keep this finding even though the current state of knowledge about social systems is only beginning to be linked with the carbon cycle.

-

***FINDING 6.*** *Urban areas in North America represent the primary source of anthropogenic carbon emissions, as well as an indirect source of carbon from the emissions associated with goods and services produced outside city boundaries for consumption by urban dwellers. Therefore, carbon monitoring and budgeting in urban environments is increasingly important, including the avoidance of double counting with sectoral data on $CO_2$ fluxes.*

- This finding illustrates the problem of singling out urban regions as an emissions category. Given that the finding does not highlight other major sectors such as forests and agriculture, it is not obvious why “urban” is highlighted. The urban focus would make more sense if the finding identifies how particular components of carbon emissions are best controlled at the urban level (e.g., through steps such as restructuring urban development patterns to reduce driving), and if it is better integrated with the “decision making” discussion (Chapter 18) by identifying opportunities to effectively manage carbon emissions at the different governance levels (local/state/federal) or different system levels (forest, freshwater, etc).
- The finding could be improved by incorporating some of the information articulated in Box ES5—for instance, the point that monitoring could help inform the emission reduction pledges made by cities.

RESPONSE: We agree that this is a good place to highlight carbon management opportunities and processes. Comments accepted and key finding has been edited according to recommendations. We also need to work to clarify through which mechanisms urban areas contribute directly and indirectly to carbon emissions and are therefore key sources of mitigation actions.

***FINDING 7***. *Overall, research has led to an improved ability to attribute observed changes in the carbon budget to specific causes, including social and economic factors, technological change, climate variability, and management practices. Understanding these processes and their interactions aids in projecting future changes in the carbon cycle and developing adaptive capabilities. One projection is of significant concern the 5% to 15% of the carbon stored in soil pools in the circumpolar permafrost zone is considered to be vulnerable to release to the atmosphere by the year 2100, considering the current trajectory of global and Arctic warming.*

- The last sentence of this finding is the critical point to emphasize. The first two sentences are perhaps not even needed.
- It would help to delineate here the potential impacts of permafrost thaw on $CO_2$ and on $CH_4$ separately (rather than on carbon collectively), and to translate the percentages into absolute amounts (Pg, Tg of carbon) and/or into ppm in the atmosphere.
- Add a finding about projections – decreasing capacity of land and oceans to absorb $CO_2$.

RESPONSE: We made this into two findings, one on attribution and one on projections, each enhanced with additional content. The attribution (first part) is important in and of itself, and the second item about permafrost is also an important finding, and the last bullet was added so there is one finding dedicated to projections.

***FINDING 8.*** *There are still regions and ecosystems that are less well understood that would benefit from additional research and monitoring (e.g., the Arctic and boreal regions, grasslands, wetlands, inland waters, and tropical ecosystems among others described in SOCCR 2). Uncertainties for particular sources, sinks, and fluxes must be reduced to provide consistent and accurate inventory (bottom up) and verification (top down) estimates. Filling these gaps will be important milestones for the third SOCCR a decade from now.*

This finding offers little substance. A finding focused on research needs could be useful if it avoids being a vague list that could potentially encompass anything. Instead it could identify specific knowledge gaps that could feasibly be addressed with a focused research agenda.

- This Finding should include consideration of what research and support is needed to advance our understanding of carbon cycling and resource management on Tribal Lands, and to advance the integration of social science with the natural sciences of the carbon cycle.

RESPONSE: We think this finding is important to point to future research needed to fill some important knowledge gaps. Some specific knowledge gaps were already mentioned (first sentence, in parentheses). Replaced second sentence with specifics about tribal lands and social sciences.

## Comments on Summary Figures / Tables

Figure ES1: This figure shows that the domain of this report includes Puerto Rico, Hawaii, and U.S. Pacific islands, yet the report provides little information about these locations. The authors should re-draw ES1 to exclude these places, or mention in the text their carbon significance.

RESPONSE: We redrew Fig. ES-1 excluding specific mention of the US territories and Pacific Islands. We made Hawaii as an inset which allowed the map to stay focused on North America continent.

Figure ES2: This is potentially a helpful figure, but the following improvements are suggested:

- Augment to also show lateral fluxes of carbon – especially given how the report emphasizes these lateral fluxes as one of the important scientific advances of recent years.
- The box labeled "Atmosphere" should be "Global Atmosphere". As is, one could mis-interpret the +1032 to refer to atmosphere over N America. The authors may wish to consider putting parenthesis around (+1032) to indicate that it is an estimated quantity.
- The figures are reported with too many significant digits. The numbers should include uncertainties.
- The caption is unclear, and needs to distinguish between inventory and fluxes (e.g., arrows are fluxes).

RESPONSE: Thanks, this figure has been significantly edited. We added arrows to represent lateral flows. Numbers are consistent with Chapter 2 (integration) and contain the same number of digits as reported there. Note that the label “atmosphere” refers to the increase in CO2 concentration from emissions/sinks of North America, so labelling it “global atmosphere” could be misleading. Including uncertainties in the figure itself would make it more difficult to read, so we hope that the reader will refer to Chapter 2 for the uncertainties.

Figure ES3: The graphics should be improved in several respects:

- 
- The figure is missing information about the $CO_2$ source. It should show $CO_2$ and $CH_4$ separately. Most importantly, the $CO_2$ figure should show fossil fuel carbon and land use emissions over time.
- It shows emissions as “negative sinks,” which will be very confusing to most readers.
- Many readers may be unfamiliar with the “micromole per mole” units used. The report should include a note about units for reporting gases and consistently use one type of unit that is most familiar to lay readers (perhaps ppm and ppb).

RESPONSE: This figure was revised to include only the top half, which removes many of the expressed confusion about it. Units changed to ppm and ppb. Since the figure now represents only atmosphere concentrations, we don’t separate these by source. This information is contained elsewhere in the ES and chapters.

Figure ES4:

- Change left Y-axis label to “Annual emissions (PgC)”, as now it only indicates unit, but not parameter/variable.
- Figure legend: Change “North America” to “North America Total”, to make this more clear.
- The figure graphic quality can be improved, for example, by adding x-axis major tick marks for every 5 years, and leaving space between symbols and letters in the legend.

RESPONSE: These changes were made by chapter 2 and now also reflected in the ES.

Figure ES5 should be re-considered. This figure pertains to the total fluxes into or out of the atmosphere, but one could easily mis-interpret this figure to say that the North American net $CO_2$ flux is approximately equal to the emissions from fossil sources—as the forest sink and coastal ocean sink are countered by outgassing from inland waters. This figure would appear to contradict the terrestrial sink (land and water components). Other suggestions:

- y-axis label should be “Carbon dioxide fluxes (Tg C per year)”
- change x-axis category labels
- change from “Fossil fuels” to “Fossil fuel emissions”
- change from “Forest sector” to “Forests”
- change “Inland water outgas” to “Inland water”
- change “Arctic/boreal” to “Arctic/boreal permafrost”

RESPONSE: This figure has been deleted and essential information added to Figure ES-2.

In Figure ES2, ES4, and ES5, check the numbers/units for consistency. Even within the Executive Summary figures, there is inconsistency in the units used (a mix of Pg, Tg, other units).

RESPONSE: Numbers have all been checked for consistency.

The Committee recommends adding a figure showing the changes in the mix of energy sources and associated $CO_2$ emissions over time (at least in Chapter 3 [*Energy Systems*], if not in the Executive Summary).

RESPONSE: Forwarded to CH 3.

Table ES1 on trends, indicators, drivers from the energy system seems oddly placed in the Summary, given that there is nothing comparable presented for trends, indicators, drivers, impacts of other major components of the carbon cycle (e.g, forestry, agriculture, land use changes).

RESPONSE: Table ES-1 has been updated and simplified to represent all of the major components of the carbon cycle that can be managed.

<u>Line-Specific Comments – NOTE: because we significantly reduced the length, many of these specific line-by-line comments are no longer relevant. We responded to the ones that we could.</u>

P21, Line 12-13
Interestingly, this statement is an expression of world views characteristic of those held by indigenous communities— interconnectedness of humans and the environment. Additionally, it may be worth combining with text at p.25, l16.

RESPONSE: Both sections remain essentially unchanged, since they frame the report and provide context for connecting physical, biological, and social science aspects of the carbon cycle.

P21, Line 15
Replace "improved understanding" with "advances in our understanding".

RESPONSE: Done

P21, Line 28
It should be possible to quantify these. Can estimates be provided?

RESPONSE: this is the wording of the questions we were asked to address. Quantitative answers provided in later sections.

P21, Line 30
Discussion of ecosystem impacts is sparse. These impacts are complex and multi-faceted, involving spatial, temporal, and placeresource dependent considerations. Factors such as species displacement and migration, alteration of phenological behavior, impacts of water timing and availability, extreme events, and impacts such as introduction of genetic strains are not addressed.

RESPONSE: Here we are simply repeating the questions we were asked to address, so no editing required here.

P23, Line 20 – P25, Line 15
These main findings are disconnected from the final few pages of this chapter (beginning with p.38).

RESPONSE: Main findings have been extensively edited and now better represent the ending parts of the ES.

P27, Line 4
Forests typically are sinks, so reverse the analogy.

RESPONSE: Done.

Box ES2, paragraph 2.
Some of the chapters use different units [g/$m^2$ ]. More consistency is needed. Also methane units need to be included. Definition of $CO_2$e should include the time horizon (typically 100 years).

RESPONSE: Chapters are consistently using Tg and Pg for pools and fluxes. Rates (e.g. g/m2) are allowed to be represented by the units commonly used by that discipline. Methane units added and clarified definition of CO2e.

P28, Line 29-36
This is largely repetitive of concepts presented at p.23, lines 9-16.

RESPONSE: We recommend deleting text on p. 28 lines 29-36

P29, Line 8-18
While the numbers appear consistent with those in the literature, information on the significance of these increases would be valuable for a lay audience. For example, what is the importance of these increases for ecological processes, human health, food supplies, quality of life, and habitability? Might these statistics be more effectively presented at p.32?

RESPONSE: The section beginning on p. 32 addresses significance. Suggest leaving this comment for science editor to review when considering the overall organization of the ES.

P29, Line 5-28
These concepts are described in detail in other USGCRP documents, is it necessary to repeat in SOCCR2?

RESPONSE: It is true that these are described elsewhere, but we think this is important context in this report for the lay audience.

P29, Line 2 Suggest changing to "Evidence strongly suggests that changes…." (add "that").

RESPONSE: Done.

P29, Line 10
The unit for the atmospheric $CO_2$ concentration here (ppm) is different from the one in Figure ES3 (umol/mol).

RESPONSE: figure ES-3 has been converted to ppm and ppb for consistency

P31, Line 5-7
The confidence statement used here should clarify that the *magnitude* of sources/sinks contains uncertainty, but not the *process*.

RESPONSE: moved statement to end of paragraph.

P31, Line 12
Discuss methane after this line.

RESPONSE: OK

P32, Line 22-30
Need to mention that $CO_2$ fertilization is transient storage, as it is followed by greater litter inputs and enhanced decomposition and $CO_2$ efflux from soils.

RESPONSE: Done.

P33
This section should have a sub-section on $CO_2$ and one on methane. Here methane sources and sinks appear as a single bullet (p.35) interspersed between $CO_2$ fluxes and stocks. The methane subsection should include the recent studies of U.S. methane sources (e.g., Kort et al., 2014) and other references listed below for Chapter 2). Contrary to p.36, line 2, Turner et al. (2016) finds a trend in U.S. emissions.

RESPONSE: wWe moved the methane bullet to appear right after the CO2 bullet to give it more prominence, but did not create an expanded section for methane; rather, other aspects of the ES were shortened such that a better balance between methane and CO2 was achieved.

P33, Line 2

Shouldn't the focus be on carbon flux instead of atmospheric concentration when discussing sources and sinks?

RESPONSE: We are looking at both fluxes and how stocks (concentration) are affected. We think it is important to show the effect of the fluxes on the atmospheric concentrations.

P33, Line 22
The use of "now" refers to what period? 2004-2013?

RESPONSE: This has been clarified.

P33, Line 22
An important factor is the declining trend in North American contributions as a percent of global emissions; this deserves some elaboration.

RESPONSE:Done.

P35, Figure ES5
It is not clear how one gets 634 Tg from Figure ES2. Please check numbers and ensure their compatibility across the document.

RESPONSE: This figure has been eliminated.

P35, Line 11-14.
This contradicts Key Finding 3 of Chapter 2 (between ¼ and ½ of fossil fuel emissions were offset by natural sinks on North American land and adjacent coastal ocean. The authors should re-think how to present the information.

RESPONSE: The numbers will be checked.

P36, Line 13
As stated this sentence indicates that "land" sinks include inland waters and the coastal ocean. The word "land" here should be eliminated, or perhaps replaced with "continental". P36, Line 36-37
Why is the term "reservoirs" used? Is there an intended difference from sinks?

RESPONSE: We switched "land" to "continental." Reservoir is a synonym for a stock; sink is used to denote a flux from the atmosphere.

P37, Line 1-4
Carbon storage and risks of greenhouse gas and soot emissions from forests is heavily dependent on vegetative management practices, such as prescribed burning, mechanic removals, and species manipulation.

RESPONSE: We cannot tell which lines you are referring to.

P37, Line 13
Why aren't tillage practices mentioned?

RESPONSE: These are mentioned a few lines later.

P37, Line 35
Why isn't methane emission from reservoirs mentioned ?

RESPONSE: We do not have this number in chapters of the report

P37, Line 21-22
The results and findings presented here are inconsistent with ones in Chapter 13: the stated net carbon sink from terrestrial wetlands of 64 TgC/yr (36 TgC/yr by nonforested wetlands, plus 28 TgC/yr by forested wetlands) is different from the value of 53 Tg/yr presented as Key Finding 2 in Chapter 13— despite the fact that the Executive Summary indicates Chapter 13 as source for that information. The authors need to check for consistency with the latest version of relevant chapters of the report.

RESPONSE: Thank you, all numbers have been checked for consistency as chapters were made final.

P37, Line 25
The methane source from terrestrial wetlands [21 Tg $CH_4$/yr] is different than in Key Finding 2 of Chapter 13 [18 Tg $CH_4$/per year].

RESPONSE: Thank you, all numbers have been checked for consistency as chapters were made final.

P38 [section: *"A systems approach to linkages between the carbon cycle and society"]*

- Despite the definition of "systems" in the footnote, there is no discussion of economics in this section or throughout Chapters 6 and 18.
- As this section provides background information and little new insight, it could be shortened significantly. ● The discussion of $CO_2$ from urban areas confuses the terms "drivers" and "sources". Urban populations drive $CO_2$ emissions elsewhere. (Page 40 – The $CO_2$ that is emitted locally from urban areas are from transportation and natural gas consumption at residential and commercial establishments. $CO_2$ from fossil fuel combustion is released at power plants far away).

RESPONSE: This section was shortened considerably. Discussion about urban areas was clarified.

P40 [Section: *"Projections of the Future ..."*]

- Is important to mention that the capacity of the land and oceans to act carbon sinks decreases with projected climate change.
- Need to include projection of methane.

RESPONSE: We include a paragraph outlining the factors that may cause the land sink to decrease.

P41, Line 16-21
Is important to mention that $CO_2$ fertilization effects are likely overwhelmed by climate change effects.

RESPONSE: We don't fully agree with this statement as it is too broad. Climate change effects are highly variable, and we still lack understanding of large-scale effects of $CO_2$ fertilization.

P42, Line 10-18
Perhaps note the challenges in carbon accounting involving the world commons. Under IPCC rules, coastal states do not get credit for ocean sinks.

RESPONSE:

P42, Line 19-40
This discussion seems disconnected from SOCCR2. Issues of carbon management, systems of governance, etc. could be more fully integrated with discussions in other chapters. While the authors understandably shy away from prescriptive statements, SOCCR2 could usefully inform decision makers about actions that make significant contributions to reducing GHG emissions.

RESPONSE: This section is not meant to contain specific details about potential activities. Editing done to make this part significantly more concise.

P43, Box ES.3
This box is a bit perplexing. Why is the discussion limited to cities? The presence of states, businesses, and tribal governments at Bonn and participating in various climate-related initiatives is likely more substantial and significant. As indicated in Chapter 4, a main obstacle to the ability of cities to influence carbon-flux is jurisdictional fragmentation and the lack of a multi-level system of carbon governance.

RESPONSE: Box ES.5 (not ES.3) has been enhanced to include more statements about initiative besides those by cities.

P43, Line 8
The 3rd option involves storage in geologic reservoirs as well.

RESPONSE: Done.

P43, Line 8
The reference to geoengineering needs to be more carefully worded to avoid giving a mis-impression that solar radiation management techniques could directly affect the level of carbon gases in the atmosphere (they cannot).

RESPONSE: We removed the sentence since it is not relevant to controlling GHGs.

P45, Table ES.1
Why is this included while corresponding treatment for other chapters is absent?

RESPONSE: Table ES.1 was added in response to previous (agency review) comments that suggested we give more treatment to options for reducing fossil fuels. However in response to this and other comments, the table has been simplified in terms of content, and lengthened to add terrestrial and other C management options.

P46, Line 37
This statement suffers from the paucity of data on effects of tribal management practices on carbon fluxes and the lack of a means to "upscale" actions to determine their significance.

RESPONSE: This section was eliminated.

.

P47, Line 2-37
These co-benefits and trade-offs suggest the existence of an integrated cross-boundary economic and jurisdictional system, which does not exist. Should the focus instead be on multi-level integration involving local communities, urban areas, regional, national and international carbon accounting and decision-support systems?

RESPONSE:

We have added relevant content in the ES, Preface and Appendix in response to your comment. Thank you.

Chapter 1 Overview of the Global Carbon Cycle

**Review of the Draft of the Second State of the Carbon Cycle Report (SOCCR2): Chapter 1**

**General Comments:**

***The chapter is not balanced between $CO_2$ and $CH_4$. There needs to be a key finding about $CH_4$. The term "carbon" often refers only to $CO_2$, and not methane (e.g. Section 1.3.2).***
***For methane, it is important to mention recent studies that have used "top down" observations to derive emission estimates that are higher than the "bottom up" EPA inventory estimates (e.g., Kort et al., 2014; Miller et al., 2013; Turner et al., 2016). Studies that have fused large collections of airborne, ground-based, and satellite data should be given particular attention.***

***….Also the discussion of methane is very slim and should be enhanced.***

Strategy: Add more discussion of the global $CH_4$ budget. Clarify throughout whether PgC refers to $CO_2$ or $CO_2$ + $CH_4$.
Mention that inventories have often overestimated $CH_4$ emissions, but try to leave the US emission material for Chapter 2.

***There was not always a consistent mention of cement production and inland waters as important sources of carbon emissions to the atmosphere. And there is some problem with consistency for fluxes within this chapter and Figure 1.1, as well as across other chapters.***

Strategy: Not sure where inconsistency in fluxes is unless it's related to issue above. Pretty sure that consistency among chapters needs to be a top-down post-revision editorial process.
Believe that specific comment responses will address concerns about cement and inland waters.

***Some sections are not well organized or clearly written. For example, Section 1.1 should have, at a minimum, a much stronger introductory sentence that illustrates the fundamental importance of the carbon cycle. See, for example Cole, 2013.***

Strategy: Organization and writing can always be improved given enough time for revision, disagree that this chapter needs substantial re-writing. Specific comment will address the Cole 2013 reference, but we haven't gotten our hands on that book yet.

***Some of the headers are ambiguous and, in some cases, do not describe well the content of the paragraphs that follow. For example: Section 1.1 (The Role of Carbon)—where, and for what? Section 1.2 (The Natural Carbon Cycle)—the 2nd paragraph talks about human influence on the carbon cycle.***

Concerns about headers were brought up in the specific comments and we addressed those. The reviewers have a point about the 2nd paragraph of Section 1.2 and we moved it to Section 1.3 about perturbations of the natural carbon cycle.

***The Chapter is missing a conceptual figure to illustrate feedbacks. The nature and magnitude of feedbacks are likely to be crucially important, which is identified in the text***

***It would be helpful to add:***
***- a figure (conceptual or more specific) that shows carbon cycle feedbacks; - a figure or table illustrating Key Finding 3, "uncoupled from economic growth" - a table or figure that show the relative radiative forcings of greenhouse gases (including non-carbon) as per Key Finding 2—although the authors may wish to consider whether discussion of radiative forcing is even appropriate for this report, or whether that topic should instead be restricted to the NCA report.***

***The suggestion that "global emissions became slightly more uncoupled from economic growth" requires a synthesis figure and/or references to support this finding. Often time frames are missing (see above and line comments)***

***In addition to figure and table suggestions noted above, it would be good to add an explanatory sentence or two about radiative anthropogenic forcing on page 5, should the authors decide to keep radiative forcing in this report.***

Strategy: It seems to me that we're talking about adding 3 figs/tables here. We should discuss this. I have several versions of the radiative forcing contributions based on observations, so that one is easy. Rich and I have sent out a couple of feedback possibilities. I'll see what I can find re KF#3 since that's also a specific comment. A related issue is that several of the public comments asked for flux arrows of the existing figs to be scaled by the relative sizes of the fluxes.

***One of the major new pieces of information for SOCCR2 is the emissions contribution from inland waters. This was not highlighted in the key findings, but should be. The authors should be careful to explain that this flux includes the background flux, not just fluxes in response to anthropogenic emissions.***

Strategy: We need to add some text about the inland waters. Rich sent out a potentially useful reference. I made a change to key finding #4 about inland waters. Probably not sufficient to satisfy this reviewer, but I'm not sure a separate key finding is needed. Will revisit this later, when main text is updated.

***• Are the research needs identified in the report appropriate? They were not explicitly part of this chapter.***

Strategy: adding research needs in text

**Specific Comments:**

***P49, Line 14-20***[AMM1]
***Please reference years or range of years in Key Finding 1 as is done for Key Finding 2.***

Response: done.

***P49, Line 35***
***Change "slowed" to "decreased", since there is a negative value for percentage change.***

Response: done.

***P49, Line 36***
***Flat growth is an oxymoron, it seems.***

Response: done.

***P50, Line 8-9***
***Please clarify, e.g., mitigation activities, such as…***

Response: done.

***P50, Line 13-15***
***This wording is awkward, rewording is suggested.***

Response: done.

***P49, Line 13 – P50, Line23***
***Missing from the findings are a direct reference to (1) new global emissions estimates from inland waters and (2) the proportion of cement production emissions contributing to global CO2. The data for (1) are in Fig 1.1***

Response: partially done – will consider again later.

***P50, Line 24***[AMM2]
***This chapter should start with some introductory text.***
We added an introductory paragraph here that we hope will set the stage for the rest of the global carbon cycle overview in this chapter.

***P50, Line 24 – P51, Line 15***
***This section should be edited for content and clarity. (Role of Carbon—where, for what?)***

We changed the title of this section to make it more descriptive. We also re-read and edited the paragraph where necessary for clarity.

***P50, Line 25-27***
***The first couple of sentences are vague. See Cole, 2013.***

We disagree that the first couple of sentences are vague. We are trying to get the Cole, 2013 book since the reviewers recommend it. We may revisit this item.

***P51, Line 16-21***
***Some background discussion about the Earth system would be helpful here: for instance, give %marine, %land, % terrestrial, % freshwater, % ice.***

We are unclear about what the reviewers want here. We have interpreted this to mean that they would like us to discuss how much carbon is stored in the various reservoirs, so we have added estimates (in approximate %).

***P51, Line 16-21***
***Missing from the system discussion and from Figure 1.1 are feedbacks; this should be included.***

There is a more detailed discussion of feedbacks in section 1.4, but we introduced the idea here as well. We are considering a feedback figure.

***P51, Line 38-40***
***Add temporal reference for slow carbon cycles and geologic reservoirs.***

We think this is a request for timescales of transfers between the geologic reservoirs. These estimates are already given later in the same paragraph.

***P52, Line 4-11***
***Mention spatial extent of carbon stored in soils, permafrost, etc.***

We added more details about the distribution of soil carbon and more information about permafrost.

***Page 52, Line 29***
***The Southern Ocean is the largest region of carbon sink (e.g. Gruber et al., 2009). It is more diffuse than the North Atlantic, but acts over a much larger area. It should be mentioned here forcompleteness. Also, "top-down" estimates of the North American CO2 sink are tied to the highly uncertain magnitude of the Southern Ocean sink.***

We included a mention of the Southern Ocean as a region of uptake.

***Page 52, Line 31***
***While small interannual variability of the ocean is what Wanninkhof et al., 2013 suggests, newer information suggests that the variability is likely substantially larger than previously thought (Landschützer et al., 2015).***

We thank the reviewers for pointing out that we needed to include results from this interesting paper. We added a brief discussion of these results.

***P52, Line 37-38***
***This reads as if El Nino of 2016 is the only driver.***
We clarified our use of ENSO as an example of a driver of IAV in carbon fluxes.

***P53, Line 4***
***Add time frame***

The period covered by the ice record is mentioned in the same paragraph.

***P53, Line 7***
***Why "note?"***

We removed this unnecessary phrase.

***P53, Line 36***
***Many of these natural processes are anthropogenically influenced or are a results of feedbacks.***

We agree and added a sentence that describes these anthropogenic and climate feedbacks.

***P54, Line 6***
***Since 1870? From when to 1870? Is the time frame 1870-2014?***

We added the correct time frame from LeQuere et al. (2016).

***P54, Line 7-11***
***Need references and or a synthesis figure to support this assertion.***

***P54, Line 27-28***
***It seems odd to include the reference on how to avoid emissions here.***

We think sentence and its reference fits into this paragraph.

***P54, Line 29-32***[AMM3]
***Name major sources of OH, to provide more context for this sentence.***
We have added text to clarify the proportion of the $CH_4$ sink attributable to reaction with OH, the uncertainty associated with that sink, as well as basic information on how OH is produced and destroyed.

***P54, Line 22-32***
***This paragraph mixes emissions and reduction of emissions strategies and processes.***

We have now separated this paragraph into two, with the first focusing on the major methane sources and sinks, and the second focusing on a couple of examples of strategies for reducing emissions or enhancing the OH sink.

***P55, Line 20***
***What time frame is used for the cumulative emissions discussion?***
We have clarified that the timeframe for the cumulative emissions estimate is 1850-2011.

***P55, Line 33-39***
***The accounting is not clear as written. North American emissions (from fossil fuel burning and cement production only—not inland waters?) are being compared to the terrestrial sink in North America?***
That is correct. This passage is comparing anthropogenic emissions to the terrestrial carbon sink.

***P56, Line 3-4***
***Where is the boundary vis-à-vis ocean uptake? Is any part of that considered a North American sink? This is unclear. The text would benefit from a figure showing feedbacks and how they may interact to influence the future carbon cycle.***[AMM4] These numbers and percentage for North America referred only to the carbon sink attributed to the terrestrial biosphere, and the percentage is therefore based on the global uptake by the terrestrial biosphere. This has been clarified.

***P56, Line 25-27***
***As is pointed out elsewhere in the document, the situation is not quite as direct as CO2 causing a direct fertilization effect.***
We have edited the wording to avoid a over-simplified reference to a direct fertilization effect, and also added a brief description of other factors that have affected the cumulative land sink over the 20$^{th}$ century.

***P56, Line 32-35***
***This paragraph is rather superficial. In particular, deeper discussion of the likely future response of the ocean sink is needed, as it is not as simple as suggested here. There is substantial uncertainty in future uptake, as a function of ocean circulation, warming, chemical changes (Lovenduski et al., 2016; Randerson et al., 2015). The sensitivity of the ocean sink to emission pathways needs more study. The idea of a continually growing sink in the ocean only applies under a high emission trajectory. If a low emission trajectory is taken, the ocean should outgas carbon, particularly in the subtropics where waters with high anthropogenic carbon content are circulating in the upper ocean (DeVries et al., 2017).***
This paragraph has been rewritten to offer a more detailed view, and the first two references suggested by the reviewer have been incorporated.

***P56, Line 4***
***The title for this subsection is mismatched with content.***
We believe this comment referred to P57 Line 4. This title has been changed to "The Carbon Cycle and Climate Mitigation."

***P57, Line 26 Safe for***
***what?***
This is an excellent question, and indeed gets at the heart of the challenge associated with defining levels of "acceptable" climate change. This has been clarified in the revision, by adding ", and the definition of "safe," as well as the components of the Earth system that the term applies to, are themselves subjective."

***P59, Line 4-5***
***There are too many "furthermores" used here.***
Two of the three instances of "furthermore" have been removed from this paragraph.

***P60, Line 14***
***This should be estimates of cumulative carbon emissions, correct?*** Correct. Edited as noted.

***P61, Line 22-26***
***It would be helpful to see a summary figure showing the economic and emissions data.***[AMM5]

***P61, Line 35-39***
***This is a vague paragraph.***
This paragraph has been updated to better explain the scale and source of the uncertainties.

***P62, Line 15-17***
***What about emissions from inland waters?***
Emissions from inland waters are currently not typically broken out in these global constraints, and are therefore implicitly included as part of the terrestrial component. A sentence to clarify this has been added.[AMM6]

***Page 62, Line 15-26***

***The presentation here suggest equal uncertainty in the land and ocean sinks, which is not the case (LeQuéré et al., 2016; 2017). In addition, the approach to estimation of the ocean sink is misrepresented. The cumulative ocean sink is best constrained using ocean interior data (DeVries, 2014; Khatiwala et al., 2009; 2013; Sabine et al., 2004; Sabine and Tanhua, 2010), surface ocean pCO2 data can provide independent confirmation of the magnitude of the mean sink (Landschützer et al., 2013; 2014; 2015; 2016; Takahashi et al., 2009). There remains substantial uncertainty with respect to interannual variability in the global carbon cycle, with models believed to strongly underestimate the actual variability (Landschützer et al., 2015). These models are quantitatively tied to the estimates from interior ocean data and surface ocean pCO2.***

This paragraph has been substantially updated and expanded to discuss the relative uncertainties of ocean and terrestrial sinks in terms of mean flux, interannual variability, and cumulative sink, as well as to expand the discussion of the approaches used to constrain each of these.

**NAS Review Comments (Narrative Response): Chapter 2**

We thank the reviewers for their constructive comments, which have helped improve this chapter. Here we describe the actions that we have taken to address the main issues provided by NAS for Chapter 2.

**1-** ***Key findings***: We have revised the key findings to account for NAS comments and public comments. Specifically, we have modified key findings 1, 4 and 5, but we have also revised the overall wording of this section. We kept the key findings within a vision of the continental-scale for North America and avoided any sector-specific (e.g., forestry, soils, oceans) highlights as they pertain to sector-specific chapters in SOCCR-2.

**2-** ***Interannual variability of carbon fluxes and impacts of severe and extended droughts***: We have added text and citations to highlight the importance of interannual variability and the impact of droughts mainly across the southwestern United States (see section 2.5.2). We clarify that the goal of this chapter is to compare information at the continental scale from SOCCR-1 and SOCCR-2; consequently, quasi-decadal estimates are presented. These estimates summarize information from bottom-up and topdown approaches, but interannual variability is not explicitly captured by bottom-up approaches (e.g., surveys, inventories). Key findings have been edited to improve clarity. We recognize that past interannual variability is explicitly discussed in Chapters 8 and 19.

**3-** ***Indicators and feedbacks are missing from Section 2.4***: We have renamed this section as "Trends in the North American carbon cycling". Thus, we do not discuss indicators and feedbacks.

**4-** ***Data analysis***: We have not performed any formal statistical analyses as this chapter is a synthesis of information from published literature and the main findings of other chapters in SOCCR-2. We recognize that there are differences in methodologies and sampling intervals between top-down and bottom-up approaches. After consideration of this point and based on guidelines provided by the science-leaders of SOCCR-2, we only report values that are consistent with published literature and from other chapters of this report. We now conclude that there is an "apparent agreement" between bottom-up and top-down approaches as an expert opinion conclusion from available information provided throughout SOCCR-2.

**5-** ***Improvement of clarity in the text and figures***: We have carefully revised the main text based on NAS comments and have edited the Figures to improve clarity and presentation.

**Point-by-point response to reviewers from National Academy of Sciences**

Overview/Main Issues

Comment: This chapter nicely summarizes and synthesizes the latest scientific information on the North American carbon budget by incorporating terrestrial, anthropogenic, aquatic, and coastal margin $CO_2$ and $CH_4$ dynamics. Starting with a historical context, the chapter summarizes current understanding of the magnitudes and trends of carbon stocks and fluxes at the continental scale. It also provides a regional context by stratifying the continent to countries and climate assessment regions and discusses the societal drivers, impacts, and carbon management decisions. Knowledge gaps and research needs are also identified. This chapter is well-written and clearly organized, and provides a broad context beyond individual chapters.

*Response: We greatly appreciate your positive comment about our report and hope that you find the content useful*.

Comment: Some of the main ways the chapter can be improved include the following:

- some work is needed on the Key Findings (discussed below);

*Response: Key Findings have been edited to address NAS and public comments.*

- the goals and objectives should be explicitly described;

*Response: The sections identified have been rearranged to incorporate your suggestion. Goals and objectives have been moved early to the beginning of the introduction.*

- critical content areas missing from the chapter are interannual variability of carbon fluxes and impacts of severe and extended droughts;

  *Response: We have recognized the issue of interannual variability and droughts in the edited version (see below). We clarify that interannual variability cannot be explicitly discussed because this chapter synthetize efforts from top-down and bottom-up approaches, where bottom-up approaches do not capture interannual variability (e.g., forest inventories).*

- indicators and feedbacks are missing from Section 2.4;

  *Response: This section has been renamed as: "Trends in the North American carbon cycling"*

- consistent use of units is recommended;

  *Response: We have revised the document for consistent use of units.*

- numbers with 3-4 significant digits over-state the confidence the reader should have, and all numbers should include uncertainties.

*Response: We report to the closet Teragram (Tg) and uncertainties are reported based on information from other chapters in the SOCCR-2.*

Comment: And one broader concern to note: This chapter follows the global overview in Chapter 1, where "sinks" are sinks in the cycle perturbed by anthropogenic $CO_2$ and $CH_4$, and the assumption is that globally, the net unperturbed background sinks are zero summed across all reservoirs. Yet in this chapter, "sinks" are net fluxes out of the atmosphere, background + perturbation. For the coastal ocean, inland waters, etc.—where lateral transport is significant—these sources and sinks include background/preindustrial fluxes that are balanced by fluxes elsewhere. These distinctions must be made clear so that the reader is not given an impression of a greater or lesser sink for anthropogenic $CO_2$ than is there (e.g., P74, lines 4-6).
*Response: We find this comment unclear as our goal was to recognize and include carbon fluxes that were reported throughout SOCCR-2.*

*We have modified the sentence of original P74 (lines 4-5) and now it reads: " North American land and its adjacent oceans almost certainly represent a net sink for atmospheric $CO_2$ excluding anthropogenic emissions (King et al., 2015; Peters et al., 2007)".*

Statement of Task Questions

- Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?

The goals and objectives of this chapter are described in an implicit way at the end of the Introduction section. It would be better to rephrase that paragraph to clearly outline the goals and specific objectives. The intended audience is not clearly described. But this is perhaps something that only needs to be described in the Executive Summary. The report meets its stated goals to a large extent, with one exception—indicators and feedbacks are not clearly described.
*Response: The text has been revised to incorporate this suggestion. The objectives of this chapter are now included in the first paragraph of the introduction. We have removed the intent of describe indicators and feedbacks from the introductory statement and form section 2.4.*

- Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report? The report accurately reflects the scientific literature to a large extent. For part of the Introduction and the Historical Context section, it is appropriate to cite some older but classical studies (e.g., Caspersen et al., 2000; Goodale et al., 2002, etc) for the historical context of the North American carbon cycle studies. One more recent study could be useful here is Zhang et al. (2012), which shows that on average the carbon sink in the conterminous U.S forests from 1950- to 2010 was Tg C/yr 87% of the sink of living biomass.

*Response: The text has been revised to incorporate this suggestion. We have included the following reference for the historical context in this chapter: Goodale et al., 2002 as it addresses the North American perspective. We recognize that other chapters will cover in detail the historical context of other components in the carbon cycle of North America.*

Comment: Two critical content areas missing from Chapter 2 are the interannual variability of the terrestrial carbon sink and the impacts of drought. It would be better to have a section to address the interannual variability of carbon fluxes. Besides disturbances, drought has significant impacts on ecosystem carbon uptake and has been studied extensively using modeling and upscaling methods in North America.
*Response: The text has been revised to incorporate this suggestion. We have changed the context of section 2.4 and avoided to address feedbacks and indicators. We now only focus on "trends". That said, we recognized the importance of interannual variability and drought in the revised version.*

Comment: Another critical content area missing is indicators and feedbacks. The title of Section 2.4 is "Indicators, Trends, and Feedbacks", but the section does not really touch on indicators or feedbacks. These two components should be added or strengthened; alternatively, the title should be changed to "Trends".
*Response: The text has been revised to incorporate this suggestion. This section has been renamed as: "Trends in the North American carbon cycling"*

Comment: Discussion of the methane budget should be expanded and updated in a few ways:
- Throughout the chapter, "carbon" refers to CO2 and not methane. The chapter should mention the difficulty of unravelling methane sources and sinks, given that there are so many sources. The chapter should include references for fossil fuel methane emissions: e.g., Hendrick et al. (2016), Jacob et al. (2016), Kort et al. (2014), Turner et al. (2016).

*Response: We have included some of these references: Hendrick et al 2016, Turner et al 2016. We have included a statement in the introduction that now reads: " We highlight that there are still challenges for unravelling $CH_4$ dynamics across North America as there are multiple natural (Warner et al 2017) and anthropogenic (Hendrick et al 2016, Turner et al 2016, NAS 2018) sources and sinks that still need to be fully quantified".*

Comment: The chapter should also mention that there is not agreement about whether U.S. methane emissions or methane sinks are under-estimated or not; e.g., see Bruhwiler et al. (2017), Miller et al. (2013), Turner et al. (2015), and Wecht et al. (2014). *Response: To the discussion on methane uncertainty (as in comment above), we added: "Furthermore, there is not agreement among recent studies as to whether or not the reported magnitudes of methane emissions and sinks in the US are under-estimated (Bruhwiler et al., 2017; Miller et al., 2013; Turner et al., 2016).".*

Comment: Table 2.1 of the 2017 EPA report (EPA 430-P-17-001) shows 2015 has lower total emissions than 2005. The upticks in "natural gas systems" and "manure management" are nearly cancelled by downturns in emission from landfills. This has important information for managing the methane budget. Where possible, the numbers and conclusions should be consistent with this EPA report; departures from and updates of that report should be highlighted.
*Response: The text has been revised to incorporate this perspective. We have included text for management of methane fluxes and cited the 2017 EPA report. Otherwise, the SOCCR-2 complements the 2017 EPA report but it is different as we synthetize the state-of-the-art knowledge of carbon dynamics across North America (including Canada, Mexico, and the United States), and use information from top-down and bottom-up approaches. These two approaches are not equal but we clarified that there is an "apparent agreement" (between bottom-up and top-down approaches) along Chapter 2.*

Comment: Coastal oceans were reported as a carbon source in SOCCR1 but as a carbon sink in SOCCR2. This can perhaps be considered as a key finding. In addition, it would be useful to have a key finding on $CH_4$ budget.

*Response: We have highlighted in the text the issue about the coastal ocean, but that key finding pertains to Chapter 16. In this chapter we focused on a synthesis vision of the carbon cycle (both $CO_2$ and $CH_4$) across North America. We have cited the 2018 NAS methane report in Chapter 2. However, we do not feel that the synthesis presented here on the methane budget provides significant enough new information to warrant a key finding for this chapter.*

- Are the findings documented in a consistent, transparent and credible way?

Comment: The way that Key Finding 5 is documented needs a bit of work. "Significantly" is typically associated with a statistical test. Is the average carbon sink estimated by top-down approaches not statistically higher than that by bottom-up estimates? If a test couldn't be done, "significantly" should be replaced with something like "quite" or "substantially".

*Response: The text has been revised to incorporate this perspective. The Key Finding 5 now reads: "There is an apparent agreement in the estimates of the magnitude of the continental carbon sink over the last decade between top-down (634 Tg C per year ± 45%) and bottom-up (577 Tg C per year ± 75%) approaches, considering the uncertainty ranges around these two calculations."*

- Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?

Comment: Key Finding 1 is undoubtedly true but this has been known for a long time, thus it may be unnecessary as a key finding. It includes a new estimate (1,032 Tg C per year), but this number is already reflected in Key Findings 2 and 3. Note also this should say "net source of CO2" (not carbon).

*Response: The text has been revised to incorporate this perspective, but believe that it is an important, first key finding to form the context within which the other findings can be interpreted. First, we changed this to CO2. Second, we are highlighting that this is a new estimate for the SOCCR-2 time period. Third, as found in the other key findings as well as the main body of the chapter, there are several new pieces of information synthesized here that result in this finding. KF#1 now reads: "The North American continent—including its energy systems, land base, and coastal oceans—was a net source of $CO_2$ to the atmosphere from 2004 to 2013, having contributed on average approximately 1,032 teragrams of carbon (Tg C) (±50%) annually."*

Comment: Key Finding 4 should reflect the fact that there is significant interannual variability in the carbon sink. It is also unclear what is meant by "natural terrestrial carbon sink." Does this include natural ecosystems such as forests and grasslands? Are agricultural soils included? Even forests/grasslands are managed to a large extent. Does this include wood products, land use changes, etc., which may be considered anthropogenic rather than natural? The authors should either specify what is included (in parenthesis following "natural terrestrial carbon sink") or slightly rephrase the term as something like "land/ecosystem sink." This also applies to Key Finding 3.

*Response: These are important comments, and we agree that such statements need to be made more clearly here given these various key issues. However, the details of these cannot be covered in a short and concise statement on a specific key finding. Therefore, we have changed the focus of KF#4 to the general consistency in the SOCCR-1 and SOCCR-2 sink estimates, rather than on the more complicated drivers of the sink. Now it reads: "Given the ranges of uncertainty around the two estimates, there is an apparent consistency in the bottom-up, inventory-based calculations of the average annual strength of the land-based carbon sink between that reported here (577 Tg C per year ± 75%) and in SOCCR-1 (505 Tg C per year ± 50%)."*

Comment: Key Finding 4 and 5 should include quantitative estimates and uncertainties. No need to emphasize the approach (topdown, bottom-up), just focus on the actual outcomes.

In the regional context, it would be good to add a figure to quantitatively illustrate how much the size of the carbon sink is in each national climate assessment region, how much fossil fuel emissions are released in each region, and what percentage of the emissions is offset in each region.

*Response: Quantitative estimates and uncertainties have been added to both KFs #4 and #5. We have left the reference to the approaches, however, since we feel the convergence of estimates between the top-down and bottom-up perspectives is a key finding. We agree with the suggestion that region-by-region C budgets would be very valuable, but we deemed that unfortunately there are not enough consistent and comprehensive data sets available across all NCA regions to make this possible at this time.*

- Are the research needs identified in the report appropriate?

Comment: Research needs could include how to better integrate modeling approaches with observations and how to reduce the uncertainty in carbon sink/source estimates.

*Response: The final paragraph of the chapter (in section 2.7 Synthesis, Knowledge Gaps, and Outlook) provides this perspective as an overview of future research priorities – mainly synthesized from those across the other chapters in this report – including and especially the integration of field and remote sensing data in modeling efforts.*

- Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?

Comment: As mentioned above, the authors should test whether the mean of the carbon sink estimates from top- down approaches is not significantly higher than that of the sink estimates from the bottom-up methods. Otherwise, the statement should be rephrased accordingly. In addition, both Tg C and Pg C are used for carbon fluxes (larger than 1,000 Tg C or 1 Pg C). It would be better to use the same units (Tg C) throughout the chapter. In addition, both Tg C per year (page 80, line 11) and Tg $CH_4$/yr (Figure 2.4) were used. While this is commonly done in the scientific literature, it may be helpful to include in parenthesis CO2-equivalent for both $CO_2$ and $CH_4$.

*Response: The text has been revised to incorporate this perspective. We have rephrased the text to mention that there is an "apparent agreement" between bottom-up and top-down approaches. We have also revised the text to report only Tg C. We have not performed any further statistical analyses as this report relies on published literature, but also it is not possible to compare these two approaches because there are conceptual and technical differences between them.*

- Are the document's presentation, level of technicality, and organization effective?

Comment: There are repetitions or mistakes in sentences. For example, the statement on P82, Lines 14-16 is essentially the same as the first two sentences of the following paragraph. These two paragraphs should be combined and modified.

*Response: The text has been revised to incorporate this perspective. These two paragraphs have been combined into one and redundant statements have been deleted.*

Comment: In addition, Key Finding 1 is exactly the same as Key Finding 5 and should be corrected. This chapter can be difficult to read at times, with awkward use of language in many places (e.g. Key Finding 2: "a level of magnitude"). The chapter would benefit from editing.
*Response: The key findings have been edited and we have revised the overall document for consistency. Furthermore, KF #1 states and quantifies the overall, emissions-driven CO2 source to the atmosphere estimated from this report, whereas KF #5 focuses on top-down vs. bottom-up estimates of the land sink component of the continental carbon cycle.*

- What other significant improvements, if any, might be made in the document? - For Section 2.4, the first two paragraphs would fit better into Section 2.3.

*Response: The author team has deliberated and agreed to keep these two paragraphs under section 2.4 as they are considered a transition from section 2.3 to 2.4. Furthermore, the title of section 2.4 has been modified to: "Trends in the North American carbon cycling"*

- Section 2.4 should also discuss the interannual variability of carbon fluxes besides indicators and feedbacks.

*Response: The text has been revised to incorporate this perspective. First, the title of section 2.4 has been modified to: "Trends in the North American carbon cycling". The issue of interannual variability has been added but not fully discussed as it is not a main focus of this chapter or this revised section.*

- Section 2.5.2 should quantitatively describe the regional carbon sinks, fossil fuel emissions, and the percentage of fossil fuels emissions are offset by ecosystem carbon uptake.

*Response: As with the comment above, we agree with the suggestion that region-by-region C budgets would be very valuable, but we deemed that unfortunately there are not enough consistent and comprehensive data sets available across all NCA regions to make this possible at this time. Furthermore, we have not performed new, specific statistical analyses as the goal of this report is to present a synthesis of information derived from other chapters in SOCCR-2. We provide a summary table using information from this report (see Table 2.2) and a synthesis figure (Figure 2.3) which we believe is a useful summary for carbon dynamics at the continental scale.*

Comment: The discussion throughout the chapter on “top-down” and “bottom-up” is not informative to the general readership of the report, and could be shortened considerably. The authors are advised to carefully check the “Inventory of U.S. Greenhouse Gas Emissions and Sinks 1990-2015”, and see whether the estimates of emissions and sinks should be compared against SOCCR2 estimates and whether there are nice figures that can be used in SOCCR2.
*Response: The text has been revised to incorporate this perspective. We believe that this section is important as we feel the convergence of estimates between the top-down and bottom-up perspectives is a key finding of this chapter. As such, the minimum explanation of what each approach includes and measures – and how they do and do not compare – is critical to the premise of this chapter, i.e. synthesizing and reconciling estimates of the various components of the continental carbon budget.*

Line-Specific Comments
P71, Line 21-25 Why not use Pg C here, as is used elsewhere (e.g., p.73, Line 41). Having more consistency in units will make the report less confusing to the audience.
*Response: The chapter presents units as Tg. We have revised the units in former P71, Line 21-25.*

P71, Line 30-33 This key finding ignores the previous research findings showing interannual variability in ecosystem carbon fluxes caused by drought and disturbances.
*Response: This is an important comment pointing out that our wording needs to be more careful so that the findings are not misleading. In this case, trends or interannual variability in carbon uptake can be ascertained from the inverse models because of their annual precision. However, this is not the case with the inventory-based estimates that really represent an “average” over a coarser time period such as a decade. Furthermore, we agree that the details of the various drivers cannot be covered in a short and concise statement on a specific key finding. Therefore, we have changed the focus of KF#4 to the general consistency in the SOCCR-1 and SOCCR-2 sink estimates, rather than on the more complicated drivers of the sink. Key Finding # 4 has been revised to read: “Given the ranges of uncertainty around the two estimates, there is an apparent consistency in the bottom-up, inventory-based calculations of the average annual strength of the land-based carbon sink between that reported here (577 Tg C per year ± 75%) and in SOCCR-1 (505 Tg C per year ± 50%).”*

P72, Line 5 Change “3 centuries” to “three centuries”.
*Response: Done.*

P72, Line 9-10 This statement needs to be rephrased. Continental carbon sources are only partly offset by sinks from natural and managed ecosystems.
*Response: The text has been revised to incorporate this perspective. We modified the sentence to say "continental carbon sources are partially offset by sinks from natural....."*

P72, Line 21 The focus here should not be North American carbon balance, but North American carbon sink, or the carbon sequestration capacity of North American ecosystems.
*Response: We disagree with this comment as this sentence highlights the limitations of the variability of sources and sinks. Consequently, this uncertainty limits our capacity to predict future carbon balance across North America.*

P72, Line 23-25 Since this statement is put in the historical context, it is better to cite some older and classical studies (e.g., Caspersen et al., 2000; Goodale et al., 2002).
*Response: These references have been included.*

P72, Line 38 The authors should mention that atmosphere-based estimates provide limited information on the exact location of carbon sinks/sources.
*Response: The text has been revised to incorporate this perspective. Now it reads: "Atmosphere-based estimates are broadly inclusive and include all surface-atmosphere $CO_2$ exchange as one integrated flux, but there is limited attribution information on stock changes within individual components, internal processes, lateral transfers, or exact location of carbon sinks/sources."*

P74, Line 6 Change "50%" to a specific number (in units of teragrams).
*Response: The text has been revised to incorporate this perspective. Now it reads: " In the ca. 2003 time frame, which includes SOCCR-1, estimates of North America's terrestrial sink was estimated at 505 teragrams (Tg) Cper year (plus minus 50%), which represented about 15% to 40% of continental fossil fuel emissions (Pacala et al., 2007)."*

P74, Line 9 The phrase "be of sufficient magnitude" is redundant and can be removed.
*Response: The text has been revised to incorporate this perspective. Now it reads: " More recent analyses suggest the terrestrial carbon sink continues to offset a substantial proportion of the carbon source from fossil fuel emissions, though estimates of this proportion..."*

P75, Line 29-42 This paragraph is somewhat disconnected. The first two sentences are on the missing components of synthesis efforts, while the following sentences are on inventories or methane. Is methane one of missing components? This paragraph should be modified to improve logical flow.
*Response: Yes, the intended flow of the paragraph was to include $CH_4$ as an important component missing from previous synthesis efforts. The paragraph has been modified to improve logical flow by clearly stating this assertion: "Also missing from carbon budget studies historically has been a comprehensive assessment of $CH_4$ fluxes. Despite being an important carboncontaining GHG, but $CH_4$ budget synthesis efforts have been limited to a few global-scale, atmospheric-based estimates…"*

P76, Line 18-21 It would be better to briefly summarize how the estimates across various scaling approaches were reconciled. *Response: It would be difficult to briefly summarize the various sectors, data sets and techniques used to reconcile these estimates, so we feel it is appropriate to refer the reader to this publication for the details. Here we added the term "best estimate" as a very general reference to the way in which the various estimates were reconciled: "Reconciling the estimates across these various scaling approaches, King et al., (2012) concluded that the "best estimate" of the magnitude of the continental $CO_2$ sink early in this century was 635 ±26% Tg C per year, offsetting about 35% of fossil fuel emissions over that time period."*

P76, Line 22 It is better to switch "fluxes" and "stocks" in the section title because the first paragraph is on stocks, not on fluxes. *Response: Done.*

P76, Line 41 "There is very high confidence that" can be removed.
*Response: We have been asked in past reviews to keep this statement to indicate a "level" of certainty along the text.*

P77, Line 32-33 What is the difference between "uncertainty and "error"? It would be good to make this clear.
*Response: The text has been revised to incorporate this perspective. We modified this statement to be explicit about what "error" means. Now it reads "... uncertainty at the scale of individual cities (see Ch. 4: Urban) is poorly constrained, ranging from 50% to 100% variation around the mean.".*

P78, Line 20 Starting the paragraph with "Figure 2.3" makes the paragraph look like a figure caption. (May be confusing to readers).
*Response: The text has been revised to incorporate this perspective. Now it reads: " We developed a continental-scale diagram showing the flows of carbon among the major components of North America for the decade since the ca. 2003 estimates reported in SOCCR-1 (Figure 2.3). This figure aimed to reconcile atmospheric flux and lateral transfer estimates with estimates of stock changes among the major sectors treated throughout this report".*

P78, Line 27 "-274 Tg C per year" should be changed to "274 Tg C per year". *Response: Done.*

P80, Line 25 – P81, Line 24 These two paragraphs are not on indicators, trends, or feedbacks (except the last couple of sentences of the latter paragraph). Perhaps integrate these paragraphs into Section 2.3?
*Response: The author team has deliberated and agreed to keep these two paragraphs under section 2.4 as they are considered a transition from section 2.3 to 2.4. Furthermore, the title of section 2.4 has been modified to: "Trends in the North American carbon cycling."*

P80, Line 33-35 This is actually something new and could be considered a key finding of the chapter.
*Response: We tried to keep this chapter with a general perspective and that specific key finding is highlighted in Chapter 16, which is the chapter that presents these advancements.*

P81, Line 17-19 The so-called "browning" and "greening" trends should not be interpreted as evidence for vegetation carbon gains or losses. These trends are based on the normalized difference vegetation index (NDVI). They can indicate trends in photosynthetic activity or gross primary productivity, but not net carbon uptake.
*Response: The text has been revised to incorporate this perspective. Now it reads: " Studies based on time-series, optical satellite data have shown both "greening" in Arctic tundra and "browning" in boreal forests (e.g., Beck and Goetz 2011), suggesting regional variability in vegetation photosynthetic dynamics that could lead to carbon gains and losses, respectively (e.g., Epstein et al., 2012)".*

P84, Line 2-6 Surprisingly, drought was not mentioned here or anywhere else in the chapter.
*Response: The text has been revised to incorporate this perspective. We have modified this section and it reads: "Furthermore, there is recognition of the importance of drought for the inteannual variability of carbon dynamics in water-limited ecosystems across the southwestern United States (Schwalm et al 2012, Biederment et al 2016). Future carbon sinks in the western United States are projected to decline from the region's contemporary rate of uptake. The projected decline occurs mainly in ecosystems of the Northwest region in response to future climate warming and associated drought effects (Liu et al., 2012).*"

P86, Line 20 The authors should provide a few examples of observational networks, particularly those that have emerged since SOCCR1.
*Response: The text has been revised to incorporate this perspective. We refer the reader to Appendix A which has an extensive list of networks across North America.*

P86, Line 21-29 Perhaps provide an example about how interoperability can benefit carbon management decisions?
*Response: The text has been revised to incorporate this perspective. We have including the following statement: "For example, interoperability could be increased by defining inventory protocols (conceptual barrier), using standardized instrumentation (technological barrier), defining the specific roles of participants (e.g., researchers, government agencies), and being sensitive to cultural expectations (e.g., perception of data ownership)."*

P86, Line 34 Why is "very likely" used here? This should be definitely true.
*Response: The words "very likely" have been removed.*

P87, Line 20-21 This statement should be reflected in Key Finding 4.
*Response: We have added the quantities (of the sink estimates) from the two reports to KF #4: "Given the ranges of uncertainty around the two estimates, there is an apparent consistency in the bottom-up, inventory-based calculations of the average annual strength of the land-based carbon sink between that reported here (577 Tg C per year ± 75%) and in SOCCR-1 (505 Tg C per year ± 50%)." Unlike like statement in the text, however, in the KF we use the smaller sink estimate excluding the coastal ocean uptake term, which is more comparable to the atmospheric estimate.*

P88, Line 10 "land and water fluxes" should be changed to something like "carbon fluxes from land and water" to avoid confusion. Water fluxes are generally used to indicate evapotranspiration, river discharge, etc.
*Response: Done.*

P88, Line 15-19 These sentences clearly indicate that there is large interannual variability in ecosystem carbon fluxes, which contradicts Key Finding 4 (that the magnitude of the terrestrial carbon sink has persisted at a similar magnitude over time). The interannual variability in ecosystem carbon fluxes is overlooked in this chapter and should be highlighted. Upscaling of flux observations and biosphere modeling studies also show significant interannual variability.
*Response: We clarify that there is an "apparent agreement" between bottom-up and top-down approaches. We also stated that interannual variability cannot be assessed by bottom-up approaches 'because of averaged stock change estimates over the longer time periods between inventories." We have recognized in the main text that interannual variability is evident, but we have kept the overall message as a comparative approach for a longer time period between what was reported in SOCCR-1 and in this new report.*

P90, Line3-5 Key Finding 1 here is exactly the same as Key Finding 5.
*Response: We respectfully disagree. Finding 1 refers to the carbon balance of the continent and key finding 5 is based on agreement between bottom-up and top-down estimates.*

P103, Figure 2.1. Figure Legend: Spell out NCA; change "Forest" to "Managed Forest"?; change "Other Land" to "Other Land Ecosystems"?
*Response: We spell out NCA at the end of the figure legends. We kept all other definitions as per Wei et al 2013 as cited in the figure legend.*

P104, Figure 2.2. The graphics can be improved. Change left vertical axis label to "Fossil fuel emissions (PgC)".
*Response: This figure has been revised with all of the suggested improvements to te graphics, including the axis label and the formatting of the legend.*

P105, Figure 2.3. Figure caption: The numbers for net carbon uptake (top to bottom) are written in the opposite direction to those for carbon release (bottom to top), which makes the figure a hard to read. Why not show all these numbers in the same direction (from bottom to top)?

*Response: We have revised this figure so that all numbers have been re-aligned to the same direction – in horizontal alignment for best readability.*

P106, Figure 2.4. Figure caption: change "million tons" to "Tg" Wetland $CH_4$ emission of 47 Tg is much higher than the value in Chapter 13 (18 Tg $CH_4$/yr) and the Executive Summary (21 Tg $CH_4$/yr).
*Response: The suggested change has been made to the caption of this figure.*
*The numbers are different than the Executive summary because we are using modeled estimates for North America from the recent assessment by the Global Carbon Project (Saunois et al. 2016).*

P109, Table 2.2. In the heading, change "central estimates" to "Median Estimates" (or some other appropriate description).

*Response: We changed the wording to "median estimates".*

**Chapter 3 Energy Systems**

**Energy Systems Responses to the NAS review**

This paper presents our understanding of the NAS review of Chapter 3, Energy Systems in the SOCCR2 draft and Chapter 3 responses to the comments and suggestions. We thank the reviewers for their detailed read and helpful suggestions, questions and overall impressions. We believe that the chapter is much better because we have addressed these queries.

The report is structured in two parts. The first presents the understanding of the overall NAS comments and categorizes these presented comments into different groups. In the second part, the report presents the specific comments and responses. In each case, the NAS comments are in italics and the responses are provided directly below the NAS comments in bolded font.

**I. Overall NAS comments**

*Overall, the NAS review argues that the chapter has two major weaknesses. First, comments state that the chapter lacks enough information for decision makers to take action prescriptively. Reviewers do not suggest any particular prescription but argue that trends and conditions related to mitigating carbon emissions have not been addressed adequately. More information on the role of the dynamics and factors in increasing or decreasing energy use and carbon emissions and what this might mean for our changing climate was requested. More specifically, information on how to mitigate these trends is considered necessary. Second, they note that the chapter is filled with important information about the current North American energy system but lacks enough synthesis to be useful to readers. In part, this is related to the lack of clear mitigation alternative direction for decision makers and in part this is due to the lack of detailed illustrations, examples and synthesis of the information provided. Both of these comments are set out in a number of different and more specific comments for the chapter authors to consider.*

**Response: Chapter 3 authors agree with both criticisms and have attempted to address them. The specific NAS comments and suggestions made are provided below followed by our responses.**

*We have divided the overall comments from NAS into three separate criticisms related to both the substance of the chapter and specific parts.*

*The first overall comment was that Chapter 3 "loses the forest for the trees." The reviewers recommended addressing this issues by:*

- *Including Sankey diagrams that show energy system in the context of the overall carbon cycle and/or a pictorial illustration of energy system stocks and flows by source (similar to ES2 e.g., https://flowcharts.llnl.gov/commodities/carbon)*
- *Making greater use of the Kaya Identity, ex-post accounting categories that are useful for quantitatively decomposing trends.*

**Response: Chapter 3 now has both Sankey diagrams for the U.S. energy and CO2e emissions with text to explain the major elements and flows and a Kaya $_{\text{illustration4}}$ for the region of North America to help synthesize information. There is also a more detailed description of Kaya formula, what it represents and results of Kaya decomposition studies for the U.S. Therefore chapter 3 responses have attempted to address all these concerns**

*The second overall comment was that there was no aspirational direction to the chapter, meaning that it included nothing on mitigation, the Paris Agreement or the costs of the large-scale energy transition that will be required in the future. NAS recommended:*

- *Including a wider variety of energy scenario types for the North American energy system ● Including a quantitative discussion of the carbon management impacts.*
- *Including a discussion of the UN Framework Convention on Climate Change and Paris Agreement*
- *Including a discussion of the economic dimensions of the energy sector's role in the carbon cycle in terms of potential for mitigation sequestration and other sinks*
- *Including the research needs for the quantification of mitigation potential and its cost-effectiveness. There are also important research needs concerning technological, economic and behavioral potentials. How to achieve the largescale energy transition poses many new research questions.*

**Response: Chapter 3 now has a goal of providing mitigation costs to achieve a 2° C trajectory. These costs were obtained through reviews of a wide variety of energy scenario types for North America including backcasting efforts. We also include a quantitative (cost) of carbon decisions if no actions are taken for a wide variety of sectors in the U.S. In the Carbon Management section we (re-)introduce and describe the commitments of the three economies to the Paris Agreement. The final section of the chapter has been re-written to include a wider set of research questions that are associated with the extensive energy transition required to meet the 2° C trajectory. Therefore, chapter three responses have attempted to address all these concerns.**

*The third overall comment was that the major key findings were not clear and did not represent the adequately the messages from the chapter. They recommended:*

- *Re-write the key findings as suggested below:* o *Since 17XX the North American (or U.S.) energy system has emitted XXX petagrams of fossil C into the atmosphere. Of that amount, YYY petagrams remain increasing the global concentration of CO2 in the atmosphere by ZZ parts per million. This represents W% of the total increase in atmospheric CO2 since 17XX.*
  - o *The North American (or U.S.) energy system emitted x petagrams of fossil C in 2016(?). Of that amount, y petagrams will remain in the atmosphere through 2zzz, (account for the fate of the rest).*

**Response: Chapter 3 authors are appreciative of the NAS attempts to re-write the Key Findings. The examples for the first two suggested key findings include results of emissions and the lifetimes of $CO_2$e components in the atmosphere since the eighteenth century. This "Key finding" is surprising for a number of reasons. First, the chapter does not review the emissions levels from energy use going back this far. Our remit is since 2004! For a review of the longer time span, please see the previous SOCCR1 report, chapter 3, pp. 30-31, and chapter 4, pp. 57-58 (both chapter are now cited in the text to refer readers). Including a similar review in this chapter may be redundant to this work. The chapter currently does provide a very brief overview of historical emissions trends in comparison to the ROW in section 3.5, however. Second, there is no mention of energy use trends in the suggested key findings. Given that this is an energy chapter, we argue for the need of a key finding in this regard and it should be up front. Third, in the current chapter, there is no discussion or review of accounting for atmospheric lifetimes or turnover rates of the different GHG compounds, as would be**

**required by the suggested key findings. We also are not convinced the presentation and discussion of the issues belongs in the energy chapter anyway. That is, these estimations and issues are not specific to energy systems, although of course they are related. This information is discussed, however, in Chapter 8. We have now directed the reader to that chapter for this information (in our current Section 3.5).**

**We have changed several of the other key findings, however., we keep our original first two findings. The first key finding is about the current (2013) state of energy use and GHG emissions from the North American energy system. The second key finding concerns trends in energy related GHG emissions since the first SOCCR. The new third finding attempts to summarize the Kaya discussion, as suggested by the NAS review. We present the underpinning reasons for changes in energy use and emissions levels based upon the review of this information. The new fourth and final key finding relates to the results of a review of energy scenarios to 2040/2050.**

**II. Line specific comments**

*Throughout the document: 1) reduce significant digits in the presented values and 2) Include a mention that $N_2O$ emissions are part of the energy systems GHG emissions*

**Response: The values for energy use and $CO_2$e emission are now presented in 2 or 3 significant digits throughout chapter 3 and there is now a sidebar on the definition of $CO_2$e which includes the $N_2O$ emissions (as well as other types of emissions) in fossil fuel energy related emissions, although this sidebar has been placed in the Preface as it is applicable to the entire volume. We refer to this sidebar when necessary.**

*P110, Line 152*
*The chapter contains a wealth of statistics, which makes it difficult to see the forest for the trees.*
*Perhaps a figure illustrating the role of energy systems in sources (mostly) and sinks (to a much lesser degree) would be helpful. It could be accompanied by a discussion of how energy systems fit into the overall framework of the carbon cycle. In a similar vein, the chapter introduces the Kaya Identity as an organizing concept but then doesn't use it, either as an organizing concept for presenting status and trends or analyzing them. More specific recommendations are made separately.*

**Response: The illustration of a carbon balance illustration for North American and global terrestrial (including fossil fuel sources), oceanic and atmospheric systems is needed for the volume. It may not best be placed in the energy system chapter, however, as it summarizes a great many details in the volume. We have suggested that this illustration be included in the Preface.**

**Chapter 3 uses the Kaya Identity elements as a framework for presenting the drivers of change to the energy system. Given comments by the NAS, we have strengthened the discussion of Kaya formula meaning and provided more empirical analyses from Kaya decomposition studies (but only for the U.S.), as requested by the reviewers.**

P110, Line 152
*The treatment of mitigation is unsatisfying for several reasons:*
- *impacts, past and potential future, of management actions are not quantified;*
- *the critical role of increased energy efficiency in all sectors is given inadequate attention, especially since there have been and are important initiatives in place in North America; - the challenge of transforming the energy system to a low carbon system (energy transition) is also not adequately discussed and analyzed;*
- *none of the projections of future energy use come close to achieving climate stabilization goals, a critical issue for the future of the energy system.*

*If North America seriously attempts to mitigate climate change, the energy system and its role in the carbon cycle will change profoundly. This should be a key topic of the chapter, but it is not.*

**Response: Chapter 3 now includes a discussion of the potential mitigation costs to reduce emissions to a 2° C trajectory for the U.S. This was done through a review of backcasting scenario exercises and Kaya decomposition studies. Note that now the goals of the assessment include "quantitative indicators of energy use and carbon dioxide equivalent ($CO_2$e) emissions from different energy system components and quantitative and qualitative analysis of the changes in system dynamics, particularly since 2003 and the technologies and costs to put the system on a 2° C trajectory." The latter goal was avoided previously.**

P111
*Add footnote on energy units.*

**Response: Chapter 3 already has a footnote on energy units (#14 in the FOD).**

P111, Line 10
*What is land-based carbon?*

**Response: This sentence and key finding has been removed.**

P112, Line 9-29
*The historical context discussion reads like it's all about recessions. Recessions have been important but so have energy prices and energy efficiency regulations. Attributing changes in carbon emissions via the Kaya identity would show this. Specific recommendations on sources of decomposition analyses of trends in carbon emissions from energy are suggested separately.*

**Response: The historical context was meant to emphasize rapid decreases in $CO_2$ emissions from ff use in the energy system, which happened to be associated with recessions. We focus on this because of the recent drop in emissions and the initial "trigger," the GFC. The previous SOCCR provided a broader more historical analysis (see SOCCR1 chapter 3, "The North American Carbon Budget Past and Present"), which was an excellent overview as**

**requested by the reviewer. Rather than repeat that work, we focused on what was new and most relevant including any historical precedents. We have clarified why we focus on this theme and have pointed the reader to the previous study for other types of information.**

*P113, Line 10 – P114, Line 36*
*The focus on proved reserves gives a misleading impression of the potential for future carbon emissions from combustion of fossil fuels, especially for petroleum. As the box on the subject acknowledges, proved reserves are a very conservative measure of potential future resources. There is a large literature on this subject that could be summarized as follows: Proved reserves are mainly a stock that energy entities maintain to insure adequate production in the near future. At a global scale, for example, proved oil reserves relative to current production have changed very little over decades. Resources have various definitions, but as a very broad generalization, technological advances have consistently overcome depletion of fossil fuel resources. This is likely to continue. Why is this important? Utilizing resources beyond proved reserves holds enormous potential for increasing the carbon concentrations in the atmosphere.*

**Response: We have incorporated the recommended text in the chapter in this section.**

*P114, Line 21-27*
*Renewable generating capacity is mentioned but not renewable resource estimates. This might be a useful addition with relevance to how the carbon cycle might be changed.*

**Response: We discuss the change in renewable resources in the region in Section 3.4.3 on increasing renewable energy and Section 3.6.4 on decreasing carbon intensity, as the increase in renewables is an important shift in the region's energy system. To respond to this comment the chapter now also directs the reader those sections from the paragraph mentioned.**

*P116, Line 19-22*
*The residential and commercial emissions of CO2 do not seem to include emissions from electricity generation, or at least they are not consistent with the EIA's data, which indicate 1.1 Pg for residential and 0.9 Pg for commercial in 2013 [https://www.eia.gov/environment/data.php#summary].*

**Response: Chapter 3 mentions that "Buildings dominate electricity and natural gas consumption," and discusses these sources for these sectors in detail. In the paragraph mentioned by the reviewer, which comes directly after the one mentioned above, the figures remove electricity energy and emissions from the sectors. This is done because we have another section (the previous one) on electricity and want to avoid any instance of "double-counting." Therefore, the reviewer is correct, the numbers do not match with those of the EIA's data!**

**To address this issue, the chapter has made specific mention of why the building sector's energy and $CO_2$e values do not include electricity data and related emissions. Importantly, leaving it this way is consistent with another of the reviewer requests, which is to include a Sankey diagrams from LL, as the figures on those Sankeys also do not include electricity related emissions for these sectors and can be cross-referenced!**

*P118, Line 2*
*This opening sentence is one of many examples of a "topic sentence" that doesn't really convey the main point of the section.*

**Response: Thank you for your comment. We have reviewed the topic sentences and have found them directly applicable to the paragraph they are associated with.Each subsection is symmetrical in the provision of information. That is, they all follow a similar format. We think some of the overall sections lack are introductions explaining the structure of the subsections, accordingly, Chapter 3 sections have been re-written with introductions that provide full overviews of the main points when appropriate and directs the reader as to what information can be expected in each sub-section.**

*P119, Line 7*
*"As demonstrated,…" is arguable and unneeded.*

**Response: Removed.**

*P119, Line 11-20*
*Why does this section not mention technologies identified by the EPA/DOT rulemaking for increasing light-duty vehicle fuel economy through 2025, and medium and heavy-duty fuel economy as well? This is all thoroughly documented in the rulemaking and supporting documents. And what about other transportation modes?*

**Response: This section, as defined, is, "A description of the current state of the North America energy system… [and] first identifies the size of the system, in terms of population and economy, energy resources, and primary energy supply. End-use sectors of electricity, buildings, industry, and transportation are then discussed and their regional contributions to the carbon cycle evaluated. Technologies for increasing efficiencies and lowering emission levels are briefly described for each sector." Each sector in the section are written with this information. The paragraph identified by the reviewer concerns technologies for increasing efficiencies and lowering emissions levels in the transportation sector. It does not include a discussion of regulations as none of the sub-sections in this section do. Carbon management, governance and rulemaking appear in later parts of the chapter. That is the reason why this specific rulemaking sentence is not mentioned in this section.**

**More specifically, Chapter 3 includes discussions of Corporate Average Fuel Economy regulations in other sections such as Section 3.6.3 Reduced Energy Intensity, and in Section 3.7.2 National energy and carbon management decisions. We have now up-dated the latter sub-section to include the new rulemaking decisions. We also refer the reader to this subsection.**

*P123, Line 16-30*
*Isn't the right way to present information on the carbon cycle role of biofuels to quantify the emissions from biofuel combustion as a source and the production as a source and sink?*

**Response:** As pointed out in Section 3.4.5 New Understanding of biofuel and natural gas contributions to the carbon cycle dynamics, however, this accounting if far from clear (see Farrell, et al, 2006, Khanna et al, 2012).

*P124, Line 34-35*
*"more than an estimated 18.6 million" is confusing unless the intention is that 18.6 is an absolute lower bound.*

**Response: Line removed.**

*P124, Line 31 – P126, Line 9*
*Shouldn't this section be attempting to quantify the sources, sinks and flows of carbon in the biofuel system, including uncertainty bounds?*

**Response: As mentioned above, the point of this section is that we have only recently developed the conceptual tools for such as a review. The research to provide these estimates is important. Such as task, however, is currently beyond the scope of this review. We have included a sidebar that notes the different calculations and the potential differences between current official numbers and what has been identified in the research. We argue that the type of response the reviewer is requesting is not a review, but a fairly significant research project in-and-of-itself. It is therefore outside the scope of this work but will be important for the next SOCCR.**

*P126, Line 11*
*What is a "feedback mechanism scenario?"*

**Response: Removed the word "scenarios."**

*P127, Line 6*
*Please cite the projection referenced.*

**Response: These projection came from a report by Navigant research (see http://www.greencarcongress.com/2013/09/20130910-navigant.html).**

**We have, however, changed this statement to:**

**The DOE (2018) projects combined sales of new electric, plug-in hybrid electric, and hybrid vehicles grow in market share from 4% in 2017 to 19% in 2050 in the Reference case translating into a vehicle fleet of over 2 million at the end of the period.**

> **The new reference is: DOE (2018) Annual Energy Outlook, 2018 with projections to 2050, Washington, DC: U.S. Energy Information Administration, Retrieved from: https://www.eia.gov/outlooks/aeo/pdf/AEO2018.pdf**

*P128, Line 1*
*The Kaya equation can also include sectoral structure (summing over sectors). Why not add that? It is disappointing that the authors do not use the identity or cite the work of others using the identity to decompose trends into components.*

**Response: Chapter 3 now has a larger discussion of the Kaya Identity and its role as identifying the influences on GHG emissions. The chapter also reviews a number of studies that focus on the decomposition of the Kaya Identity and includes results of such efforts for the U.S., as requested.**

**The chapter mentioned the possibility of including sectoral structural change in the formula, but as it is not a focus of the discussion, it is not emphasized. When possible, we mention research results that have examined these dynamics, although they are restricted to studies of the US.**

*P130, Line 31*
*Total vehicle miles traveled in the U.S. has increased every year since 2011 according to the Federal Highway Administration (FHWA1).2 We are not able to match the other data in this paragraph (e.g., fuel economy/energy intensity) to the FHWA data either.*

Table VM-1: https://www.fhwa.dot.gov/policyinformation/statistics/2015/vm1.cfm.

**Response: We have re-written this paragraph to:**
**"The on-road transportation sector in particular has seen reductions in fuel use total average passenger per capita vehicle km traveled, as well as reductions in emissions of $CO_2$e. According to the U.S. DOT (2016), from 2005 to 2015, total average km traveled per passenger vehicle dropped from approximately 20,100 to 18,200 and total average fuel use per vehicle dropped from around 2,100 liters to 1,800 liters. As a result, total average km per liter of fuel consumed increased from 9.4 to 10.1. These efficiencies have been influenced by changes in the vehicle weight and power and by corporate average fuel economy (CAFE) standards. For example, according to the US. DOT (2014) CAFE fuel standards have increased from 11.7 km per liter in 2010 to 14.5 km per liter in 2014 (based upon projected required average fuel economy standard values and model year reports). As a result of these changes, in 2015, in the U. S., vehicle travel was 4% higher than that of 2007, but $CO_2$e emissions for transportation were 1.73 Pg $CO_2$e (472 Tg C) in 2015 compared to 1.89 Pg $CO_2$e (515 Tg C) in 2007 (U.S. EPA 2016)."**

*P131, Line 14*
*This section deals with carbon intensity and refers to F/E (amount of CO2 emitted per unit of energy produced). The authors do not provide U.S. data for carbon intensity of transportation, however. This will depend on the carbon intensity assigned to ethanol, which is controversial/uncertain.*

**Response: This statement is true**

*P132, L 32 - P133, Line 8*
*Should CCS be considered a carbon sink in the framework of the carbon cycle or a determinant of carbon intensity (as is done here)?*

**<u>Response:</u> Removed this material from this sub-section and created a new sub-section entitled, "Carbon sink technologies"**

*P136, L 36 – P137, Line 1*
*The rulemaking documents provide reasonable estimates of the carbon source reductions that would be achieved by GHG emission regulations under the Clean Air Act.*

**<u>Response:</u> Added the sentence, "The new standards are estimated to lead to corresponding reductions in $CO_2$ emissions totaling 491 Tg C during the lives of light duty vehicles sold in model years 2017– 2025" with citation to the Federal Register.**

*P137, L19 – P138, Line 11*
*California's comprehensive GHG reduction plan and legislation deserves mention here along with other states that have such plans (less comprehensive and potent, in my opinion). California has a cap and trade system, Zero Emission Vehicle and Low Carbon Fuel standards, among a suite of comprehensive policies.*

**<u>Response:</u> We had mentioned the California plan, but now have added more text to describe and emphasize it.**

*P138, Line 12 – P140, Line 18*
*None of the scenarios discussed correspond to a serious attempt to reduce GHG emissions to levels that would stabilize global warming at target levels proposed by climate scientists (e.g., 2°C, 2.5°C, etc.). All are variations on business as usual. In a report of this nature at least one serious mitigation scenario should be included, as such scenarios do exist.*

**<u>Response:</u> Chapter 3 now includes a review of three different types of energy scenarios for North America and the Globe including backcasting (aspirational) efforts for 2° C.**

*P141, Line 6-12*
*The list in this paragraph has to include increasing energy efficiency. And in addition to decreasing the use of carbon-intensive fuels it should include a transition to low-carbon energy sources.*

**<u>Response:</u> This list has been changed to reflect this comment.**

*P142, Line 1 – P143, Line 5*
*It is good that this sidebar acknowledges the different definitions of resources and reserves. However, the discussion in the text focuses almost exclusively on proved reserves, which is a less relevant measure than the other discussed in the sidebar.*

**<u>Response:</u> We included the statement provided by the reviewer to places the large amount of proved reserves in context future carbon concentrations in the atmosphere if utilized.**

*P144, Line 18-19*
*The 3% number is relevant for electricity production but because the CO2 reduction for $CH_4$ use in transportation vehicles is only about 15-20%, only about a 1% leakage rate will eliminate GHG benefits.*

**<u>Response:</u> We have included this in a new sentence as: If $CH_4$ losses are larger than about 1-1.5% for vehicles and 3% of coal power electricity production, the immediate climate impact of natural gas combustion could equal or exceed that of coal (Alvarez et al., 2012; Myhre et al., 2013; Camuzeaux et al 2015).**

*P145, Line 32-33 See*
*previous comment*.

**<u>Response:</u> See above.**

*P151, Line 30-33 (also on P111).*
*It seems odd to rate a finding that net carbon effects may be positive, negative or neutral as having "high confidence". The authors are perhaps saying they are certain that we don't know the net effect for biofuels as a whole. Likewise, with respect to $CH_4$ as a fuel (overwhelmingly of fossil origin at the present) we know for certain that fugitive emissions reduce the overall carbon benefits. Considering the report recognizes that biofuels vary in their carbon impacts, we suggest rephrasing this finding or present it in a different way.*

**<u>Response:</u> This has been removed.**

Chapter 4: Urban

**Overview/Main Issues**

This chapter provides a comprehensive overview of research focused on carbon budget associated with urban areas. It summarizes active research aimed to quantify spatial and temporal variability in carbon emissions at fine scales needed to understand the drivers of those emissions and document efficacy of management strategies. It provides a very good discussion of governance and management at the urban scale that influence carbon emissions, and it distinguishes what is controllable from what is uncontrollable due to decisions being made at larger state to national scale or the long turnover time of built infrastructure.

This chapter, which follows the Energy chapter that focuses on energy **production,** would be strengthened by including a brief discussion of energy **consumption** patterns in North America and by sharpening the introductory section so that the rationale for singling out urban areas is highlighted.

Introductory paragraph has been edited to emphasize the rationale for urban focus and contrasting the consumptive focus.

Somewhere in SOCCR2, there needs to be a discussion about carbon management choices at state and national levels that mirrors the excellent section in the urban chapter. Instead of leaving off at stating there are factors that can't be controlled at urban level, provide some discussion elsewhere on what the options are and what is being done to better understand them.

Agreed and this message has been passed on to other chapters. In particular, there are changes to chapter 11 and the executive summery has material on this.

The urban chapter stresses the observation that urban emissions contribute disproportionately to anthropogenic emissions of carbon relative to their land area, which is true but not as illuminating as starting from the observation that urban areas concentrate population and economic activity that are responsible for carbon emissions. This point isn't made until p.175, Line 26. It would make an effective starting point for the introduction.

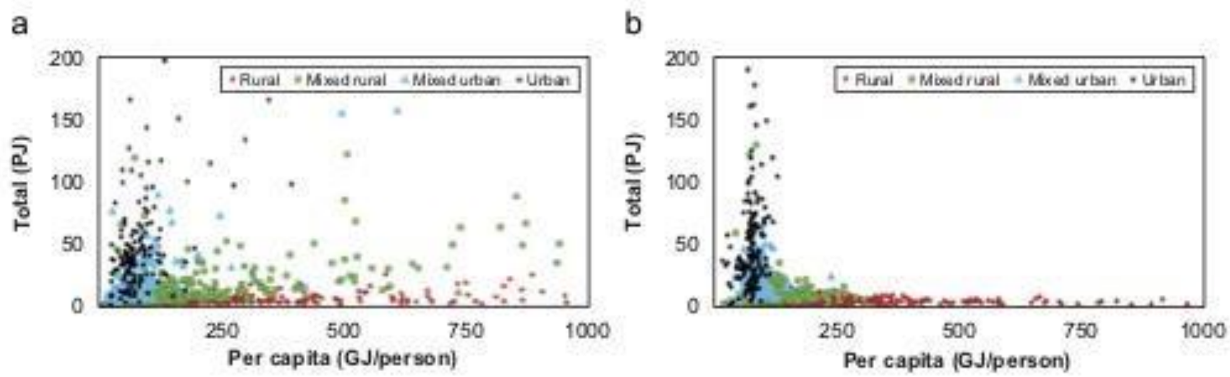
That sentence has been moved and the opening paragraph rewritten somewhat.

Follow up by noting how urban emissions diverge from a constant per capita value. It is precisely the divergence from constant per capita emissions that make urban emissions especially interesting and provides the reason to study them as a separate entity.

We agree that it is one of the reasons that make urban emission interesting to study (there are many). We have included the only citation we are aware of that makes this point beyond single- (or few-) city analysis.

As the text points out (at the end of the 3rd paragraph in the introduction) there is a need "to explore how urban infrastructure and urban morphology will influence current and future energy consumption and development." A figure could be added here to show the range of per capita carbon emissions for different cities or as a function of population density. Because there are emission differences, there are opportunities to influence them and a research need for understanding what causes those differences.

We are aware of only one study that attempted to comprehensively show relationships of per capita quantities across cities (Parshall et al., 2010). The figure of note in this study is of per capita direct energy consumption, a reasonable proxy for energyrelated $CO_2$ emissions. It does show a contrast between urban and rural and shows the differing levels of variance between the two. This is now Figure 4.2




The chapter is right to point out that some of the differences in emission strength between urban areas decrease when the indirect emissions (energy, goods, and services consumed in the urban area that were produced and counted as emissions elsewhere) are considered. Discussions about emission intensity correlating with various factors need to caution against basing mechanistic understanding on correlations and trends alone.

**Agreed.**

The chapter makes an important point that urban structure and infrastructure investments influence carbon emissions and that governance structures that operate at the urban level are either not present or very different at state and national levels. This point needs to be noted early in the text and given a strong emphasis.

**Sentence added to the last introduction section paragraph to emphasize this point.**

Wherever possible the discussion and estimates of what has been and might be accomplished in the future in terms of managing the carbon cycle at the urban level should be more quantitative.

**We have attempted to include as much quantitative information as possible – the challenge remains the limitation in the literature and the distinction between single-city studies and more comprehensive treatment of the urban carbon cycle. While there is quantitative information for single-city studies, there is very limited quantification across cities. It is the latter that is needed to avoid anecdotal conclusions.**

Also of interest but not discussed quantitatively are the time constants for changes to be effected. The notion of turnover times for infrastructure is alluded to by the phrase "infrastructure lock-in," but this could be expanded in a more quantitative way by noting typical lifetimes for different classes of infrastructure. Infrastructure isn't necessarily locked, but it's difficult to replace before the end of its designed life. Past experience provides some guidance on issues like how quickly the vehicle fleet is updated compared to time constant for appliances, housing stock, transportation networks, and energy delivery. This is not to suggest that the concept of infrastructure lifetime needs to be exhaustively reviewed in the chapter. It needs to be highlighted as critical factor with a rich history to guide our understanding of how quickly emission changes can be accomplished.

**We have expanded the explanation in section 4.3.5, alluded to it in terms of behavior in section 4.3.3 and included further elaboration/clarification in key finding 2. We have also added a Figure to better demonstrate the specific aspect of lock-in related to technology (not the only aspect). Further details on our response to this comment can be found below in response to the comment on key finding 2.**

The urban chapter has a very thorough section documenting Societal Drivers, which seems to provide the specific examples of carbon being embedded in societal activities that is the key finding for chapter 6. Is there sufficient cross reference between these chapters?

We have included psecific callouts to other chapters where this issue arises so hopefully the reader will be able to quickly capture the different places in the report where this scale interaction comes up. There is also material in the executive summery attempting to cover this important point.

**Statement of Task Questions**

- *Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?*

Although the chapter does meet several goals that can be identified by reading through the text, the goals and objectives are not articulated clearly enough and the intended audience is not specified. Having a summary of the objectives and audience in the introduction would guide the reader and provide a focus to sharpen the remainder of chapter.

We have tried to better indicate the goals of the chapter in the revised 1st paragraph.

- *Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?*

The chapter covers scientific literature up to the onset of writing and chapter review. However, there are a few recent papers that could be incorporated to illustrate some key new results. Notably, the emerging field of using satellite observations to quantify emissions from large urban regions is not given enough attention. Hakkarainen et al. (2016) demonstrate the ability to quantify $CO_2$ emission hotspots from satellite observations and provide a very good illustration of how anthropogenic carbon emissions are concentrated into urban areas.

The Hakkarainen et al paper demonstrates that remotely-sensed $CO_2$ can identify regions of anthropogenic $CO_2$ emissions. It is worth noting that the spatial characteristics of anthropogenic emissions have long been understood from a variety of ground-based observations including basic fuel statistics and infrastructure inventories. However, this capability has been noted and the Hakkarainen et al. reference included on page 177, lines 13-14.

- *Are the findings documented in a consistent, transparent and credible way?*

Yes, the chapter provides excellent documentation of its key findings.

- *Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?*

The graphics have room for improvement. Neither of the two figures that are in the chapter are quantitative, and they don't effectively provide information beyond what is in the text. At the very least, it would help to have a graphic artist review them with an eye toward making them more effective in the web-published version of the report.

It would help to add a figure in the introduction illustrating the sharply focused carbon emissions from urban areas, based on leftmost panel of Figure 1 in Hakkarainen et al. (2016) that shows $CO_2$ hotspots together with $NO_2$ concentrations and emission inventory. The point that urban emissions do not follow a constant per capita ratio could be effectively presented by figures adapted from recent reports that show emission per density, or that have aggregated different urban areas and ranked them by emission.

Have added a figure that shows US emissions highlighting the urban landscape (now Figure 4.1). This figure is from recent bottom-up quantification and seems a better representation than that found in Hakkarainen which only claims a rough correspondence to regional anthropogenic emissions.

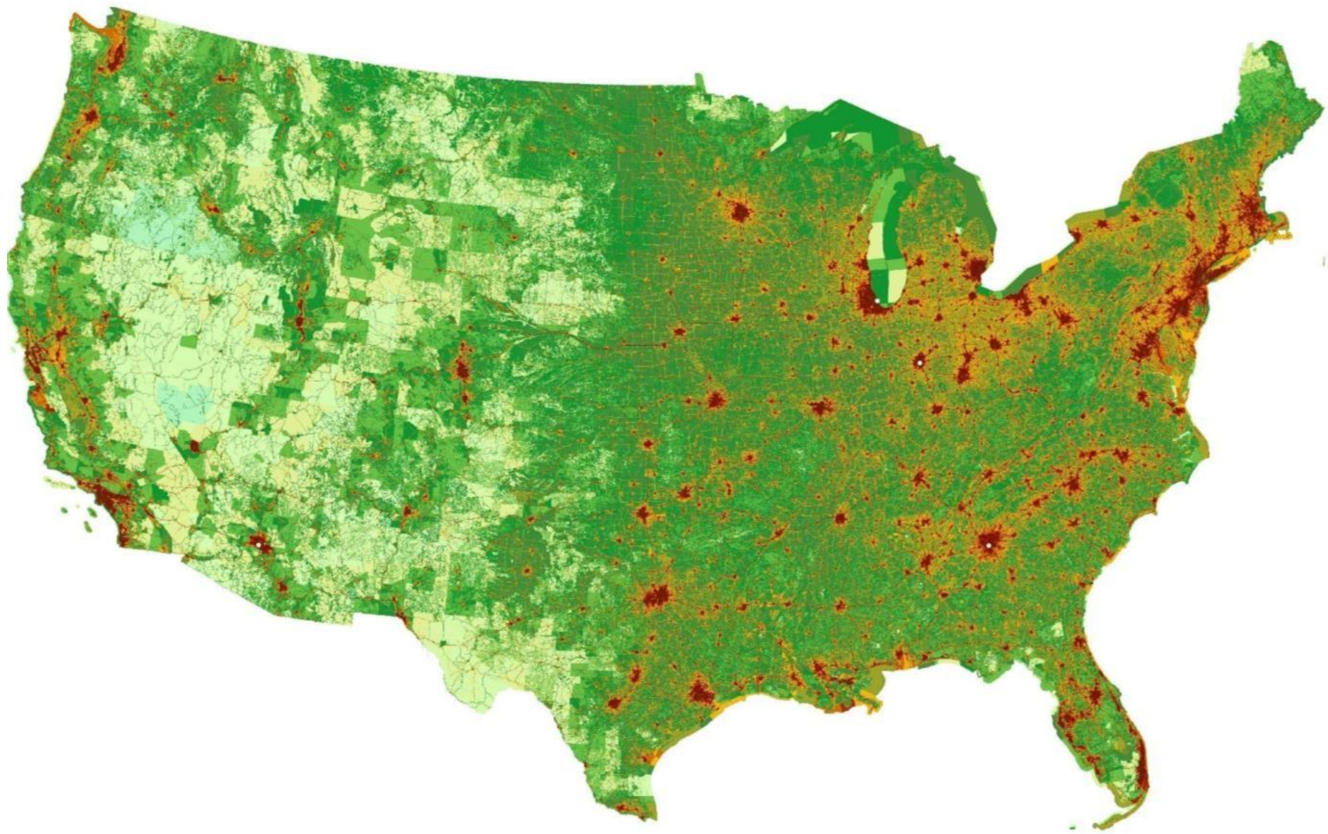


Figure 4.2 doesn't clearly show the relationships described in the text that refers to this figure. Need to clarify the point that figure is supposed to illustrate and revise accordingly. If all the icons represent GHG emissions it is confusing to include a wind turbine in the electricity generation. How does water fit into GHG emission?

The location of this figure reference in the text has been moved to more appropriate location where the link between the figure and main text is more logical.

Comments on specific Key Findings:

*Key Finding 1.* The confidence in this finding seems to be understated and could be "high confidence and very likely", rather than medium. The first sentence in of the introduction section clearly states that carbon fluxes resulting from urban activities account for 80% of the total North American anthropogenic $CO_2$ flux to the atmosphere. The key finding statement would be improved by making it more quantitative (e.g., replace "large proportion" with a number). Consider rephrasing the statement so the result and its reason are given together. Urban areas are a primary source of anthropogenic carbon emissions because humans and human activity are concentrated there. In addition, they are an indirect source of emissions embedded in goods and services consumed by urban dwellers.

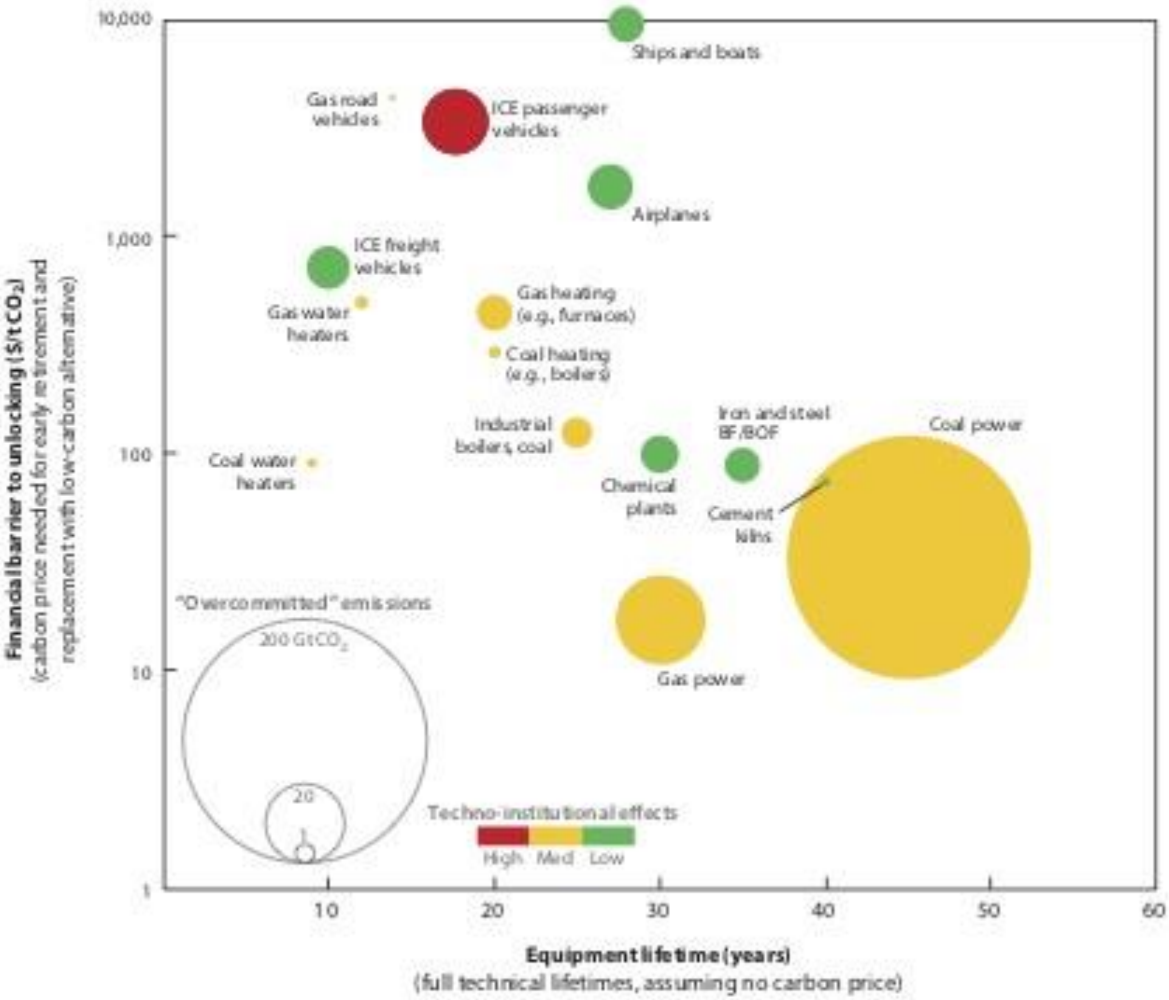
The confidence in this finding is not justified as "high confidence and very likely" given the range of estimates in the peer reviewed literature. There are only 3 sources that have attempted to quantify the contribution of urban areas to GHG emissions and only two of these isolated North America (Marcotullio et al. 2013; Grubler et al., 2012). They both contain considerable uncertainty and are additionally sensitive to the urban boundary applied. The IPCC 5th assessment report (WG3, chapter 12) concluded values for the global domain and said the following:

> There are very few studies that have examined the contribution of all urban areas to global GHG emissions. The fraction of global $CO_2$ emissions from urban areas depends on the spatial and functional boundary definitions of urban and the choice of emissions accounting method. Estimates for urban energy related $CO_2$ emissions range from 71 % for 2006 to between 53 % and 87 % (central estimate, 76 %) of $CO_2$ emissions from global final energy use (*medium evidence, medium agreement*). There is only one attempt in the literature that examines the total GHG ($CO_2$, $CH_4$, $N_2O$ and $SF_6$) contribution of urban areas globally, estimated at between 37 % and 49 % of global GHG emissions for the year 2000. Using Scope1 accounting, urban share of global $CO_2$ emissions is about 44 % (*limited evidence, medium agreement*).

Within the chapter 12 of the 5th assessment there is a chart that includes estimates for the North American domain, referring to two peer-reviewed studies. These are the two studies relied upon here to condition the language and the confidence assessment. Supporting evidence for the Key Finding 1 has been further elaborated to support our conclusions.

*Key Finding 2.* This would be a logical point to frame the issue in context of infrastructure turnover time. Urban infrastructure is built to last decades if not longer. Major changes are difficult if not impossible and expensive as well as an additional carbon emission associated with demolition and new construction. Infrastructure improvements could be evaluated in terms of payback time. The support for this finding presents a long list of citations, but it would help the reader to present a brief summary of the overall results that are common to those studies.

Infrastructure turnover times is one of three core elements of carbon lock-in – the other two eing institutional and behavioral. We have attempted to summarize this aspect of key finding 2 in both the main text, the key finding and the key finding support. We have included an additional figure that captures the turnover time/cost aspect of technological lock-in.




*Key Finding 5.* The statement could be phrased positively to focus on what has been learned instead of what we don't know. $CH_4$ emissions have been poorly characterized, but the combination of improved instrumentation, modeling tools, and heightened interest in the problem is defining the range of emission rates and highlighting infrastructure characteristics that affect $CH_4$ emission.

This key finding has been redrafted as: "Urban $CH_4$ emissions have been poorly characterized, but the combination of improved instrumentation, modeling tools, and heightened interest in the problem is defining the range of emission rates and source composition as well as highlighting infrastructure characteristics that affect $CH_4$ emissions. [High confidence]."

- *Are the research needs identified in the report appropriate?*

The research needs to have improved information on fluxes and their drivers, and improved understanding of successful mitigation (as identified in Section 4.7) are appropriate, but more details should be provided. For example, how is the urban carbon flux projection capability expected to improve in the future? How should the various approaches for estimating urban carbon fluxes be integrated and reconciled? Emerging technologies—for example, connected and automated vehicles—potentially will have a large impact on urban emissions and ought to be identified as a topic for future research. Future research needs another place where the use of satellite observations should be noted as an emerging approach.

Text has been added to section 4.7 that attempts to respond to these useful comments. Added detail on what our recommendations are for various knowledge gaps are included.

- *Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?*

Yes, though the chapter lacks a summary figure or table that presents a quantitative bottom line for carbon stocks, budgets, and transfers.
A summary figure is difficult on this topic in that there remains no peer-reviewed literature that fully encompasses urban carbon quantitatively.

- *What other significant improvements, if any, might be made in the document?*

Suggestions on ways to sharpen the introduction are noted earlier. Overall the chapter would be more useful if it included specific quantitative statistics on the magnitudes of carbon fluxes/stocks and their trends at the national and continental scale. Some additional figures would be very helpful. The first additional figure would show a map of emission fluxes. A second would rank total carbon flux or per capita flux for specific urban areas. The table of urban carbon budget studies might be accompanied by a table that summarized some quantitative results from those studies.

We have included two new figures as described above. As for inclusion of quantitative results, the plethora of definitions, accounting approaches, etc. make any form of comparative urban fluxes difficult to arrange… Indeed, there is no peer-reviewed literature with results as stated.

**Line-Specific Comments**

P175, Line 4
One should not aim to *improve* urban heat islands.

This must be intended as "Line 1" not "Line 4". Changed to "urban heat island reduction" to accommodate reviewer concern.

P175, Line 18
Better explanation of the respiration component is needed. In part, for top-down budgeting from atmospheric measurements, respiration needs to be estimated and separated from total urban $CO_2$ emission to get the fossil carbon component. If respiration is included in the emissions the carbon uptake in the full carbon budget needs to be counted as well.

It is not clear from the text whether respiration is being treated consistently with the agricultural products that are being consumed. Notably, the executive summary mentions that the biomass in agricultural systems isn't included in the budget (presumably because it has short lifetime, though this is not explicitly stated).

Whether or not one considers the fluxes that have opposing sign outside of an urban domain goes to the heart of the accounting framework. If one is linking fluxes to atmospheric monitoring (reflecting the urban domain), gross fluxes such as human respiration and waste decomposition must be included. The net value is appropriate if one is accounting for urban fluxes in a larger sense of atmospheric responsibility. In the introductory paragraph, these are mentioned to outline the potential complexity of fluxes that comprise the urban carbon cycle. Later in the chapter we attempt to clarify under what circumstances these various fluxes may or may not be included in an assessment of flux.

P174, Line 28-32
An observation pertaining to the structure of the report overall: energy efficiency is clearly a major driver of urban carbon emissions. However, as the chapter points out in several places, urban governments have some leverage over energy efficiency (and the carbon intensity of energy), but it is limited. Probably the majority of the capacity to change energy efficiency and carbon intensity in urban areas belongs to federal and state governments. The report does not have a logical place to discuss these carbon management capabilities and policies, unless it is chapter 3. As a consequence, energy efficiency improvement and transition to low-carbon energy sources are generally under-represented in this report. From our perspective, energy efficiency is not so much a "driver" of emissions but a mitigation option to make them change. Nevertheless, the broader point is important, and we have tried to include some references to other chapters where this is taken up as a topic in addition to our urban chapter since it is broader than just "urban."

P175, Line 26-27
Punctuation, Change ";" to ",".

It is not clear what the intention is here. If this means to replace "energy, goods" to be "energy to goods" we would respectfully decline the recommendation. There are goods that have embedded carbon (such as food, fiber, etc.) whose fluxes are not related to the energy used in production.

P179, Line 7-8
Has any estimate been reported for Mexico?

We are not aware of an estimate for Mexico.

P180, Line 34-41
Why so many examples for UK? It is better to use examples of North America.

The literature is limited, and the co-author of this section indicated that there is no equivalent to the insight provided by the UK referenced literature.

P180, Line 21
Extra "," before the citation should be removed.

This has been done.

P180, Line 29 – P181, Line 6
In the behavior section it could be noted that residents in different cities or geographical regions also have different lawn management practices (e.g., fertilization, watering).

Could borrow from the bio section and/or get authors to add content here.

P182, Line 23 – 30
This section could just be labeled "Climate." The examples in the text cover large-scale climate as much as local climate moderated by heat island.

Agreed, change made.

P182, Line 35-36
A source is needed for the national growth rate numbers.

Agreed, reference has been added.

P182, Line 31-36
This section is mainly qualitative, and more quantitative trend analyses are needed.

The lack of quantitative information reflects the state of knowledge.

P184, Line 19
Presumably this sentence means positively correlated; that should be stated explicitly, or phrased as consumption increases with area per person, or is it more informative to state that consumption is inversely proportional to urban density?

Changed to "inversely proportional to urban population density."

P186, Line 3- P188, Line 26
The ability to discuss energy efficiency improvement and low-C energy is limited by constraining the scope to the governance capacities of urban areas. As noted on P186, lines 34-37, mitigation is strongly affected by vehicle energy efficiency, but this has been addressed at the national level almost exclusively (efforts of the CARB, and of cities, states or provinces to enact feebates would be an exception). Vehicles with lower emissions due to energy efficiency improvement can accomplish as much $CO_2$ mitigation as transition to low carbon energy (see, e.g., NRC, 2013).

We have included references to other chapters where some of these topics are taken up in the broader sense since there are not uniquely urban.

P190, Line 30-32
This statement shows that the level of confidence for Key Finding 1 (Lines 3-6 on this page) should be high confidence and very likely rather than medium confidence and likely.

This has been responded to previously.

P190, Line 27
"TBD" is not really acceptable at this stage.

This has been deleted as it is not relevant for this chapter

P190, Line 3-6
Give quantitative estimates.

Response can be found under Key Finding 1 comment and response.

P190, Line 27
Says "TBD." Was there something to fill in for the likelihood of impact, or is that component not appropriate for this finding?

Not appropriate.

P191, Line 27-30
The challenge could be expanded to include the actual data collection. It is not just analysis and uncertainty quantification of multiple carbon flux approaches that is challenging. Designing and executing urban flux studies is far from routine.

Language changed to answer concern.

P212, Figure 4.2.
In this chapter it wasn't exactly clear how agricultural products are treated. Agriculture is noted explicitly as a city process but doesn't clearly show up as an upstream process. The accounting could all be correct, it is just that the text is not always clearly indicating that agricultural biomass is being treated consistently across all sectors.

There is no singular consistency as it must be defined within the context of the accounting system being used. Language has been added at the beginning of the report that should clarify this.

| **Figure/ Table Number** | **Start Page** | **End Page** | **Start Line** | **End Line** | **Comment** | **Response** | |
|---|---|---|---|---|---|---|---|
| | | | | | There are a few statements in this chapter that are not entirely consistent with common understanding of carbon cycling in agro-ecosystems and may be misleading. For example, the authors claim that leaving land fallow tends to lead to carbon losses (p.218, line 15). Relative to land under intensive tillage, fallow land (particularly if occupied by perennial vegetation) tends to build SOC. If the authors disagree, they should strengthen their case with proper references. | Insert the "without vegetation' following “fallow” to clarify for readers. Often when ag systems are fallowed - it is from water conservation and the land is kept vegetation free with tillage or pesticide. Fallow in this context is not the same as land that was taken out of crop production and managed with perennials. | |
| | | | | | The authors also claim that crops are carbon neutral because after they are harvested, they grow again in the subsequent year (p.227, line 7). This is inconsistent with common understanding that intensive row-crop agriculture, by stimulating soil carbon mineralization, is a net source of carbon to the atmosphere. The authors should reconsider these statements or provide additional explanation. | The point is clarified within the paragraph that even though the biomass is regrown annually that it still impacts the global balance. Reworded to since the authors are pointing out how annual croplands C is considered. The paragraph even in the original format went on to point out that treating cropland as C neutral may not be an appropriate assumption. | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | | | | | Changes in soil carbon in agriculture is in part controlled by inputs from above- and belowground biomass. From the 1930s to today, there have been enormous increases in crop yields, with relatively small changes over this time period in the allocation of biomass within a crop between grain and aboveground biomass. Consequently, biomass inputs have gone up at same time that yields have increased, and both yield and biomass are projected to increase well into the future. High yields today already are producing more stover than can be assimilated into the soil. It would be useful if the authors gave some consideration of how projected increases in yield and biomass may affect SOC. | The reviewer has a valid point. Added a brief discussion under historical context, adding several citations. Also added a reference that suggests increasing temperatures will decrease crop yields. | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | | | | | A major driver of changes in SOC, particular in the rain fed Midwest U.S., is soil drainage. Expansive areas of the corn belt are underlayed by tile drains, and the installation of these drains, along with intensive tillage, were responsible for dramatic losses of SOC beginning in the 19th century. Some discussion of how the interaction between changing precipitation regimes and tile drainage affect SOC in the future would be interesting. | We currently acknowledge that drainage interacts with other management. I am not finding a good reference to substantiate the statement that for the rain fed Midwest. The inclusion of tile drainage, can increase yield and associated biomass inputs so it may not be a foregone conclusion that drainage causes a loss of SOC. Dramatic loss of SOC in Histosols. | We currently acknowledge that drainage interacts with other management. I am not finding a good reference to substantiate the statement that for the rain fed Midwest. The inclusion of tile drainage, can increase yield and associated biomass inputs so it may not be a foregone conclusion that drainage causes a loss of SOC. |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | | | | | While this chapter addresses “gaps” and “uncertainties,” the authors do not clearly articulate future research needs in this chapter. | | |
| | | | | | The discussion of the role of no-till agriculture (p.225) as a modulator of SOC does not seem well balanced. Measuring changes in SOC over relatively short periods of time-a decade or less-is problematic. Spatial variation often swamps temporal trends in SOC. The best indicator of how management/crop type/land use change effects SOC may be eddy covariance-a direct measurement of Net Ecosystem Exchange. One study of how cropping affects SOC (Bernacchi et al., 2005) demonstrated rather convincingly that no-till agriculture and clay-rich mollisols builds SOC. | The range of response based on EC studies added including the suggested Bernachhi et al., 2005. The evidence from EC support the conclusion that while no tillage CAN shift soils to a sink, it would be incorrect to assume that adopting no tillage practices WILL result in soil behaving as a sink. Jane, here too is the theme of questioning SOC with different management comment. | |
| | 214 | | 21 | 25 | Would be interested to learn more about temp effects on SOC | | |
| | 217 | | 3 | 17 | Include references from Bernacchi and Robertson | | |
| | 217 | | 17 | | Include references by Bernacchi | | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | 217 | | 21 | | What is forage productivity? | Forage productivity is simply forage production here. | |
| | 218 | | 1 | | Drainage has a major impact on SOC. | True drainage would impact by soil environment. It was added to the list, although it should be noted this is a list of examples it is not exhaustive. Here too Jane I'm not sure how best to address this and if we want to delegate to another co-author? | |
| | 218 | | 1 | 2 | Extent and efficiency of drainage? | | I think they maybe looking to have drainage added to the list of choice. I will see if I can find a find some references to address |
| | 218 | | 15 | 18 | How? Not sure this is believable. | I could not corroborate the statement in the USDA 2014, which I would have expected to find in Chapter three. Clause deleted. | check on this - other citations |
| | 223 | | | | More discussion of how expected increases in temperature will affect SOC would be useful. See a recent paper by Black et al. (2017). | | |
| | 224 | | 20 | 22 | Why add on for Canada and not Mexico? Scope? | We added information where data were available.<br>Unfortunately, there are fewer data available for Mexico. | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | 225 | | 13 | 14 | Two is not numerous | We give example references to orient the reader and keep text streamlined.Tthere are more available. | |
| | 225 | | 13 | | Delete “Numerous authors and models have reported that…” | We changed to "Numerous publications have reported that…" | |
| | 226 | | 1 | 20 | Very difficult to measure SOC changes directly; preferable is data from eddy covariance. (See Bernacchi et al., 2005) | We agree that measuring changes to SOC is a challenge, but so too is using eddy covariance because there could be C losses not captured in net atmospheric exchange (e.g., DOC leached from the system). This could mean that we would incorrectly calculate this C as stored when it was lost but not as a $CO_2$. A range of response based on EC studies added including the suggested Bernachhi et al., 2005. see response at line 15. Alex and Jane, this was my response, but I am not an expert in eddycovariance and I will ask an expert collaborator her opinion to make sure I'm getting this right. Alex-OK. | |
| | 226 | | 1 | 20 | The authors may wish to acknowledge that while perennial, cellulosic biofuel feedstocks still suffer from high costs of conversion, they have enormous potential to build SOC. Some of these crops increase SOC by as much as 1 Mg/yr after removal of aboveground biomass. | The benefits of perennial cellulosic biofuel still increase SOCwas addressed on P. 217 line 1-2 in section 5.3.1. | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | 226 | | 14 | 20 | There is enormous potential of perennial bioenergy crops to restore SOC and reduce $N_20$ | The benefits of perennial celluloisc biofuel still increase SOC was addressed on P. 217 lin 1-2 in section 5.3.1. Citation added in regards to $N_2O$, which increase when perennials are managed for feedstock. | |
| | 227 | | 7 | 8 | Not consistent w/effects of intensive agriculture on SOC. | The point is clarified within the paragraph that even though the biomass is regrown annually that it still impacts the global balance. Reworded to since the authors are pointing out how annual croplands C is considered. The paragraph even in the original format went on to point out that treating cropland as C neutral may not be an appropriate assumption. | Jane, here too is the theme of questioning SOC with different management comment. |
| | 227 | | 33 | 34 | Remove “such as nitrates” from the end of the sentence and place before “also.” | Changed as suggested. | |
| | 233 | | 37 | | Delete the word “managing.” | Changed as suggested. | |
| 5.1 | | | | | The axes are unclear— both left and right y-axes state million acres. Also, an additional y-axis labelled with hectares would be helpful, since the text uses hectares. | Alex, can you take this one? Sasha, I added a note to the figure caption about the axes (this addresses the 1st part of the comment). I looked but could not find the actual data that went into the graph (and don't know who included the graph on 1st place), so could not recreate it in ha. I think we should leave it as it is. I'll send you a hi-res version to send along to the editors to replace the current graph. | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 5.4 | | | | | The authors should be explicit in the legend that negative values represent a flux of carbon from the atmosphere to soil, and positive values represent the opposite. | Changed as suggested. | |

Chapter 6 Social Science Perspective

**Response to NAS comments, SOCCR-2 Chapter 6, Social Science Perspectives**

*Lack of Economics Research:* When the author team outlined the chapter, we chose to focus on social science research that starts with analyses of people in actual settings. Within the confines of 15 pages, we could not cover the wide and deep social scientific research that grows out of climate research, with its aggregated sectoral data and sparse descriptions of people (typically population, GDP/income/wealth, and possibly a technological factor). Although this kind of research (which includes much of economics research and integrated assessment) has been immensely valuable in scoping problems and outlining first-best and idealized policies and other activities, it does not account for the variation in social arrangements (governance, institutions, networks, social practices, technologies, and so on) that affect people's knowledge, attitudes, actions, and ability to act.

We also explicitly decided to *not* begin the chapter with a version of this explanation, which might well be taken as criticism of research we were not including, even though we would not intend such criticism.

It now appears that we made the wrong decision. We have completely rewritten the introduction to (1) state the goal of the chapter (using the NAS-suggested wording pulled from the last section), and (2) better explain the boundaries of what the chapter covers, specifically stating that most economic research will not be covered (except for behavioral economics and some market-based research).

*Need for more actual insights gained from social science research:* The state of this body of research necessarily includes statements about issues and process, but findings are included in many of the subsections. However, we have added other findings, e.g., the Stern et al. table showing potentials for emissions reductions from behavioral change; vulnerability results for the U.S., Canada, and Mexico; results from the cited social network analyses; and programs that have attempted collaborative interdisciplinary work. We have also added a list of insights/findings from those discussed in the chapter.

*Key Findings:* We agree with the comments and have completely redrafted the Key Findings, beginning with the suggested Key Finding from the NAS, somewhat revised. The new findings are better grounded in the chapter's findings.

*Comments about specific sections:* The specific comments on Section 6.2 (Energy and Embedded Carbon) have the form of lineby-line comments; responses have been added to the line-by-line responses. Section 6.4 (Scenarios) has been revised in accordance with the comments about the broader sources and uses for scenarios. Section 6.5 (Vulnerability) has been revised to make the distinction between research framed as starting with climate and the carbon cycle and research starting with social conditions. Section 6.7 (Sociological Transitions) has not been changed; technological histories of transitions to large technological systems, such as electricity (Hughes), railroads (Dobbin), nuclear power (numerous sources), and automobiles (Geels and others), clearly show the crucial importance of government policies. Hultman et al. (2012) interviewed energy experts in the U.S., Sweden, and Brazil to indicate important factors in the development of nuclear power and biofuels; a clear finding was the importance of government policy. War and a general decline in whaling seem to have been factors in the rising price of whale oil. We would not wish to use hand-picking/mechanization of cotton processing as an example, because of its strong association with the plantation system and slavery.

**CHAPTER 6—RESPONSES TO NAS COMMENTS**

P254, Line 30-31
Not clear why vulnerability research is an exception.
RESPONSE: The phrase has been deleted.

P255, Line 20
Vague. Be clear what specific theory is referred to.
RESPONSE: Edited to specify economic theory.

P255, Line 30
Define systems order policy.
RESPONSE: Edited to indicate policy that addresses systems (rather than narrowly addressing one or a few factors).

P255, Line 33-35
Statement inappropriately limited to energy sustainability arena.
.RESPONSE: Phrase has been deleted.

In Section 6.2 on Energy Behavior and Embedded Carbon:

Statements about the lack of study of the structure and evolution of energy demand (e.g., p.256, lines 2-4) are not correct RESPONSE: We have clarified this statement and given an example. The claim was about *relative* emphasis on device-level interactions versus the more structure issues. We have also excepted economics in the previous sentence.

The text largely ignores price/cost as one determinant of the behavior of "the people" that are the focus of the chapter and does not place the behavioral science in the context of decades of work on price and income elasticity (e.g., p.56, lines 23-34).

RESPONSE: We have added references to price and cost-effectiveness as explanatory factors, and have tried to better integrate economics into the flow of the chapter

The cited work in behavioral economics (e.g., p.257, lines 20-31) fails to make the distinction between studies based on experiments with small numbers of individuals versus empirical analysis of populations in actual market circumstances.
RESPONSE: We have added a reference, but do not agree that there is a need to distinguish small-n and large-n studies.

The discussion of rebound effects fails to put the discussion in the context of a history of empirical analysis of rebound behavior observed in particular markets.
RESPONSE: We have added a reference to Gillingham, Rapson, and Wagner 2016 but cannot go into the depth of the discussion in this chapter.

P256, Line 1-4
Statement is incorrect unless meant to exclude economics research. Please clarify. RESPONSE: Qualifying phrase has been dropped.

P256, Line 19-21
The statement should be qualified in that all of the costs are not intangible. RESPONSE: Revised.

P257, Line 16-19
It is not true that this research is defined by short-term policy objectives, or that it ignores the sources of energy use. RESPONSE: Clarified. Added a mention of the increasingly long-term viewpoint.

P257, Line 33
"Recognized need..." is imprecise. State by whom it is recognized, and based on what evidence? RESPONSE: Rewritten and reference added.

P258, Line 1 - P260, Line 28
The authors miss an opportunity to make the point that energy appears to be an area where markets do not function as predicted by rational economic behavior, so that much economics analysis has fallen far short of providing understanding and guidance for decision makers.
RESPONSE: Incorporated this point, drawing on Jaffe and Stavins.

P258, Line 17-19
Statement is incorrect and ignores analysis of price elasticity
RESPONSE: Revised statement.

P258, Line 24-28
Statement is empty unless provide alternative "labels" and explain what is meant by policy perspective and priorities. RESPONSE: Changed wording to clarify.

P258, Line 37
Not clear what is meant by "regulated energy efficiency industry". Provide examples to clarify.
RESPONSE: Dropped the word "regulatory"; please see reference for any discussion of 'energy efficiency industry' that is not evident from the statement.

P258, Line 31-32
There are no "traditional" definitions of efficiency, but different definitions depending on the context (engineering, economic; energy, labor, all factors).
RESPONSE: Changed the term from "efficiency" to "energy efficiency."

P259, Line 20
Again, what is the efficiency industry?
RESPONSE: See reference.

P260, Line 29
Multifaceted seems another example of jargon, without meaning in this context.
RESPONSE: Deleted word.

P262, Line 16-17
Why single out transportation? Industry? Commerce?
RESPONSE: Deleted the phrase.

P263, Line 12
What is meant by "sustainability" of the carbon cycle? The cycle is not threatened.
RESPONSE: Edited; the word "sustainability" has been deleted.

P263, Line 26

What alternative organizing force is imagined, to make this a question?
RESPONSE: Edited for explanatory clarity.

P263, Line 33-35
Not clear what is meant by “engagement with the normative dimensions . . .”
RESPONSE: Edited for clarity.

P264, Line 4-7
Potential confusion in the writing: the scenarios are not "tools" but the result of the application of tools.
RESPONSE: The perspective that scenarios result from the application of tools would seem to reflect a framing of scenarios based on the perspectives of the integrated assessment modeling community in which scenarios are largely the outputs of a model experiment. This is, however, but one way of developing scenarios. Moreover, references to scenarios as tools are ubiquitous in the scenarios literature. As a case-in-point, one of the most common descriptions of scenarios cited in the literature is that of the futurist Peter Schwartz: “A scenario is a tool for ordering one’s perceptions about alternative future environments in which one’s decisions might be played out.” As such, referring to scenarios as tools seems entirely appropriate. Schwartz, P. (1991). The art of the long view. New York: Doubleday.

Section 6.4 on Scenarios provides an inadequate description of the field:

1. Incorrectly the text ties all work in the area to cases developed to support IPCC activities (IS92, SRES, RCPs) and ignores the efforts of the EIA, IEA, industry groups (e.g., Shell, Exxon, BP) and the large literature of the community of integrated assessment modelers.

RESPONSE: The original text did largely focus on a limited set of scenarios, largely because these scenarios have been instrumental for global change research that informs carbon cycle dynamics. EIA, IEA, and Shell scenarios prescribe pathways, but have seen limited application in impact assessment or as inputs in integrated assessment modeling. Moreover, the RCP/SSP framework is very a product of the integrated assessment modeling community, as were the SRES scenarios. An additional paragraph has been added discussing the Shell scenarios specifically with references to other private sector or trade association scenarios as well as IEA and EIA scenarios.

2. Incorrectly it also says the scenarios are developed largely for inputs to Earth System Models and ignores another main use as a basis for policy studies.

RESPONSE: Scenarios in general certainly have a prominent use in policy studies and analysis. The original text, however, referred specifically to IS92a, SRES, RCPs, etc. which have largely (but not exclusively) been used to force climate models. However, the text has been modified to emphasize that this class of forcing scenarios has been important to the climate modeling community without suggesting that is their only application.

3. Though the citations are provided, the discussion could convey a better understanding of the reason for the structure of the RCPs (i.e., to avoid the time disjunction between the emissions projections and climate runs in the IPCC process), and the original purpose of the SSPs (to seek coherence between socioeconomic assumptions in emissions projections and in analysis of impacts and vulnerability).

RESPONSE: An additional sentence has been added to address the motivation for the parallel scenario process with respect to reducing the time required to generate new scenarios for climate modeling. However, the later point about the SSPs being developed to “seek coherence” is debatable, given that the need for the SSPs is a function of the choice to develop the RCPs independent of explicit socioeconomic assumptions. This necessitated development of those assumptions after the fact to fill a contextual gap. In fact, because the RCP/SSP framework assumes independence between SSPs and RCPs, yet some of the SSPs are assumed to be inconsistent with some of the RCPs, some have found the RCP/SSP framework rather incoherent.

P264, Line 39-41
Not correct. Vulnerability research covers many other sectors and concerns (e.g., species survival, ecosystem damage). RESPONSE: Section has been revised to acknowledge the broader use of vulnerability in the research literature.

P265, Line 30-31
Not clear what alternative design is suggested.
RESPONSE: The intent of including carbon cycle considerations in future studies is discussed in the next paragraph; the intent of this paragraph is to point to studies that do not include carbon cycle considerations; text has been slightly edited.

P270, Line 35-37
As with P256 / Lines 19-21, the statement should be qualified in that the costs are not all intangible or unknown.
RESPONSE: Text has been edited to indicate that intangible/unknown costs may be *perceived*, thus lowering motivation to change.

Chapter 7 Tribal Lands

The authors of Chapter 7 (Tribal Lands) greatly appreciate the constructive and detailed review comments provided by the National Academy of Sciences Review Panel. In response to the reviewers' comments, the authors have reorganized and redrafted the chapter. The revised text highlights the role that traditional knowledge and traditional agricultural practices play in carbon cycle management on tribal lands. Specific issues that have been addressed in the revised 4$^{th}$ Order Draft (4OD) dated 24 May 2018 are below:

The Introduction has been rewritten to address the Overview/Main issues identified by the reviewers and to incorporate the suggested alternative approach provided on pages 4-5 of the NAS review, in specific:

- The introduction now focuses on the differences in worldviews between Indigenous Peoples in North American and those of European settlers on the continent. This, as well as information about how these different worldviews affect land management, forms the basis for explaining why Tribal Lands are treated as a separate chapter in this Report.
- Many of the statistical details about Indigenous communities in the United States, Canada, and Mexico have been moved to Appendix 7A.
- Clear acknowledgement is made that there is a dearth of carbon stock and flux measurements on tribal lands, which makes assessing a baseline difficult. This important point helps underscore the value of improving our understanding of Indigenous land carbon cycling. The lack of data is addressed by providing specific examples, "case studies", comparing the impact that traditional (tribal) agricultural practices have on carbon management with the impacts resulting from Euro-centric agricultural practices. Comparisons of tribal and non-tribal agricultural practices on different land types (e.g., soils, forests, agricultural lands) guide our understanding of how traditional practices play a role in carbon management, even if carbon management is not an explicit tribal focus (although we give ample examples of when it is) and even though carbon fluxes on tribal lands have not been measured. Numerous suggested additional references have been included.
- The daunting political, historical and socioeconomic challenges facing native communities in North American (U.S., Canada, and Mexico) are summarized and documented with many supporting references. These are explicitly called out as "formidable challenges" to addressing the topic of carbon management on tribal lands in the Report. Other substantive changes made to the body of the Chapter:
- Key Findings have been rewritten to explicitly link Indigenous practices to carbon management.
- Detailed Case Studies are presented in section "7.3 Current Understanding of Stocks and Fluxes" to illustrate specific traditional practices (agriculture/forestry) and their implicit role in carbon cycle management. This gives the reader more detailed information while avoiding generalizations across the diverse groups of Indigenous Peoples in North America. Contributions from fossil fuel and uranium extraction on tribal lands to carbon emissions and the role of renewable energy generation are explicitly discussed as a Case Study.
- Section "7.4 Indicators, Trends, and Feedbacks" was rewritten to reflect understanding gained by comparisons of tribal and non-tribal practices on different land types.
- Section "7.5 Societal Drivers" was rewritten linking specific cultural, socioeconomic and governance challenges faced by native communities to carbon stocks and fluxes.
- Section "7.6 Synthesis, Knowledge Gaps, and Outlooks" was rewritten to emphasize the critical role educating youth plays in sustaining traditional practices tied to sustainable land management that can affect (or be affected by) carbon management on tribal lands in the future.

In accordance with discussions at the SOCCR-2 Authors' Workshop (April 2018), Indigenous groups in Mexico and Canada are still included in the Chapter with statistical details moved to Appendix 7A and specific examples outlined in the Case Studies.
That said, most of the Case Studies presented pertain to Native Americans in the United States.

**Chapter 8: Observations of Atmospheric $CO_2$ and $CH_4$**

**Overview/Main Issues**

The chapter has a clear organizing structure and clear findings that summarize the current state of research on monitoring global $CO_2$ and $CH_4$ and using inverse analysis to resolve carbon emission and uptake estimates. Discussed below are some suggestions for how the chapter could be augmented to provide a more complete picture of the state of research in this realm — in particular regarding discussion of current understanding of North American trends in $CH_4$ emissions.

**Statement of Task Questions**

- *Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?*

If the goal of this chapter is to define the stock and trends of $CO_2$ and $CH_4$ in the atmosphere and to present best estimates of carbon emissions and sinks at global, continental, and national scales, then yes, it does an excellent job of that. The first paragraph of introduction alludes to that goal "*Atmospheric concentration measurements of these two species provide fundamental constraints on sources and sinks, quantities that need to be monitored and understood in order to guide societal responses to climate change. These atmospheric observations also have provided critical insights into the global carbon cycle and carbon stocks and flows among major reservoirs on land and in the ocean*." However, the paragraph could be phrased to more directly state these as goals. The audience isn't specified, but implicitly it is the same audience as the overall SOCCR2 report.

- *Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?*

We believe the  report accurately reflects the scientific literature. However, more discussion about controversy surrounding inverse analysis of $CH_4$ emissions in North America is needed.

Mention of the ongoing debate could be merged with discussion about what are the limits on trend detection from inverse analysis using the current array of measurements.

· *Are the findings documented in a consistent, transparent and credible way?*

Yes, be believe the main findings are well documented.

· *Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?*

Figure 8.1 needs some work. Putting both $CO_2$ and $CH_4$ together in the figure does not work well. It is confusing why only the non-fossil fuel emissions (a negative value, thus a sink) are presented. It would be more informative to show both the fossil fuel source and the non-fossil, net sink, separately for $CO_2$ and $CH_4$. Because this is a North American carbon cycle report, it would helpful to separate the North American contribution from the total global contribution. This figure could refer to elsewhere in the chapter for partitioning of the fossil fuel source by energy type if it is presented.

Figure 8.2 shows a good illustration of the expanded $CO_2$ observation network (not GHG monitoring network as stated in the figure caption). Can a corresponding network of $CH_4$ observations be presented? Part of reason for not being able to quantify trends in $CH_4$ could be that the observing network is still too sparse.

Figure 8.3. The $CO_2$ "emission" panel is not clear. The values are negative, a $CO_2$ sink. Does that mean this is only the non-fossil fuel contribution as was shown in Figure 8.1. The text does clarify a little that the $CO_2$ is a sink, nevertheless the figure ought to be able to stand alone to be the main thing some readers will see by looking at the on-line version of the report. This figure would be more informative if it included the fossil fuel source. Although that's not a result from inverse analysis, it puts the net, non-fossil fuel uptake in context.

Figures are referred to in the text out of order.

· *Are the research needs identified in the report appropriate?*

The chapter gives a good overview of research needs and next-generation observations that are coming available now.

· *Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?*

Statistics from multi-model comparisons are used where appropriate. Uncertainty analysis for the individual models isn't summarized.

· *Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?*

Yes, this is reasonably well done.

· *What other significant improvements, if any, might be made in the document?*

One place for improvement would be to the recognition of controversy regarding  the status of North American trends in $CH_4$ emissions. The chapter reports an absence of evidence for $CH_4$ emission trends in temperate or boreal regions. Recent papers suggesting otherwise are not mentioned (e.g. Hendrick et al., 2016; Jacob et al., 2016; Kort et al., 2014; Turner et al., 2016). This finding is controversial, and some other recent literature contradicts it, e.g., see Bruhwiler et al., 2017; Miller et al., 2013; Turner et al., 2015; Wecht et al., 2014. Nevertheless, the chapter should point out the disagreement. The chapter could more strongly highlight the point that there is no apparent trend in North American $CH_4$ emissions despite clear indication that production and use of $CH_4$ has been increasing in recent decades. Table 2.1 of the 2017 EPA report (EPA 430-P-17-001) shows 2015 has lower total emissions than 2005, with the 2015 upticks in "natural gas systems" and "manure management" nearly cancelled by downturns in emission from landfills. Recent studies generally report that EPA national inventories and EDGAR global inventories are too low for parts of the U.S. (e.g., Miller et al., 2013). If there is no trend in total $CH_4$ emission despite increased activity that historically has been an important emission source, that is a very important finding— suggesting an improvement in technology that is reducing leakage and by-product losses. To put this point in perspective, it would help to report what the minimum detection limit would be for quantifying a trend in $CH_4$ emissions. Is the increase in $CH_4$ use high enough to exceed the uncertainty in inverse analysis of the sources? Figure 8.3 shows an uncertainty in inverse model estimates of $CO_2$ and $CH_4$ source/sink based on standard deviation between models. That is useful, but there are uncertainty analyses presented in the individual inverse analysis papers that provide better quantification of the uncertainty and explore its causes.

The text mentions in several places that the bottom-up inventory estimates of U.S. biospheric $CO_2$ emissions show less interannual variability than the results from inverse analysis of atmospheric data. The text does note that inventory estimates are based on Forest Inventory and Analysis (FIA) sampling that is only repeated at 5-year or longer intervals. The current approach to estimating U.S. biospheric emissions is simply not designed to provide annual estimates. Comparing the inverse analysis and inventory estimates at annual time steps is not appropriate. A better comparison would be to examine whether decadal averages agree. One can make the point that biospheric $CO_2$ exchange should be viewed as a multi-year average, but there is no need to belabor the issue. If annual estimates of biospheric $CO_2$ exchange inventories are desirable, the chapter could point this out as a critical future research need.

Throughout the chapter, the way $CO_2$ sinks are presented should be checked to ensure consistency with the rest of the SOCCR2 report. Presenting a $CO_2$ sink as a negative emission value is mathematically correct but requires the reader to be paying very close attention. Just a note to be sure this is consistent throughout.

There needs to be some additional discussion about the $CH_4$ sinks. Its lifetime is mentioned, but text doesn't mention that destruction by OH is the main sink.

The SOCCR2 report should somewhere mention the contribution from $^{13}CO_2$ isotopes to our understanding of the carbon cycle. If not in overview chapter, then it could be noted here.

· *Are the key findings well stated and supported by the detail provided in the chapter?*

The three key findings are well stated, sufficiently quantitative, and give a good summary of the supporting evidence and its uncertainties.

Key Finding 1 presents the incontrovertible result that the global atmospheric burdens of $CO_2$ and $CH_4$ are increasing. The point made in this finding could be sharpened by giving the pre-industrial values of $CO_2$ and $CH_4$ for reference as a final sentence in the finding. Thus, the finding would read: "stand at ……, compared to xx ppm and yy ppb for $CO_2$ and $CH_4$, respectively in the preindustrial atmosphere."

Key Findings 2 and 3 present estimates of emissions and sinks for $CO_2$ and $CH_4$ in North America estimated by inverse analysis. Key Finding 2 reports a fairly constant $CO_2$ emission with small variance and a sink that is about 1/3 of the continental source but has nearly 50% variability and suggestion of increasing trend. Inverse analysis for the land sink disagrees with the inventory estimate.

Key Finding 3 is that $CH_4$ emissions over North America are fairly constant and do not show clear evidence of trends, unlike global emissions which have been growing over the period. In the key finding statement it isn't clear how the inverse analysis results compare to reported emissions because they are given in different units (Tg $CH_4$ vs $CO_2$ equivalent-100yr). Please use common units. Conversion to $CO_2$ equivalent can be added to the text elsewhere if there is a section comparing the budgets of $CO_2$ and $CH_4$.

· *Are there other key findings or critical literature that are missing?*

Turner et al. (2016) and subsequent responses should be mentioned to better characterize the extent of debate in the community about $CH_4$ emission trends in North America.

P317, Line 28 onwards: Cite Kort et al. (2014) for $CH_4$ emissions from the Four Corners region of the southwestern U.S.

See comments and references regarding methane budget in Chapter 2.

**Line-Specific Comments**

P312, Line 24-25

It would be helpful to report the global emission trend and total global sink for the same period in order to address the obvious question of whether the proportion is holding constant or not. Emissions doubled from 5000 Tg in 1980 to 10000 in 2015. Sink increased by 2.5x from 2000 in 1960 to 5000 in 2015

P314, Line 13

There are no $CO_2$ data yet from the National Ecological Observatory Network (NEON).

P324, Line 8-10

As currently being deployed, the NEON network will NOT be reporting any $CH_4$ concentrations. They are measured but not going to be computed and reported because the $CH_4$ calibration was cut. This report would be a good forum to point out this penny-wise pound-foolish decision.

P319, Line 23-25

Does "emission of less than –500 Tg C" mean a greater sink (e.g. of –600 TgC)?

P341, Table 8.1.

Fossil fuel column: boreal and temperate North America do not add up to the North America total.

Chapter 9 Forests

.

**Response to NAS Review**

**Chapter 2 – Forest-related Comments**

**Comment**: The report accurately reflects the scientific literature to a large extent. For part of the Introduction and the Historical Context section, it is appropriate to cite some older but classical studies (e.g., Caspersen et al., 2000; Goodale et al., 2002, etc.) for the historical context of the North American carbon cycle studies. One more recent study could be useful here is Zhang et al. (2012), which shows that on average the carbon sink in the conterminous U.S. forests from 1950 to 2010 was Tg C/yr with 87% of the sink in living biomass.
**Response**: This has been partially addressed in Ch 9 Forests wherein the Introduction section aims to provide a brief setting of the stage with minimal citation. Nonetheless, we included citation to Goodale et al. 2002 to support the statement "In North America, forests—including urban forests, woodlands, and the products obtained from them—play a major role in the carbon cycle". We also included the Zhang et al., the Caspersen et al. study, and a Joos et al. study that challenges the Caspersen et al. study, to support the statement "Forests' capacity to sequester and store carbon is influenced by many socioeconomic and biophysical factors."

**Comment**: P103, Figure 2.1. Figure Legend: Spell out NCA; change "Forest" to "Managed Forest"?; change "Other Land" to "Other Land Ecosystems?"
**Response**:
Thank you for the comment. Forest has been changed to Managed Forest and NCA has been spelled out in the figure. Other Land is a land use classification consistent with the Land Representation used in UNFCCC reporting, so it has not been changed.

**Chapter 9 - Forests**

**Overview / Main Issues**

**Comment**: A major new result in this report is the conclusion that Mexican forests now are considered a carbon sink. In the previous assessment (SOCCR1) conducted about 10 years ago, it was reported that forest harvesting in Mexico contributed about 9 Tg C per year to the atmosphere. The more complete accounting presented in this report indicates these forests sequester approximately 41 Tg C per year. This is an important result and it would benefit from further support. Specifically, the authors should address what new process or fluxes have been included, or what values were modified to switch Mexican forests from a source to a sink for atmospheric carbon.
**Response**: We agree that this is a major new result of significance and have added explanation as available. Unfortunately, a more complete and detailed documentation has not yet been developed and provided. The bold text in the following excerpt was added to clarify this to some degree: "Mexico's forests are estimated to sequester about – 41 Tg C per year, overwhelming the net effects of land conversion estimated to release 9 Tg C per year (INECC/SEMARNAT 2015). Carbon releases from land clearing still exceed carbon uptake from reforestation, but their net effect is more than offset by carbon uptake in intact and degraded forest lands. This assessment departs from SOCCR-1, which reported a sizeable net carbon release from Mexico's forests based on a gain-loss analysis that emphasized land change **but omitted consideration of carbon accumulation rates in both intact forests and in degraded forests, with a corresponding net uptake of atmospheric carbon**. Although a complete methodological description is unavailable, the new data sources and methods used in Mexico's national reporting are believed to provide an improved account of the net carbon uptake in forest lands, which was previously underestimated."

**Comment**: The definition of forest articulated on the first page (line 23) is a bit perplexing. Forest is defined as having a land area as small as 0.5 ha and a canopy cover of as little as 10%, which seems to be a very "low bar" for defining a forest. Consider alternative definitions of forests, and whether this might affect the major findings of this chapter (See additional related comment below).
**Response**: Thank you for the comment. Because this report included Canada, Mexico, and the United States, an international definition of forest land was used from the Food and Agriculture Organization of the United Nations to ensure consistency in estimates across the three countries. This definition is also widely used by nations in greenhouse gas reporting to the UNFCCC.

**Comment**: Clarifying the temporal dynamics of carbon fluxes in forests would make this chapter more approachable. One of the challenges in understanding carbon cycling in forests is the time lag caused by the long-term storage of carbon in wood. When a tree is cut down and burned, it causes an immediate release of carbon to the atmosphere. This carbon was removed from the atmosphere over the past one or two hundred years, depending on the age of the tree. This displacement between carbon uptake and release poses a challenge when considering trees for bioenergy. For this reason, it is worth considering spatial integration instead of temporal integration to evaluate carbon-budget consequences— i.e., is there enough forest area in recovery right now to offset the carbon release from the areas being harvested or disturbed?
**Response**: We agree that this complicates interpretations and have added text in hopes of clarifying what has been included. We also note that aspects of this are discussed in Sidebar 9.1. We attempt to clarify by adding the following statement to section 9.3.2

Fluxes: "The fluxes reported here represent contemporary rates in recent years, spatially integrated to the country scale. Future legacies resulting from contemporary or historical drivers of forest carbon dynamics are not included. Such trends are particularly important if those drivers exhibit long-term trends, such as a decline or increase in harvest or natural disturbance rates, which would lead to trends in carbon fluxes." We also note that section 9.4.6 Projections and section 9.7 Carbon Management both consider spatio-temporal integrations to some degree.

**Statement of Task Questions**

*Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?*

**Comment**: For the most part, this chapter accurately reflects the scientific literature, but see specific comments below.
**Response**: Thank you.

*Are the findings documented in a consistent, transparent and credible way?*

**Comment**: Yes, the findings are documented in a consistent, transparent and credible way.
**Response**: Thank you.

*Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?*

**Comment**: The embedded text in Figure 9.1 is illegible (too small), and the inclusion of "product partnerships" on the right side of the figure is cluttered and distracting. Similarly, Figure 9.5 is not readable.
**Response**: Thank you for the comment. We will work with the Editors to ensure the final graphics in the chapter are legible and publication quality.

**Comment**: The authors should define the meaning of positive and negative signs in Table 9.3.
**Response**: These signs have been revisited and corrected, where necessary, to conform with the following explanation now included in the table's caption. "Exchanges with the atmosphere (e.g. terms 1, 2, 3, 4, 5) are assigned a negative sign for transfers out of the atmosphere (aka removals or sinks). Stock changes in forest lands and in wood products are assigned a positive sign if they are increasing."

*Are the research needs identified in the report appropriate?*

**Comment**: The authors have done an admirable job of identifying knowledge gaps and research needs in 9.8.2. This section would benefit from also discussing the shortcomings of current modeling and inventory approaches because they have large uncertainty in simulations and substantial spread among models.
**Response**: Thank you for the comment. The authors agree that this is an important consideration and that much of the uncertainty in model predictions and inventories can be attributed to the gaps and research needs mentioned in this section. Text has been added to clarify this point.

*Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?*

**Comment**: The numbers for the magnitudes of carbon sinks/sources and stocks are presented with multiple significant digits and are not associated with uncertainty estimates. It is fairly important to provide some kind of uncertainty estimates or data ranges. **Response**: Thank you for the comment. In some cases, the significant digits are the result of maintaining consistency in units within and between chapters. Where possible we will limit the number of significant digits to avoid overstating precision. In terms of reporting uncertainty, in many cases uncertainties do not exist for estimates compiled over the times-series since SOCC1. Instead, the authors have provided an assessment of certainty associated with each key finding in the chapter and have also noted in the text where sources of uncertainty exist in estimates throughout the chapter.

*Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?*

**Comment**: Yes, the document's presentation, level of technicality, and organization are effective. Section 9.4: Need to clarify: what are the indicators? What are the feedbacks? Are they feedbacks to the climate system? It appears that this section does not contain indicators or feedbacks as its title indicates. These components should be added; otherwise, the title of the section should be modified accordingly.
**Response**: Guidance provided to chapter authors requested that we title this section "Indicators, Trends, and Feedbacks" but it never seemed logical or appropriate and we agree with the review comment. Section 9.4 is now titled "Attribution and Trends" to reflect the section's emphasis on attributing the aggregate fluxes and stock changes to specific processes such as land use and land use change, forest management, climate and atmospheric chemistry, natural disturbances, and trends in these and other drivers.

*What other significant improvements, if any, might be made in the document?*

**Comment**: It would be helpful if the numbers for magnitudes of carbon sinks/sources and stocks could be compared to those reported in SOCCR1, which can illustrate how the numbers have changed because of various factors (e.g., changes in carbon fluxes/stocks, changes in methods used, differences in components considered).
**Response**: This is a good thought but executing it is rather complicated given substantial changes in the way the numbers have been tabulated in this current version.

**Comment**: The second paragraph of the Introduction provides a definition of forests used in this chapter: land greater than 0.5 ha with trees greater than 5 m and canopy cover of > 10%. This is quite different from the definition of forests in widely-used classification schemes for satellite-derived land-cover maps. For classification schemes like IGBP, forests are defined as areas with >60% of tree cover with >2m tree height. Most modeling studies are based on land-cover maps with classification schemes like IGBP and therefore use a different definition of forest compared to this chapter. How were differences in forest definition and the resulting discrepancies in forest area and carbon fluxes/stocks among different approaches and studies reconciled in in this chapter? This should be appropriately addressed.
**Response**: Thank you for the comment. Because this report included Canada, Mexico, and the United States, an international definition of forest land was used from the Food and Agriculture Organization of the United Nations to ensure consistency in estimates across the three countries. This definition is also widely used for land representation in greenhouse gas reporting to the UNFCCC which requires that estimates be comparable between nations. Since estimates from these official UNFCCC communications were used from each country in this report they were assumed to be consistent and comparable.

**Comment**: As indicated by the report, there are still differences in the methods used for estimating carbon stocks and their changes among the three countries. Were these differences explicitly considered and reconciled in this chapter? If yes, how? **Response**: No, these differences were not explicitly considered or reconciled in this chapter. We sought to explain methodological differences (see for example Sidebar 9.1) but it is beyond the scope of this review and synthesis activity to explicitly resolve those differences. In fact, if we had, the results would be unpublished and would be subject to suspicion. Instead, we provided a comprehensive quantitative summary of national inventory reports and results from the peer-reviewed scientific literature.

**Line-Specific Comments**

**Comment**: P342, Line 35: Finding 3 is not written very clearly. In order for harvest to offset a part of the net carbon sink in forests the managed forestry must be considered separate from other forest land. Is that what is meant?
**Response**: No, that is not what is meant. This key finding makes two separate points, one about forest regrowth offsetting annual harvest removals and a second about biomass use and wood product carbon emissions offsetting half of the net carbon sink in forests. These findings are linked because biomass removal and use through the practice of harvesting both decreases forest carbon stocks and causes carbon emissions outside of forests. Apologies but it is unclear to us what part of the writing lacks clarity. We hope the following rephrasing helps.
"Annual harvest removals from widespread forestry in select regions decreases forest carbon stocks, but this decline in stocks is offset by post-harvest recovery and regrowth in forestlands that were harvested in prior years. Removal and use of harvested biomass causes carbon emissions outside of forests, and these emissions offset a substantial portion (about half) of the net carbon sink in North American forests."

**Comment**: P343, Line 4: In point 4, natural disturbance and land conversion are considered together. Conceptually it may be more instructive to keep them separate. A natural disturbance will often be followed by some phase of regeneration, and an appropriate way to think about the disturbance-associated budget term would be as disturbance return interval, relative fraction of land under disturbance, and regrowth rates relative to the rate during the last cycle. Except for fire management intended to reduce fire severity or frequency, there are few opportunities to alter this budget component. Anthropogenic land conversions are different because they are permanent and directly related to management decisions.
**Response**: The two concepts are expressed in separate sentences and thus kept conceptually distinct to some degree. While it is possible to expand these into two separate key findings, we feel that they can remain together without confusion. While we agree that the potential for humans to influence these two processes is distinctly different, this concept is not central to the key finding.

**Comment**: P344, Line 14-35: The summary of the forest carbon cycle doesn't explicitly note the live respiration term but focuses on photosynthesis and death, which partitions into debris, soil organic, and decomposition. Photosynthesis could be described as having multiple allocation pathways, either to new growth, or supporting respiration.
**Response**: Changed to "As trees photosynthesize and **allocate a portion of this carbon to** grow**th**, carbon is removed from the atmosphere and stored in living tree biomass." The statement now implies that a portion of the assimilated carbon is released back to the atmosphere via autotrophic respiration such that it does not contribute to a lasting removal of carbon from the atmosphere.

**Comment**: P344, Line 34: In stating which regions store the most carbon, consider distinguishing regions that have large stock because the area is large and those that have high carbon density. This statement would be enhanced by having a figure to indicate the regions or reference the map in Figure ES1 if that is how regions are defined in this chapter.
**Response**: Thank you for the comment. The authors agree that referencing graphics in the report will help readers identify regions with large carbon stocks (which may be due to the areal extent) versus areas with high carbon densities. We refer the reader to the maps describing regions in the U.S. (Figure ES.1) and the biomass density map for North America in this chapter (Figure 9.1) which illustrates the variability in biomass/carbon density in Canada, Mexico, and the United States.

**Comment**: P345, Line 2: In this chapter as in others, the units for carbon stocks are used inconsistently.
**Response**: Thank you for the comment. We are working with the Editors and Science Leads to ensure consistency in units and sign conventions within and between chapters of the report.

**Comment**: P345, Line 23: Should the loss of carbon from forest conversion to settlements be cross-checked against the sink for urban trees, to be sure there are no offsets or double counting?
**Response**: Thank you for the comment. The inventory compilers for the forest land category working closely with the team that compiled urban tree estimates in the US to ensure that there are no gaps and also that uptake and emissions are not double counted. There is often a transfer of carbon from forest land to settlements during conversion.

**Comment**: P348, Line 2: The phrase "major contributor to net reductions in atmospheric $CO_2$" is incorrect. $CO_2$ is indeed increasing. Please rephrase.
**Response**: This has been rephrased as "Land-use change, including the conversion of non-forest land to forest land, in European nations (Nabuurs et al., 2013) and the United States (Woodall et al., 2015), has taken up a sizeable amount of atmospheric $CO_2$ since 1990, but this effect is expected to slow in the near future (Coulston et al., 2015; Nabuurs et al., 2013)."

**Comment**: P349, Line 8: When discussing the nitrogen deposition influence on forests the trends in deposition should be mentioned. National Atmospheric Deposition Program (NADP) trends (a good proxy), show declining NO3 in wet deposition and increasing NH4–thus indicating a complicated national trend with spatial variations.
**Response**: We added the following: "The U.S. is a global hotspot of nitrogen emissions and deposition, with a steady rate of wet deposition of dissolved inorganic nitrogen from 1985 to 2012, however the contribution from ammonium has increased relative nitrate, and deposition is higher in the Midwest and Northeast than the South and West (Du et al. 2014)."
Du, E., et al. (2014). "Changes in wet nitrogen deposition in the United States between 1985 and 2012." Environmental Research Letters **9**(9): 095004.

**Comment**: P349, Line 14-29: "Tree" and "plant" are used interchangeably in this chapter. It is recommended that the authors stay with "tree" in this chapter.
**Response**: We feel that the word plants is equally appropriate where used in the chapter, and does not introduce confusion, and it allows us to consider all plants in forests including those that are not trees.

**Comment**: P349, Line 30-39: The paragraph discussing SOC would benefit from some mention of the role of soil warming. See Melillo et al., 2017.
**Response**: We included: "Furthermore, severe warming of forest soils has been shown to accelerate soil organic matter decay and result in net loss of soil carbon with emission as $CO_2$ (Melillo et al. 2017)."
Melillo, J. M., et al. (2017). "Long-term pattern and magnitude of soil carbon feedback to the climate system in a warming world." Science **358** (6359): 101-105.

**Comment**: P350, Line 11-33: Discussion of how disturbance affects forest carbon budget ought to consider whether the overall carbon budget across a landscape or region is or is not at steady state, where steady state can be achieved with a small area being disturbed and offset by larger area that is recovering. Critical considerations are size of disturbed area relative to undisturbed re-growing area and return intervals. Parts of this paragraph are not adequately quantitative. What are the regrowth rates after disturbance; how delayed is the decomposition? If these numbers are not well understood, then this should be spelled out as a research need.
**Response**: These are good ideas, but page limits restrict full discussion in the chapter. Section 9.8.2 Gaps already raises some of these issues (see third paragraph in the section). Also, in section 9.4.5 Natural Disturbances, we added the following bold text: "Disturbance impacts on region-wide carbon dynamics can be large and result in sizeable interannual variability in the forest carbon balance (see Figure 9.4)**, and landscapes often contain offsetting effects of large carbon releases in small areas that recently experienced severe disturbance and modest carbon uptake in larger areas at various stages of recovery from prior disturbance.**"

**Comment**: P351, Line 15: Suggesting that forest sink strength will decline as forests age relies on dogma that old forests aren't strong carbon sinks, but that is not supported by observations from the oldest stands available to study where carbon gain is still strong (Luyssaert et al., 2008).
**Response**: Observations provide clear support for the notion that forest sink strength is strongest in intermediate stand ages, and that the oldest stands can still be sinks. Even the Luyssaert et al. 2008 paper shows this in its Figure 1a, and this is shown in many other inventory-based and eddy covariancebased studies.

**Comment**: P352, Line 13: Isn't SCC typically expressed per ton?
**Response**: Thank you for your comment. Metric units are used throughout this report so the SCC has been expressed as USD per megagram (Mg). 1 US ton = 0.90 Mg.

**Comment**: P353, Line 13: The consequence of bioenergy doesn't seem to be treated consistently in this chapter. On line 15 it is counted against strategies to reduce fire, but on line 26-27, substituting biofuels for fossil fuel is given as a strategy for reducing carbon emissions.
**Response**: Thank you for the comment. The authors have clarified in the text (p353, line 15) that there may also be mitigation benefits from using wood for bioenergy, depending on the type of woody material used for bioenergy, as well as the fate of that material in the absence of fuel reduction treatments.

**Comment**: P354, Line 14-20: It would be useful to discussion whether forest harvesting is compensated by regrowth, considering regional balance as well as temporal balance. One can evaluate the balance by considering a unit of forest area that is harvested and recovers (or not) over many decades (as is done here); or one can take a regional approach where some patches are being harvested each year and the remainder is left alone. Is there enough land in regrowth to compensate for the harvest loss? The point that accelerated disturbances are reducing the carbon sink now should be paired with discussion about whether the land areas recently disturbed are likely to recover and become a large carbon sink in the near future.

**Response**: Most of these ideas are already represented in the chapter's content. We added the bold text in, "Several of the factors driving this sink are expected to decline over coming decades, and an increasing rate of natural disturbance could further diminish current net carbon uptake in the near term, **possibly giving way to increased net carbon uptake in the more distant future if forests fully recover from today's disturbance trends**."

**Comment**: P355, Line 28: The authors suggest that a priority for future research is creating a full climate impact assessment for forests, including albedo and methane and nitrous oxide fluxes. It may be prudent to cite Anderson - Teixeira et al. (2012), which provides a computation framework for integrated quantification of the climate regulating value of forests and other ecosystems. **Response**: This has been added.

**Comment**: P357, Line 34: The authors state, "*Although the reclassification of land from non-forest to forest… does not… involve emissions or removals of atmospheric carbon, the processes underlying such reclassifications invariable do*." It is unclear why the authors feel that it necessary to draw this distinction. **Response**: Thank you for the comment. This statement is meant to clarify that a reclassification of land use may result in a transfer of carbon from one land use category to another with no net change in carbon stocks but the underlying processes leading to the land conversion (e.g., a change tree cover or the areal extent of the land use) and reclassification may result in carbon emission or removals.

**Comment**: P379, Table 9.3.: Is "2.Net due to forest land gain and Loss," meant to say "Net FLUX due to…?." (Without clarifying, one might mistakenly interpret the numbers to be areas). **Response**: Corrected.

Chapter 10: Grasslands

**Summary of Revisions in Response to NAS Review**

In response to the review, we have slightly expanded the definition of grasslands to include mesic areas where disturbance or soil factors limit woody vegetation, such as in the Southeastern U.S. We added citations that had previously been overlooked in relationship to interactions between fire, invasive species, grazing and carbon cycling in grasslands. We removed the references to global grasslands, instead starting with grasslands in North America and then narrowing down to countries, regions and case studies. We retained the original organization, but we offer to remove section 10.2, Historical Context, if needed to reduce the page length. This section does contain a brief overview of topics covered in more detail later in the chapter. We have improved the consistency of terminology, abbreviations, units, and sign conventions throughout the chapter. We revised and improved the section related to nitrogen limitation to $CO_2$ fertilization. We declined the suggestions to add more detailed case studies related to carbon cycling in southeastern U.S. grasslands because the chapter is already too long. We also declined to add discussions related to agricultural crops such as biofuels because we felt that this topic fit better in the agriculture chapter.

**Chapter 10: Grasslands**

**Overview/Main Issues**

In this chapter the authors demonstrate that grasslands typically take up more carbon from the atmosphere than they release (i.e., a sink) and that, unlike forests where carbon is in wood, much of this carbon is stored in soils. Carbon storage in grasslands is sensitive to climate, operating primarily through variation in the length of the growing season; and while there is unrealized potential to store additional carbon in these systems through proper management, with current practices this system is expected to become less of a sink with time.

The authors define grasslands in part as ecosystems that occur in areas where average annual evapotranspiration is greater than precipitation. While appropriate for most grasslands in North America, this definition misses the grasslands of central Florida. These grasslands occupy relatively small area but have a rather large economic impact through beef production. This chapter would benefit by expanding the discussion of southeastern grasslands.

Response: We decided to focus on the main regions of North American grasslands, which occur where precipitation is limiting.

In discussing processes affecting grassland carbon stocks (section 10.3.2), precipitation is identified as very important. Over the past few decades, there have been demonstrable changes in the timing and intensity of precipitation. While the effect of changes in the amounts of precipitation is discussed, a bit more attention to the role of intensification of hydrologic cycle would be useful.

Response: We added some text related to the timing and intensity of precipitation.

The encroachment of woody vegetation into grasslands is increasing as the climate warms and as fire is suppressed. The authors acknowledge this trend, but it was unclear how woody encroachment affects carbon stocks and fluxes in grasslands.

Response: We expanded slightly our text on carbon stock responses to woody encroachment.

**Statement of Task Questions**

- *Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?*

The statement that *"… moisture availability exerts more control over variability in productivity and carbon storage in grasslands than does grazing*." (p.386, ln 10) is overstated, and this statement would benefit from appropriate citations. Response: We replaced that sentence with a new one, including a citation:

"However, a global meta-analysis indicates that grazing impacts on carbon storage is contingent on many factors, including precipitation, soil texture, plant species competition, and grazing intensity; for example, grazing stimulated C storage in C4 grasslands by 67% but decreased it in C3 grasslands by 18% (McSherry and Ritchie 2013)."

The authors may wish to include one or more of the following references in their discussion of how cheatgrass affects biogeochemistry and hydrology (paragraph beginning on p.385, ln 12): Obrist et al. (2003); Prater and DeLucia (2006); Prater et al. (2006).
Response: We added the most recent relevant citation.

- *Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?*

Do the areas in Table 10.1 represent total area? If so, the area of grasslands would be more appropriate.
Response: We added "Grassland" to the column header to make it clear that the numbers refer to the grassland area in each region.

- *Are the research needs identified in the report appropriate?*

In the discussion of knowledge gaps (section 10.6.2), another source of uncertainty is the interaction between changes in land use and climate change. There are very few studies that investigated how changes in land use (grazing) in concert with changes in climatic factors (precipitation) will alter carbon processes. This lack of knowledge hinders our capacity to predict the response of carbon storage in grasslands to future climate changes, as we know that ecosystem responses derived from knowledge of single-factor experiments are likely to be misleading. See for instance: Norby and Luo (2004); Templer and Reinmann (2011).

Response: We added a sentence:

"Interactive effects of grazing, climate, soil type and plant community composition on carbon storage are not well constrained (McSherry and Ritchie 2013)."

In the discussion of "major uncertainties" (p .395, line 21), another major uncertainty is how much carbon sequestration in grasslands can be increased through management practices, plant breeding, or genetic modified organisms.
Response: We acknowledge that there are many other uncertainties that we have not been able to consider in this review chapter.
These crop breeding type uncertainties are more relevant for the Agriculture chapter.

- *Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?*

Most of the uncertainty and statistical analyses are presented in the primary literature cited in this chapter, rather than being applied directly in this synthesis.

**Line-Specific Comments**

P380, Line 13
Would it be advisable to present the key findings in order of confidence?
Response: We tried to organize the main content of the chapter to flow from large scale geographic patterns to process-level responses to climate change and land management. We prefer to maintain a similar order for the key findings.

P380, Line 32
Start with areal extent of grasslands in the U.S.
Response: We prefer to organize the chapter starting with the largest spatial scale (North America), then narrow down to regions and case studies.

P380, Line 33
In discussing the areal extent of grasslands it would be good to open with their coverage in North America rather than globally. Response: Amended.

P381, Line 19-20
Using the term "C uptake" without at least specifying if this refers to net or gross carbon uptake is misleading. See for instance, comments under section 10.3.2. It is unclear if references to C uptake always refer to net carbon sink or they are referring to GPP. Need to define what GPP is and that GPP-Reco is NEP. This way one could avoid using less-specific terms such as "all C uptake".
Response: Amended.

P381, Line 19-21
Should this convention be adopted in all chapters?
Response: We have attempted to align sign conventions for C fluxes across all chapters to be negative for uptake by the ecosystem and positive for loss from the ecosystem.

P381, Line 20 (and elsewhere)
Phrases such as carbon uptake and loss are used rather loosely. Do the authors mean net or gross? Where possible, it would be best to use standard ecosystem carbon cycling terminology, e.g. GPP, Reco, NEP, NPP, etc.
Response: We specified net carbon uptake or used standard ecosystem carbon cycling terms.

P381, Line 32-34
One would think this would be extensive to changes in precipitation such as drought. Arid systems will be more vulnerable to reductions in precipitation.
Response: We believe this is what we have conveyed in this sentence.

P383, Line 16-28
Does woody encroachment affect carbon cycling in grasslands?
Response; Please refer to Section 10.5.2 where we discuss woody encroachment effects on carbon cycling.

P383, Line 34
Standardize units across chapters.
Response: We have attempted to standardize our flux units to g C $m^{-2}$ $y^{-1}$ in this chapter.

P385, Line 1-11

One could use this study, along with others, to strengthen the idea (raised in section 10.5) that grasslands have potential to sequester additional carbon if managed properly.
Response: We believe that we have conveyed this message throughout this chapter.

P385, Line 12-26
It would be good to comment on precipitation changes expected in the future, and the fact that these changes will have a marked seasonality.
Response: This topic is discussed in more detail in Section 10.4.3.

P385, Line 14
The discussion of how growing season "plasticity" and corresponding variation in productivity (NPP?) responds to climate would benefit from consideration of how this variation would affect carbon losses by plant and soil respiration.

Response: Amended. We also note that this topic is discussed in more detail in Section 10.4.4, including discussion of soil carbon decomposition.

P385, Line 19-23
It would be good to also mention carbon losses, and the fact that the inter-annual variation in ecosystem productivity reflects interactions between SM and temperature controls on both “all C uptake” (GPP?) and carbon losses (Reco) - not only on carbon uptake. This would help illustrate that the sensitivity of GPP and Reco to these climate factors will likely differ, and this determines the net carbon sink or source strength of grasslands. This exemplifies how using the term carbon uptake is misleading, as it is unclear whether the authors mean gross or net carbon uptake.
Response: Amended.

P385, Line 33-36
Might want to add the recent paper Gomez-Casanovas et al. (2016), which shows that grazing increased the carbon sink strength of subtropical pastures. Subtropical grasslands are very important for U.S. beef production (look up Florida in the rankings for beef production); and along with tropical pastures, they are one of the most abundant grassland types across the world. Response: Thank you for this suggestion. However, due to length limits, we decline to add more case studies to the chapter.

P386, Line 9-11
This seems like an overstatement—at least if not accompanied by literature. If one of them exerts more control over productivity or carbon storage, it will depend on how much these factors change. Think about increasing the stocking rate from moderate to heavy. That will certainly affect carbon storage, which they acknowledge. Perhaps what is meant is that we can alter grazing intensity to a desired outcome—for instance, increased NEP-C storage in grasslands is more resilient to grazing than to precipitation because theoretically we cannot alter precipitation (although the management practice of “rain harvesting” may allow for this to some degree).
Response: Thanks for this comment. We have changed the sentence to:
However, a global meta-analysis indicates that grazing impacts on carbon storage is contingent on many factors, including precipitation, soil texture, plant species competition, and grazing intensity; for example, grazing stimulated carbon storage in C4 grasslands by 67% but decreased it in C3 grasslands by 18% (McSherry and Ritchie 2013).

P386, Line 12-15
See papers by Prater cited above.
Response: We added a citation to Prater et al.

P387, Line 31
The authors mention that models predict an increase in forest land carbon stocks in the Great Plains by 2050. It would be useful to know what factors are predicted to drive this increase in forest area.
Response: Amended. The increase in forest land carbon stocks is explained by woody encroachment or agricultural abandonment.

P387, Line 31
The driver for increasing forest is unclear.
Response: See last comment.

P388, Line 35 – P389, Line 4
Is worth mentioning nitrogen (N) deposition in this context, as many grasslands are not fertilized and therefore their only N input comes from deposition.
Response: This paragraph has been revised for clarity:
Nutrient limitation may reduce the potential for $CO_2$ fertilization in grasslands, especially over decadal timescales (see **Figure 10.5**). For example, a long-term experiment in a nutrient-poor grassland in Minnesota revealed that elevated $CO_2$ effects on NPP were dependent on soil nitrogen availability and experiment duration. During the first 3 years of the experiment, elevated $CO_2$ stimulated aboveground biomass by 11% and was not contingent on nitrogen availability, but over the longer term (4-13 years), the biomass response to elevated $CO_2$ increased by up to 20% with added nitrogen fertilizer (Reich and Hobbie 2013). However, in the coming decades, elevated temperature may offset the effects of nitrogen limitation, as shown by Mueller et al. (2016). On the other hand, increasing nitrogen deposition will stimulate NPP, up to a threshold, and greenhouse gas emissions may also have follow a similar non-linear response to nutrient loading (Gomez-Casanovas et al. 2016). Interacting effects of multiple global change factors still represents a large source of uncertainty in predicting carbon cycle responses (Norby and Luo 2004).

P388, Line 38 – P389, Line 4
Accurately predicting the response of carbon sequestration to elevated $CO_2$ and warming depends on the limitation or saturation of ecosystem processes to nitrogen. For instance, if a system is limited, one would expect $CO_2$ to increase carbon sequestration as biomass increases— and one would expect the opposite if the system is saturated. Predicting if grasslands will be N limited or saturated therefore depends on N deposition rates these systems experience in the future (at least for grasslands that are not fertilized by humans). It is also worth acknowledging that there is a large uncertainty in carbon responses to N deposition, and that this is hindering our capacity to accurately predict grassland carbon response in the future (see for instance Gomezhttps://docs.google.com/document/d/1bLd2gS_jLC3wwIkL0U0X1nbT7j85nQRJqMPOEfSdIEQ/edit - heading=h.4d34og8Casanovas et al., 2016).
Response: Amended (see above).

P389, Line 18-25
It may be worth mentioning that the responses of carbon storage to changes in precipitation will likely differ in xeric, mesic, and hydric systems, although we are not capable of accurately predicting the magnitude of the response. Need more discussion of timing of precipitation
Response: We added a short section on this topic in Section 10.4.3, in relation to productivity.

P390, Line 25 – P392 Line 19 The discussion of "societal drivers" would benefit from a short paragraph stating which other practices along with changes in grazing management and fire regime could potentially increase carbon sequestration in grasslands. It would also be interesting if the authors could link this to the uncertainty in future carbon stocks in grasslands. Mainly, we don't know which and how practices other than grazing and fire could affect carbon sequestration in grasslands.
Response: We decided to focus on the main management drivers relevant to grasslands, including fire, grazing, and the Conservation Reserve Program (CRP). Page limits preclude an exhaustive review on this topic.

P391, Line 8
The statement that removal of above ground biomass by grazing reduced soil carbon stocks would benefit from a reference or two.
Response: Amended.

P391, Line 26-31
How does woody encroachment affect SOM?
Response: We added a sentence:
Soil carbon pools may increase with woody encroachment, depending on other disturbance factors, especially fire (Barger et al. 2011).

P392, Line 8
Specify annual crops. It is a different story for perennial biofuels. Response: Amended.

P392, Line 1-19
The discussion of how converting grasslands to other vegetation types or management regimes (namely crops) needs to be a bit more specific. Some of the pressure on grasslands in the future will come from the expansion of perennial bioenergy crops. While replacing perennial grasslands with annual row crops typically reduces soil carbon stocks, replacing these systems with high-yielding perennial grasses for energy production can have the opposite effect.
Response: A detailed discussion of agricultural crop impacts on carbon cycling is found in a separate chapter.

P392, Line 22-31
The synthesis section would be improved by including some mention of grasslands in Southeastern U.S. because these grasslands are grazed and globally, tropical and subtropical grasslands play an important role in the carbon cycle; they store vast amounts of carbon, some of which is emitted to the atmosphere as $CH_4$. In addition, they are important from an economic perspective as the contribution of beef production in Florida is large.[1]
Response: Thank you for this suggestion. However, because our chapter is already too long, we decline to add more case studies and examples.

[1] See http://www.freshfromflorida.com/Divisions-Offices/Marketing-and-Development/Education/For-Researchers/FloridaAgriculture-Overview-and-Statistics.

P393, Line 21
Change “easily” to “readily.”
Response: Amended.

P393, Line 32-33
Is this true even when considering row crops?
Response: Yes.

P395, Line 21-29
A major uncertainty is how much we can increase carbon sequestration in grasslands through management practices, plant breeding or genetic modified organisms—because of the lack of field data. This seems a crucial point to make in addition to the uncertainty in precipitation patterns.
Response: This consideration is more relevant for the Agriculture chapter.
Response: We have discussed management-related uncertainties in this chapter. We acknowledge that there are many other uncertainties that we cannot discuss due to space limitations.

P411, Table 10.1.
Specify that “approximate area” is referring to grasslands.
Response: Amended.

Chapter 11: Arctic and Boreal Carbon

Overview/Main Issues
This chapter summarizes the current knowledge in high-latitude (mostly permafrost) carbon storage and dynamics. Arctic and boreal regions contain large carbon stock, especially in permafrost soils. The factors that control carbon storage have been changing rapidly over the last several decades. As a result, this large carbon pool is highly vulnerable for carbon loss in a future warming climate. There are major needs to reconcile model and observations in assessing permafrost carbon balance and in understanding the importance of abrupt thaw of permafrost.
The authors have done a commendable job in providing an updated synthesis of data and knowledge in high-latitude/permafrost carbon dynamics. It is very well written in general. The chapter provides a clear circumpolar/global perspective to provide context for the discussion of North American carbon cycle. The Committee makes the following suggestions to help improve the chapter.
Author response: We thank the reviewer for helpful comments in improving the content and format of this chapter.

- There is some discussion of long-term carbon accumulation processes (in 11.3.3) and discussion of projected future change in the year 2300 (in 11.4.2), but further discussions of longer-term past and future perspectives would be useful. For example, the permafrost carbon pool has been accumulating and has been relatively stable over the last several thousand years at least, but recent abrupt changes in controlling factors (warming, disturbances) may cause instability and degradation that the system hasn’t experienced in thousands of years. See wording suggestions on subsection 11.3.3 and key findings below. Author response: The scope of this chapter was intended to be on the current stocks and recent changes to high latitude carbon pools. In that light, we did not include much material on long-term historical drivers, although we think that the chapter is clear when it discusses that current conditions are quite different from historical conditions.

- The key uncertainties are clearly documented, but progress could be enhanced by ranking which research questions are most amenable to solution in the near term. It would also be useful to discuss ongoing research campaigns (i.e., ABoVE ) that may contribute to progress, and to provide clearer guidance to federal agencies about where they can most effectively use their resources.
Author response: A quantitative ranking is probably beyond the scope of this report, but the comment is useful. Text was added to this section to indicate that these issues are current research topics being addressed.

- The writing and organization are clear in general, especially in the early part of the chapter. However, it is uneven in the late half of the chapter when discussing carbon fluxes, suggesting a lack of adequate editing on subsections written by different lead authors. Note that subsection 11.5 is missing. Compared to subsections 11.3.1 and 11.3.2 on soil and vegetation carbon pools, subsection 11.3.3 on carbon pool change lacks detail. To maintain balance with the other two subsections, the authors could expand this subsection into multiple paragraphs, rather than just one densely-packed paragraph. It may be useful to state this is an overview of natural drivers affecting permafrost carbon pool, to distinguish from discussions on carbon fluxes in subsection 11.4.
Author response: This point is important and is also listed below in the specific line comments. There was intended to be a difference between the sections 11.3.1., 11.3.2 and 11.3.3 in detail and content and this was not indicated clearly with the numbering system. The numbering system was updated to reflect this and this change is detailed below in the line comments. - The chapter only cites Harden et al. (1992) on the subject of long-term accumulation histories, but there are more recent publications on this subject that could be referred to, at least for permafrost peatlands (e.g., Loisel et al., 2014).
Author response: This citation was included. The scope of this chapter was intended to be on the current stocks and recent changes to high latitude carbon pools. In that light, we did not include much material on long-term historical drivers.

Statement of Task Questions
• Are the goals objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?
The goals and objectives of the chapter are clearly stated in the first paragraph and the chapter meets the stated goals
• Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?
Yes, the chapter’s content and key findings accurately reflect the scientific literature. Some suggested references are included in the line comments.
• Are the findings documented in a consistent transparent and credible way?
Yes, the key findings are presented in a consistent transparent and credible way.
• Are the report’s key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence include an assessment of likelihood and communicate effectively?
In general, the chapter key messages and graphics are clear but see comments below on figures. The key messages reflect supporting evidence with appropriate assessment of likelihood. The figures/tables are effective in communicating the messages However the graphics and resolution of Figure needs improvement. Other figure suggestions are noted below.

• Are the research needs identified in the report appropriate

The research needs are not stated explicitly as part of Key Findings but are discussed at the end of the chapter. In particular, the chapter identifies the importance of reconciling model and observation difference in Artic vegetation greening, soil carbon stock change, and emerging research on disturbance of permafrost soils by abrupt thaw. Timescales appear to be a major factor in discussing vegetation and soils carbon pool change and carbon sequestration. The point that vegetation greening and shrub expansion may have limited or no impact on long-term soil carbon sequestration could be made more clear . Identifying and modeling processes that control long-term carbon balance seem to be important research needs. “Abrupt thaw” is an important disturbance event that causes instability for a system that has been stable over thousands of years

• Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?

Yes The results and findings are all from the peer-reviewed literature.

• Are the document’s presentation level of technicality and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?

In general, this is a well-presented chapter.

• Are the key findings in your chapter well stated and supported by the detail provided in the chapter

Comment on Key Finding: Timescales are probably an important parameter to differentiate permafrost carbon pool from forest or other biome biomass carbon pools, or even mineral soils carbon pool. This chapter is a place to put the contemporary carbon cycle in the context of longterm geological carbon cycle dynamics. Indicating the long term accumulation of permafrost carbon helps put carbon release through recent and future disturbance especially related to abrupt thaw into a broader context. The Committee suggests augmenting this finding with a statement such as “This large soil carbon pool in the permafrost z one has accumulated over hundreds of thousands of years.”

Line Specific Comments P Line
Change “ ” to “ ” to be consistent to

Authors: This change was made as suggested.

P Line
Change 1439Arctic ecosystems1439 to 1439the Arctic1439

Authors: This change was made as suggested.

P Line
Citations can perhaps be changed to 1439Romanovsky et al

1439 without repeating the author names. A similar formatting change could be made throughout the chapter.

Authors: This change left to report formatting decision P Line
It is a bit redundant to say both 1439the Arctic1439 and 1439high Arctic1439 Rephrase

Authors: This was rephrased P Line
Perhaps change the subheading to 1439Characteristics of Permafrost Carbon1439

Authors: This was an original header provided by the SOCCR report The section details the historical controls over both vegetation and soil carbon pools. Header was rephrased –
but it should be cross checked with other chapters to consider whether it should be left as is

P Line
Change 1439Ice covers1439 to 1439Ice sheets cover1439

Authors: This change was made as suggested.

P Line
Switch order of fluxes and stocks by changing to 1439Current Understanding of Carbon Stocks and Fluxe s1439 as this is the order of description below. Also why use different terms 1439stocks1439 and 1439po ols1439 here?

Authors: This change was made as suggested This chapter tries to use ‘pools’ in all locations and not
‘stocks.’

P Line
Change to 1439peatlands
Thoul Hijj carbonh 1439… and 1439mineral soils
Thoul Hijj carbonh 1439 as organic matter rarely contains much more than
Thoul Hijj carbonh and it is redundant and imprecise to indicate Thoul Hijjah for mineral soils

Authors: This change was made as suggested.

P Line
Change to 1439soils of many meters thick1439

Authors: This change was made as suggested [Oak Ridge please check this new wording]. P
Line
Change 1439sea levels were1439 to 1439sea level was1439

Authors: This change was made as suggested.

P Line
The values seem to be inconsistent as
PgC may only refer to boreal biome and tundra vegetation contains another PgC as in Table that is not included here .

Authors: This was a typo and was corrected P Line
Change heading to 1439Natural Drivers of Carbon Pool Change1439 to distinguish from subsection

Authors: See comment below P Line

This paragraph is weak especially compared to previous two subsections . This could be expanded by discussing long term historical drivers Focusing on just the past few centuries and millennia is likely too short as there are many recent synthesis of wel ldated peatland records showing that th ese ecosystems have accumulated carbon over
Years,eg Loisel et al
Treat et al
See general comments above

Authors: We agree with the reviewer that this paragraph could potentially be misinterpreted. The scope of this chapter was intended to be on the current stocks and recent changes to high latitude carbon pools. In that light we did not include additional material on long term historical drivers, although we agree there is a rich literature not covered in detail here.
The purpose of sections was to introduce the potential drivers and pathways of carbon pool change in a general background sense To that end the authors suggest to move this paragraph as the first paragraph of section and renumber accordingly [We have not made this change yet due to the effect on numbering throughout].

P Line
Change to 1439 Carbon Fluxes in Recent Decades1439

Authors This change was made as suggested

P Line
May need more discussion of boreal forest in this section to balance the focus on tundra Also perhaps cite some more recent references after the
synthesis such as Euskirchen et al

Authors: This chapter covers a broad scope of carbon pools and fluxes As a result we relied primarily
on syntheses or studies that were regional in scope that were already in the published literature rather than publications from individual study sites As a result of this reliance there is not necessarily consist ent coverage across all regions and we didn't necessary include all the latest citations from individual sites. There are currently citations from both boreal and tundra regions in this section.

P Line

Perhaps move this to section on future projections.

Authors This reviewer is correct that this statement is a projection but it relies primarily on this historical flux data that is discussed in detail in this section, rather than a full projection this statement of future carbon flux. This was meant to provide context for the g m carbon estimates in units that are comparable at regional scales. We decided that having this information here is better than splitting this discussion into two parts in the report.

P Line
Delete 1439soil area1439

Authors: Soil area is the correct modifier here Permafrost area can be larger but soil area excludes rocky mountainous regions from this comparison. P Line
Need more discussion on peatland fires, e.g. drawing upon Turetsky et al publications.

P Line
There are no discussions of insect outbreaks in the chapter elsewhere. Also provide an overview of the three approaches used for future projections at the end of this paragraph.

Authors: Insect outbreaks are introduced earlier on page.
L
In the section on page we discuss large scale pulse disturbances including insects but also say that fire disturbance is the best characterized at regional to continental scales and the review focuses there. The section on page mentioned here is again a general overview and so we think it is appropriate to retain insect outbreaks in the list even though it is not reviewed in any detail.

Text was added to introduce the three approaches used for future projections.

P Line

This subsection is weak except perhaps the first paragraph it is not really focused on upscaling

P
Line
The sentence is unclear

P
Line
Is that more suitable for the overview

Authors: These sentences were supposed to introduce the following paragraph The text was revised to b tter link these together.

P
Line
Missing subsection between and Reorganization is needed,

Authors: Section numbering needs to be checked.

P
Line
Should this be indicated as a Case Study in the subsection heading?

Authors: We agree that this should be described as a case study but are unsure how this is treated report-wide and need feedback on this.

P Line
Change to 1439Observations and modeling results summarized in this chapter1439.
Authors: This change was made as suggested

P Line
Two different statements 1439high confidence1439 and 1439very high confidence1439 Need more consistency.

Authors The confidence levels are applied to individual response variables and so vary across the paragr aph [Confidence levels in this chapter need to be assessed in reference to the whole report Lead author has had preliminary conversations with N Cavallero but no changes have yet been implemented]

P Line

Gorham may not be a correct reference for this statement. Please check.

Authors This reference is just one of several that reports soil carbon but is appropriate given, e.g. text was left as is

P Line
Change 1439Observational data1439 to 1439Experimental data1439

Authors: Observational refers to experiments and field measurements that are not experiments. Text was left as is.

P Line
Change to 1439Tundra area data1439

Authors: This change was made as suggested.

P Figure
The Y-axis labels can benefit from adding 1439year1439 on both sides

Authors: This change was made by Oak Ridge [check final figures]

P Line
The sentence in the figure caption is unclear Also change 1439carbon with the text1439 to 1439carbon i n the text1439

Authors: This change was made as suggested.

P Line
Change to 1439 see Table for references and data source 1439

Authors: This change was made as suggested.

P Figure
The authors may want to use a white color outer band for the category ‘Various Mineral’ while leaving Histosol organic gray This way three first order subdivisions Gelisol Histosol and Various are represented by three different colored outer bands.

Authors: This change was made by Oak Ridge [check final figures].

P Figure
Change the Y-axis labels to 1439Area km 1439 and 1439Area acres 1439 by showing the variables rather than just indicating measurement units

Authors: This change was made by Oak Ridge [check final figures]

Chapter 12 Soils

.

**Response to Review Comments from the National Academies**

The National Academies review noted several places for improvement of this chapter. A detailed response to their specific comments is provided as an appendix. It is important to note here that this chapter focused on the biogeochemical and microbially-mediated C cycling processes that are intrinsic to soil. We made great efforts to not repeat knowledge better addressed in other chapters that focus on land use/type/biome (e.g., Agriculture, Forests, Grasslands, Arctic and Boreal, and Wetlands). Therefore, issues such as fertilization, land management, frozen soils, were acknowledged, but more deeply addressed in those more domain-specific chapters.

Some broad themes emerged which we address here. A key concern was having consistent data reports for soil C stocks and fluxes between the soils chapter, and the budget and biome-centric chapters (notably, chapters 2, 5, 9, 10, 11, and 13). We have resolved most of these issues by cross-referencing data sources, and recognizing where differences occur (e.g., some chapters separated peatland forests from mineral forests, and others did not). We feel that the tables have explicit captions and notes, documenting the differences clearly. Note that chapter 13 had calculated their own numbers and are going through a detailed re-calculation following their NAS review. Their numbers will change.

There were also several comments regarding the need for greater clarity regarding the time frames over which C changes were measured or projected. Again, we have identified all of these, and provided specific timeframes where needed. Additionally, there were concerns about i) adequately describing the cascade of impacts that nitrogen has on the soil C cycle, and ii) with the role of changing land management on soil C cycling. Both of those require targeted treatment of land management, land use as pertains to forestry, agriculture, and wetlands. As such, these details are addressed in those chapters, as appropriate.

There was a suggestion that we include more experimental findings regarding soil warming experiments, rainfall manipulations, and other treatments. At the same time, there was a suggestion that we cite some major programs such as ABOVE and SMAP (NASA), and NGEE and SPRUCE (DOE). Given the complexity of soils and our focus on the unique processes that define soils, and not biomes or land uses, we synthesized many of these publications to extract broad generalizations that would be useful for this assessment. Highlighting individual experiments, or even suites of experiments would have led to a much longer chapter that was over-weighted with site-specific findings, rather than a general assessment. Through the ~275 publications that are now in the chapter, many of these key papers are represented and synthesized.

We also rewrote the modeling section to better reflect the scale differences of Earth system models and process-rich models and the state of the science in both.

Finally, there was some confusion with the detail in Key Finding 2. We used that comment to streamline and simplify all of our key findings in the introduction, leaving the greater detail and nuance in the explanation of key findings at the end of the chapter.

**Appendix. Detailed Response to Comments**

Some key issues to bring to the attention of the chapter authors:

- The authors state that there is no possibility of improving carbon stock estimates. This seems like a very strong statement, given that improvements have been realized and could be improved with increased effort. *i. Corrected*
- The authors focus on temperature impacts, but other chapters (e.g. grasslands and agriculture) emphasize rainfall impacts.
    i. *We have added a short section on precipitation effects.*
- In various places in the chapter, soil carbon stabilization is linked to microbial processes, to inputs from plant growth, and to physical stabilization; but there is no overall conceptual framework for how these different factors interact.
    i. *We added this sentence to explain how the chapter is structured, and moved the section on C protection mechanisms to immediately precede the section on C losses.*
    *"We outline the processes that govern overall C stocks and fluxes through soils, from inputs through microbial transformations in the bulk soil and rhizosphere, and the protection mechanisms that govern the overall longevity of C in soils."*
- The organization of the chapter by a mix of process and region means that some information and quantification may be overlapping and can't easily be combined.
    i. *The chapter was laid out to align with the other chapters; a section on domain principles (processes), a section on regional (national) context, and a section on drivers (ie, agriculture). We have carefully read through the chapter to capture and reduce overlap. This structure is particularly useful for combining information across chapters.*
- The chapter could use more discussion of experimental studies related to factors such as soil warming, tillage, rainfall enhancement and exclusion. There is a large literature on such matters that is barely referenced.
    i. *We have 270 citations that were assembled to support the generalizations we make. The wide variation in experimental approaches in soils, along with the diversity of research objectives for specific sites made it difficult to reference individual studies in an international report on the State of the C Cycle. Thus, we rolled up sets of responses to soil warming (for example) to focus on warming as a means of accelerating C losses. Tillage has variable effects across the continent, depending soil temperatures, land covers, and local climate. Thus, we note that it is promising but not prescriptive for soil C amelioration.*

The section on soil fauna indicates that fauna are important to soil—undoubtedly true, but this does not consider interaction effects of higher trophic levels or ecosystem engineers (e.g. earthworms) on soil carbon changes related to ongoing climate and land use. ● *We describe the role of ecosystem engineers.*

The section on nitrogen is weak and does not reflect conflicting evidence on N impacts through plant growth and through soil biological-chemical interactions.

- *We describe  the imbalance between N-stimulation of aboveground biomass, and its general inhibition of belowground C allocation, as well as the microbial dependencies on N and C for decomposition. Given length constraints, and the level to which we are writing, further detail than is given would be a challenge.*

Soil respiration is part of the annual cycle of uptake and release, yet the section on respiration does not distinguish between controls over the amount and phenology of the annual cycle versus decadal trends. Soil respiration is the major mechanism for carbon stock changes, so this section is confusing in conjunction with discussions of stock changes that result from altered respiration (e.g., due to climate change or tillage).

- *Chapter 8 focuses on overall fluxes of gases to the atmosphere, and they are able to break out annual dynamics, though these emissions are not exclusively microbial.*
- *We have added a statement about the role of warming in stimulating microbial respiration, and the transition to methanogenesis.*

Given the importance of modeling acknowledged in the chapter findings and introduction, the modeling section is weak and sparsely supported by literature. Some of the relevant literature is covered in the agriculture chapter, but the vast body of work from the leading soil modeling groups (e.g., USGS, Colorado State, DOE) is poorly captured compared to the far less mature Earth System Model literature. Limiting the modeling section to the ESMs does not fully portray the state of knowledge.

- *We have rewritten this paragraph to reflect the historical contribution of more traditional soil C models to contemporary ESMs, and also to separately address the advent of process-rich, microbially-explicit models.*

In a number of sections (e.g., on the Arctic/Boreal Zone), the literature review emphasizes papers from 5-10 years ago and does not seem to include recent papers based on extensive research efforts by NASA and DOE. For instance, see publications stemming from the NASA-led programs such as the Arctic-Boreal Vulnerability Experiment (ABOVE) and the Soil Moisture Active Passive (SMAP) satellite mission, and from DOE-led programs such as the Next Generation Ecosystem Experiment (NGEE), and Spruce and Peatland Responses Under Climatic and Environmental Change (SPRUCE).

- *The focus of this chapter is a generalized review of soil processes that can extend to almost any soil. ABOVE is a landscape-scale program that best serves land use change, rather than processes internal to soils. SMAP is revealing kilometer-scale moisture patterns that are more suited to climate science impacts to landscapes. SPRUCE focuses on peatland warming, which is a very specific feedback between organic wetlands and climate, that should be handled in Chapter 13. We have cited papers, particularly models that came out of NGEE-Arctic.*

Key Finding 2 notes the wide range of Earth System Model projections, which would indicate low confidence in model projections. High confidence should be applied to the result or not to the discrepancy among approaches. This could instead be listed as the last Key Finding.

- *We streamlined all of our key findings to reduce confusion in the certainty assignment. We also specified that we were very confident in the uncertainty of these estimates.*

Key Finding 3 needs to state a time frame for the soil carbon loss.

- *Carbon has been lost since tillage began; the time frame depends on this time point for all soils*

In the discussion of protection mechanisms, there is no mention of freezing and waterlogging as an important protection mechanism, especially in high latitude regions (boreal and Arctic).

- *Protection mechanisms discussed in this chapter were physical, chemical or biological. Chapter 11 (Arctic/Boreal) discusses inundation-freezing as a means of stabilizing soil C, including a detailed figure.*

P460, Line 32-36
Is this sentence useful It could be deleted..

*This sentence was NOT in our original submission, but a previous federal reviewer told us quite specifically to insert this text there, to put soil $CO_2$ fluxes in the context of other anthropogenic fluxes. Although we had not originally thought of this, we now think that it was a really good suggestion, because otherwise the flux estimate from soils is just a number without context. However, if asked we will remove it.*

P461, Line 32
Change to "The researchers found that the largest…."

*done*

P461, Line 2-4
"There is no possibility" is a strong phrase when it is known that better estimates could be made with better data. This makes it sound as if we will never have any better idea of North American carbon stocks.

*This was terrible phrasing on our part. The sentence has been removed because it was wrong and served no purpose.*

P461, Line 42 Change "huge" to "large" Delete "larger-scale" as ESMs are global scale, and thus "large-scale" is redundant.

*Agreed - done*

P461-466, Subsection 12.2.
The subsection could be better organized, perhaps as follows:
12.2.1 Plant Litter Inputs
12.2.2. Soil Microbes
12.2.3 Macrofauna (foodweb)
12.2.4 Rhizosphere Interactions
12.2.5. Protection Mechanisms
12.2.6 Nitrogen Effects on SOM Dynamics

*done – reorganized as suggested, though we moved Protection Mechanisms to the end of this list.*

P462, Line 27
Need to discuss freezing and waterlogging as protection mechanisms.

*Good point – we added this, plus other physical processes such as cryoturbation and erosion/deposition, however this is also covered in Chapter 11.*

*Added references:*

*Kaiser, C., H. Meyer, C. Biasi, O. Rusalimova, P. Barsukov, and A. Richter. 2007. Conservation of soil organic matter through cryoturbation in arctic soils in Siberia. Journal of Geophysical Research: Biogeosciences **112:** G02017. Grosse, G., J. Harden, M. Turetsky, A. D. McGuire, P. Camill, C. Tarnocai, S. Frolking, E. A. G. Schuur, T. Jorgenson, S. Marchenko, V. Romanovsky, K. P. Wickland, N. French, M. Waldrop, L. Bourgeau-Chavez, and R. G. Striegl. 2011. Vulnerability of high-latitude soil organic carbon in North America to disturbance. Journal of Geophysical Research: Biogeosciences **116**:G00k06.*
*Berhe, A. A., J. Harte, J. W. Harden, and M. S. Torn. 2007. The Significance of the Erosion-induced Terrestrial Carbon Sink. BioScience **57**:337-346.*
*Kroetsch, D. J., X. Geng, S. X. Chang, and D. D. Saurette. 2011. Organic Soils of Canada: Part 1. Wetland Organic soils. Canadian Journal of Soil Science **91**:807-822.*

*Sentence added: In addition, larger-scale processes can serve to protect soil carbon such as freezing, waterlogging, cryoturbation, or erosion-deposition (Kaiser et al. 2007, Grosse et al. 2011, Behre et al. 2007, Kroetsch et al. 2011).*

P463, Line l8
Is better to use the phrase "parent materials" rather than "source materials", because in soil literature, "parent materials" refer to bedrock or other materials that soil develops on.

*On line 8 (not 18) this change was made.*

P463, Line 11
*Change "outsized" to "important.".*

*done*

P463, Line 18-22
While this may be true, it is a logical leap that some readers may not be ready to take at face value. Yes, SOC stabilization by microbes affects the plant community, but the plant community also drives the microbial community (a chicken/egg scenario).

*We believe that the line being questioned is actually this one, not 18-22:*

*Microbes can affect plant carbon inputs by regulating plant nutrient supply (Bever et al., 2010; van der Heijden et al., 2006), which affects plant community composition and the timing, mass, and properties of plant inputs of litter and exudates.*

*We believe that it is extremely well established in the literature that microbes can regulate nutrient supply, and nutrient supply affects plant community composition and litter inputs to soil. Yes, the reverse is true as well, but that is not what this chapter is about.*

P463, Line 18-28
This appears to replicate subsection 12.2.2. Move/merge to that subsection.

*We do not feel that there is unnecessary overlap. The previous section states:*
*Protection of carbon within soil aggregates (i.e., physical associations between soil minerals and organic compounds) can lead to long-term carbon storage in soils (Jastrow et al., 1996; Six et al., 2004).*

*The current section states:*

*A key mechanism of SOC stabilization is protection within soil aggregates (Six et al., 2002), and fungal mycelia and bacterial extracellular polysaccharides are important in forming and stabilizing aggregates (Aspiras et al., 1971). SOC also is protected by chemical interactions with minerals, particularly silt and clay (Six et al., 2002), and microbes living on minerals may facilitate these interactions by depositing microbially derived carbon directly onto mineral surfaces (Uroz et al., 2015).*

*Thus, there is an overlap of about 6 words, which we do not feel is excessive.*

P463, Line 31-33
Can 2001 be called a "recent" paper

*Good point – no. This word was deleted.*

P464, Line 12-37
*Fauna: This section reads more like a primer than an assessment. Need discussion of the direction and magnitude of effects, and how fauna can amplify or moderate other effects.*

*Now all comments include the direction of effects, although the magnitude of effects has not really been measured. We added direction information in sentences such as:*
*However, one study found that the activity of earthworms increases C stabilization onto minerals to a greater degree than the increase in C mineralization, leading to net soil carbon increase (Zhang et al., 2013).*

P464, Line 41 – P465, Line 3
What is the cause of this diverging trend in nitrogen deposition— fertilizer use changes air pollution control measures other

*Pollution control, and this is now stated.*
P465, Line 12
This should be "23 grams of carbon per gram of nitrogen."

*True – done.*

P465, Line 16 – P466, Line 28
In discussing gas fluxes, need to be more explicit about time periods and spatial domains, to provide context for the numbers.

*We have endeavored to do so where appropriate.*

P465, Line 19-21
State during what time period or state "is released annually".

*Done.*

P465, Line 33-37
Could reference the Agriculture chapter here.

*Indeed, and the Agriculture chapter is using the same dataset.*

P465, Line 29-38
Need to use consistent units.

*The entire report is unifying units.*

P466, Line 2-3
What about $CH_4$ oxidation/sink in upland soils.

P466, Line 12
Only one component "of" net SOC changes.

*Fixed.*

P466, Line 30 – P467, Line 14
The literature cited in this section is particularly light and limited. Also, some terminology needs to be defined (priming, sorption, etc).

*This section has been revised.*

P466, Line 38
The "other ecosystem compartments" shouldn't include "atmosphere". Correct?

*Correct! Changed!*

P467, Line 31-32
Change the statement to "…captures change in the carbon content of soils across CONUS over time."

*But that is not what RaCA does – it does NOT examine soil C change, because it really, really measured soil C content over a very short time, meant as a snapshot of soil C.*

P467, Line 37-38
Is density the correct term for a unit that does not include volume.

*This is a great question. Carbon density is a standard term in ecosystem ecology and in forestry – used around the world – and it does mean C per unit area. Because it is such a widely-used term, and because RaCA uses it, we will use it too.*

P468, Line 15 – P469, Line 5
Check on depths and context for the numbers cited.
*Yes, clarification was added, by adding the depth to all SOC estimates.*

*Also added this reference:*

*Bliss, N. B., and J. Maursetter. 2010. Soil Organic Carbon Stocks in Alaska Estimated with Spatial and Pedon Data. Soil Science Society of America Journal **74**:565-579.*

P468, Line 17-20
Is 9.13 Pg C the 20 cm stock?

*Yes and this is clarified.*

Is it 73% of the 30 cm stock?

*No, as stated, it is "73% of the country's total terrestrial stock."*

Clarify to make the 18 Pg number (in line 19) make sense.

*Thank you, it has been clarified.*

P469, Line 7
"153.7 Pg were in organic (peat) soils" is different from Chapter 13 (Terrestrial Wetlands), where peatland soil in Canada is stated to contain 130 PgC in Table 13.1.

*Chapter 13 is revising their numbers, and chapter 2 is also engaged.*

P469, Line 7-37
This first paragraph presents conflicting data without smoothly transitioning between estimates: Tarnocai's total is 262.3 Pg C,
Kurz's estimate places boreal forest alone at 208 Pg (which is ~80% of Tarnocai's total). If the boreal forest is 208 Pg, then Tarnocai's estimate is low for total Canadian soil carbon, because of significant contributions from other regions, therefore Tarnocai's estimated total will likely increase as further research on permafrost continues.

*Agreed – and this is now stated clearly. More text was added to explain what was estimated from Tarnocai and what is reported from Kurtz.*

P469, Line 8-9
Reiterate that the remaining carbon stocks are those estimated by Tarnocai, as to not imply that all of Canada's soils are peat soils, tundra, forest, and agriculture.

*Agreed – fixed.*

P469, Line 9
Total soil carbon estimates for Canada likely will increase...

*Corrected.*
P470, Line 31-32
"Causes of soil loss in agricultural soils include..."
This list is true for all soils, except for the tillage part, which is specific to agricultural soils.

*Thank you; we endeavored to focus on generalizable soil traits.*

P470, Line 31-41
Quotes losses without specifying time periods. Over what period are these losses fast or slow?

*Thank you, we have changed it to be as explicit as possible.*

P472, Line 24
Reword as “moisture disturbances.”

*Done.*

P473, Line 10
Remove the phrase “types of.”

*Agreed, done.*

P473, Line 11-14
Could mention perennialism here.

*Very good point – we did and added this reference:*

> *Cox, T. S., J. D. Glover, D. L. Van Tassel, C. M. Cox, and L. R. DeHaan. 2006. Prospects for Developing Perennial Grain Crops. BioScience **56**:649-659.*

P498, Table 12.1
The value given for “other” (11.2 PgC) is smaller than the value given in Table. 13.1 for wetlands in conterminous U.S. (13.5 PgC). Please check for consistency between these values.

*This reviewer caught a clear discrepancy among published values, so we redid the table and updated the USGS numbers, which clearly are not correct.*

*We also a new column:*

> *Bliss, N. B., S. W. Waltman, L. T. West, A. Neale, and M. Mehaffey. 2014. Distribution of Soil Organic Carbon in the Conterminous United States. Pages 85-93 in A. Hartemink and K. McSweeney, editors. Soil Carbon. Springer, Cham.*

*We added in a column for RaCA estimates so that readers can compare.*

P500, Table 12.3
Add a new row at the bottom of the table for “Total”.

*Done but this is only for RaCA and Bliss and Sundquist – the other independent estimates do not sum*

Why is there no table for Canada?

*Done.*

P501, Figure 12.1.

- The lettering is too small to read.
- What about $CH_4$ emissions, especially from “peatland” (are they peatland, it is hard to tell near snow/ice )?
- Label “tundra” right below “snow/ice”, and label “land-use” at location between forest and agriculture
- Cannot comment on other boxes and meanings of all the arrows, as it is difficult to read what the authors intend to show

*Figure revised*

Chapter 13 Terrestrial Wetlands

.

This is a point-by-point response to the NAS review of Chapter 13. The full text of the review is given in black and author responses are provided in blue.

Chapter 13: Terrestrial Wetlands

**Overview/Main Issues**

This chapter focuses on carbon cycling in terrestrial wetlands (that is, non-tidal freshwater wetlands), providing information about area, carbon pool size, and fluxes of $CO_2$ and $CH_4$. It discusses carbon stocks and fluxes separately for peatlands (organic soil wetlands) and mineral soil wetlands. The chapter also discusses lateral carbon fluxes from terrestrial wetlands to aquatic/coastal systems. The main findings are that terrestrial wetlands continue to be a large carbon reservoir, and they have been a $CO_2$ sink and $CH_4$ source.

Some or most data used in the assessment were derived from the new compilation by the authors and are first presented in this chapter— a different approach from all other chapters, which mostly present and assess data and modeling syntheses in the peer-reviewed literature.

The Committee identifies several areas for improvement in this chapter, discussed below.

**Key Findings issues.** Four key findings focus on wetland area and carbon stocks, $CO_2$ sequestration and $CH_4$ emissions, wetland loss and carbon sequestration function, and future research needs. Some suggestions on improving/rephrasing the key findings include the following:

- Key findings 1-3 only present single values on wetland carbon stocks and $CO_2$ sink and $CH_4$ source, without any indication of uncertainties and range. Uncertainty statements are needed.

- Response: Metrics to convey uncertainty in the reported estimates of stocks and fluxes have been included. Those uncertainty metrics have been incorporated into the principal table (Table 13.1) and figure (Fig. 13.1), and statements in the Key Findings and elsewhere in the narrative have been revised to consider uncertainties in the reported data and along with comparisons to the literature.

- The values presented in the Key Finding 2 (18 Tg $CH_4$/per year) are inconsistent with values presented in Executive Summary (21 Tg $CH_4$/yr). Likewise, the carbon sink value of 53 Tg/yr is not consistent with the value stated in Executive Summary (non-forested wetlands 36 + forested wetlands 28 = 64 TgC/yr), or with the value of presented in Chapter 2 (36 TgC, on p.78, line 7, and in Figure 2.3). Please update the values and make sure the values are consistent throughout the report.

- Response: Consistency of the values reported both within the chapter and among chapters have been ensured.

- Findings 1 and 2 appear to be based on the new compilation by the chapter authors, as indicated in the Description of Evidence section, but without comparison to the estimates from top-down and bottom-up approaches (as presented in Saunois et al. (2016) for the period 2003-2012, among other papers. This chapter is supposed to provide an assessment of current knowledge on wetland $CO_2$ sink and $CH_4$ emissions. At minimum, the authors need to put the new estimate presented here in the context of what is available in the peer-reviewed literature. Another concern is that the values derived by the chapter authors do not correspond with a clear time period, as these values represent the mean of various individual measurements collected from different wetland sites over different time periods.

- Response: The approach for this chapter was to synthesize measurement-based studies in conjunction with updates in publicly available databases to assess carbon stocks and fluxes, thereby paralleling SOCCR1. While we had included comparisons with the other synthesis and modeling results (e.g., Saunois et al. 2016), we have embraced this comment to put the measurement-based information in better context with reports from large-scale top-down and bottom-up modelling reports. Accordingly, the narrative has been revised to incorporate a broader range of simulation studies as a basis for comparison. With respect to the reported values:
  The flux estimates are based on reported values since the 1980's or 1990's; these values are from intact wetlands, representing site conditions over a relatively short period of time (e.g., 1-5 yrs.). There is considerable variability among the reports due to the wide range in conditions; hence segregation of the reports by date would only exacerbate the large variability. These measurements are inherently variable; hence our goal is to capture that variability by utilizing applicable data. Accordingly, the reported fluxes reflect the estimated mean annual flux integrated with the updated wetland inventory, which is consistent with the reporting period.
  - The reported wetland area reflects updates to the wetland inventory in the US, Alaska, Canada and México since the last reporting period. These data represent the accepted basis for the end of the SOCCR2 reporting period.

- Chapter 2 does not cite the $CH_4$ results from this chapter, but instead summarizes the results from both top-down and bottom-up approaches with appropriate uncertainty ranges as in Saunois et al. (2016).

- Response: The reported emissions from this chapter have been reported to the Chapter 2 team. Also, the Saunois et al. (2016) was used in Chapter 13, and the revision incorporates additional references that provide additional context.

- Key Finding 1 of Chapter 9 (Forests) states that net carbon uptake by North American forests is 217 TgC/yr, with 80% in the U.S. This chapter (Wetlands) claims a carbon uptake by forested wetlands of 39 TgC/yr (Table 13.1). It is not clear if forested wetlands are included in the Chapter 9 uptake estimates. The authors need to coordinate with Chapter 9 authors and state clearly what is included in which chapter. Furthermore, it is not clear if $CO_2$ fluxes associated with peatland fires (p.510, lines 31-35) are included in these estimates. Similarly, there could be a link with Chapter 5 (Agriculture) in the discussion about wetlands for agriculture.

- Response: The narratives of Chapter 13, and 9 have been updated to clearly reflect the overlap with respect to reporting of forest lands.

Disturbance regimes are not included in the annual estimate of reported fluxes in Table 13.1 or Fig. 13.1; those fluxes represent natural / undisturbed conditions. We did not include them in the presentation of the annual emissions because those disturbance are quite variable in terms of the occurrence, extent, and magnitude. However, the effects of disturbance regimes such as peat fires and land use conversion are addressed in the chapter, and literature reports are used to convey the estimates of the consequences in terms of changes in the stocks or fluxes of $CO_2$.

**Data compilation issues.** Key data representation and data quality control issues related to this chapter, include the following:

- The Committee has concerns that 11 measurements of $CH_4$ emissions spanning three orders of magnitude, and two values on NEE from MN, WI, WV, MD and West Siberia (Table 13B.1), are averaged together to yield representative fluxes for forested peatlands in Canada and Alaska.

Response: The number of studies reporting NEE were relatively few, especially for forested peatlands. The estimate of NEE for forested peatlands in Alaska and Canada was recalculated using indirect approaches based on reports. For Canada the NEE incorporates the estimate used in SOCCR-1, which is essentially the soil increment plus the reported mean for Canadian forests (0.31 Mg ha-1 $yr^{-1}$) reported by Stinson et al. (2011), that reflects the forest biomass but not the soil increment. For Alaska, the NEE is now based on He et al. (2016), a value that incorporates both field observation and simulation results. The $CH_4$ estimates have been improved by incorporation of additional references and have also incorporated estimates of uncertainty that provide additional basis for considering the finding and comparison with simulation studies. We have also noted, in the narrative, the need for measurements especially on the forested wetlands.

- In Table 13B.2, the 53 measurements on non-forested peatlands include wetlands with diverse hydrologic and biogeochemical conditions as well as sites from a coastal marsh, an estuary, and a tidal creek of Chesapeake Bay in VA (The Terrestrial Wetlands in this chapter should not cover tidal wetlands).

- Response: The data for inclusion in the analyses have been reviewed, and oversights such as those mentioned were corrected to ensure exclusion of any tidally influenced data.

- At least two measurements appear to be from experimental study sites (poor fen – ammonium sulfate added in MN site, and poor fen with water table drawdown at Quebec, Canada site). The $CH_4$ measurements included in the table range over four orders of magnitude, from 0.0002 to 1.2 Mg C in $CH_4$/ha/yr. How would this range translate to uncertainties for scaled-up results for North America?

- Response: The data have been reviewed to ensure that the included values come from natural / undisturbed sites; accordingly, for a reported manipulation study, the included value would be from the reference site. If the study only reports values for the disturbed site, it was excluded. The reports on annual $CH_4$ flux are indeed highly variable. We acknowledge that variability by more extensive discussion in the narrative and inclusion of uncertainty metrics in the tables and figures; this also facilitates comparisons of the measurements with simulation studies. The 95% confidence range in the reported $CH_4$ flux in North America (~45 Tg C $yr^{-1}$) is approximately 21 to 69 Tg C $yr^{-1}$; this is also conveyed in Table 13.1.

- It appears that the raw measurements/data haven't gone through quality control evaluation, and as a result, the robustness of the new data compilation results so heavily relied upon in this chapter should not be assessed in the context of SOCCR2 review. Such a new compilation would be better presented as a new study in a peer-reviewed venue, so that the site selection criteria and individual data sets used would be critically evaluated by peer reviewers.

- Response: The majority of the reported literature used to estimate the fluxes were used in SOCCR-1 and the recent IPPC Wetland Supplement. Reports subsequent to those syntheses have also been incorporated. To ensure that inappropriate studies are not included, the data have been thoroughly reviewed (see earlier comment).

The problems above on data representation and unsuitable sites were noted by simply looking at the tables. Other issues may exist in other sites/data sets. For the reasons discussed above, the Committee suggests that the authors do not present the new compilation results, but instead focus on the available information in the peer-reviewed literature (such as Saunois et al., 2016 on wetland $CH_4$ emissions).

- Response: Our approach was to follow that which was established by SOCCR-1, which reflected the need and merit of a synthesis of measurements to consider the C cycle. For the current work we have updated the basis for the flux estimates and analyzed recently published databases on wetland area and carbon stocks to provide an improved perspective on the C stocks and fluxes in North America. The presentation provides detail by soil and vegetation classes, which is particularly useful when considering interactions with management, mitigation or disturbance regimes. The intent is to publish this work in a journal, just as the basis for the wetland chapter in SOCCR-1 was published in *Wetlands* (Bridgham et. al. 2006). With respect to the error in the included estuarine study, the included data have been thoroughly reviewed and disclosed. This work is intended to complement the large-scale simulation studies providing valuable perspectives from measurements, which are ultimately needed to validate the models.

**Scientific clarity and accuracy.** Some statements in this Chapter may not be scientifically accurate. Below are a few examples, with more details offered in the line-by-line comments.

- The statement on p.503, lines 30-31 ("*In undisturbed wetlands, carbon stocks are relatively stable over time…*") is wrong. Many published papers have documented that peatlands have continued to accumulate carbon since at least the end of last ice age, so the carbon stocks continue to grow over time (e.g., Gorham, 1991; Harden et al., 1992; Loisel et al., 2014; Yu et al., 2010, among others).

- Response: We changed to say that carbon stocks are relatively stable over short time intervals to clarify (which they are).

- On p.503 lines 37-38 the statement "*Similarly, both carbon stocks and fluxes are very sensitive to disturbance,*" is redundant, as carbon stocks and fluxes are not independent of each other but closely related. A clear discussion and statement should be made earlier in the chapter that change in carbon stocks, or in any carbon reservoirs for that matter and wetlands included, would be induced by imbalance in carbon fluxes (uptake and release). A disturbance may increase carbon emissions, which in turn may affect carbon stocks, depending on other flux terms. Such a statement would guide reader to have clear understanding of the critical processes.

- Response: We clarified this point by indicating that changes in carbon fluxes lead to changes in carbon stocks. We have also re-arranged Section 13.2 as suggested below into 3 subsections, one now titled "Overview of Disturbance Effects on Carbon Pools and Fluxes." The placement of the section is still near the front of the overall manuscript.

- At the beginning of subsection 13.3, the first sentence about "rooting zone" is problematic, as Sphagnum and all mosses have no roots, while these moss-dominated peatlands (Sphagnum- dominated bogs and brown mossdominated rich fens) are widely distributed in Canada and northern U.S. states. These moss-dominated peatlands are major carbon storage and sink, but they have no roots and rooting zone.

- Response: Agree with rooting zone being problematic because of the importance of Sphagnum. We changed to upper soil layers. Also added a reference regarding similarities between wetland and upland NPP.

**Global context.** The chapter should provide a proper global context to discuss wetland carbon stocks and fluxes in North America. For example, regarding global or northern peatland carbon stocks, some seminal, recent synthesis papers (e.g., Gorham, 1991; Yu et al., 2010) are not discussed. Regarding global and North American wetland $CH_4$ emissions, many pertinent publications are not discussed (e.g., Bloom et al., 2017; Melton et al., 2013; Tian et al., 2015); Chapter 2 provides a more comprehensive synopsis of the topic (p.80).

- Response: We omitted the inclusion of the global estimate from Table 13.1 because the large-scale modeling presentations don't typically segregate the fluxes based on soil or vegetation cover. However, we agree reflection on the global perspective is important, and the consideration of the global context has been improved accordingly. Consideration of the aggregated fluxes facilitates comparison with the large-scale simulation reports; this is reflected in updates to the narrative.

**Modeling discussion.** The evidence for Key Finding 4 about the uncertainties appears to rely mostly on a 10-page USDA Forest Service report (Trettin et al., 2001) (p.525, lines 15-16). Also, the subsection 13.6.3 (*Are Current Models Adequate* ) barely mention many recent efforts on simulating wetland $CH_4$ emissions, such as models evaluated and compared in Melton et al. (2013) and Saunois et al. (2016).

- Response: Key Finding #4 has been dropped. Narrative has been revised to better reflect top-down and bottom-up models.

**Organization and writing.** Some parts of the chapter could benefit from reorganization. For example, it may be more effective to divide sub-subsection 13.2.1 into several sub-subsections— including ones that focus on *Historical Regulation* and *Policies, Change in Wetland Area, and Carbon Stocks and Fluxes*. Within this chapter, the usage of words and technical terms is inconsistent and lacks clarity.
For example, the uses of "carbon fluxes/emissions/update/release" and "annual accretion" sometimes lack clarity and at other times are used inaccurately. There is also some repetitive text within the chapter. It is clear that the chapter was written by a team of authors but hasn't been edited thoroughly. Numerous small editorial suggestions are listed in line comments below.

- Response: We revised as suggested and added the new headers including a section called Overview of Disturbance Effects on Carbon Stocks and Fluxes (instead of Carbon Stocks and Fluxes as suggested). We went through the manuscript thoroughly taking out redundancies and clarifying our terminology throughout.

**Statement of Task Questions**

- *Are the goals, objectives and intended audience of the product clearly described in the document Does the report meet its stated goals*

The goals and objectives are not clearly described in the chapter. Presumably the goals are to provide an updated assessment of available literature on carbon cycling in terrestrial wetlands; yet it appears that the authors of this chapter instead conducted their own new data analysis and relied on these new results to reach conclusions.

- Response: We clarified the goals and objective statement of this chapter, which is to provide an updated perspective on carbon cycling in North American terrestrial wetlands based on published reports and databases. Some analyses of the databases were needed to organize and summarize the published data; we took this step to base the development of Chapter 13 on the most up-to-date information; which was essentially the same approach used in SOCCR-1. The NAS and public review have provided useful perspective to provide deeper context of the synthesized measurements through more effective consideration of large-scale simulation studies. We feel this approach avails information and context that is useful, and otherwise wouldn't be available.

- *Does the report accurately reflect the scientific literature Are there any critical content areas missing from the report*

Some key findings/conclusions appear to derive from the authors' own new data analysis results, without proper assessments and comparison with abundant recent peer-reviewed literature. The chapter lacks a proper discussion of global context in terms of peatland carbon stocks and wetland $CH_4$ emissions. The model section is weak in discussing available models and simulations.

☐ Response: The key findings have been revised and updated to better convey perspective derived from the consideration of measurement data synthesized in the Chapter along with literature reports from simulation studies. Uncertainty metrics are also referenced in the discussions. Also the consideration of the global wetland resource and the modeling have been thoroughly revised.

☐ *Are the findings documented in a consistent, transparent and credible way?*

Two main findings mostly rely on the new data compilation analysis by the authors of this chapter. As there are several issues related to the data representation and data quality control, the Committee cannot fully evaluate the credibility of the results/values and findings as commented on above. A more credible, transparent assessment would consider other peer-reviewed literature (e.g., Saunois et al., 2016 and Bloom et al., 2017 on wetland $CH_4$ emissions) and provide appropriate uncertainty statements.

☐ Response: The findings have been updated to reflect the finding of the data synthesized in this chapter in conjunction with the literature. Accordingly, those findings incorporate perspectives from the large-scale simulation studies (including those papers used as examples). Correspondingly, uncertainty metrics have been included in the presentation of the primary results (e.g., Table 13.1, Figure 13.1) and in the narrative.

☐ *Are the report's key messages and graphics clear and appropriate Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively*

Some Key Findings only present results from the chapter authors' own new data compilation and analysis. For example, the "description of evidence base" for Key Findings 1 and 2 only refer to Appendices 13A and B and are not compared explicitly with the peer-reviewed literature. Also, there are many discrepancies with wetland carbon fluxes values cited in Executive Summary and Chapter 2. Many statements are inaccurate or confusing, and there is repetition of some material.

☐ Response: The findings have been updated to reflect the finding of the data synthesized in this chapter in conjunction with the literature. Accordingly, those findings incorporate perspectives from the large-scale simulation studies (including those papers used as examples). Correspondingly, uncertainty metrics have been included in the presentation of the primary results (e.g., Table 13.1, Figure 13.1) and in the narrative.
The correspondence with the Executive Summary and Chapter 2 has been reviewed and updated to reflect the material presented in the Chapter along with the relevant literature.

Tables 13.1 and 13.2 provide a useful summary of wetland data in each country and territory in North America. However, the values presented here should be consistent with the values discussed elsewhere in report (Chapter 13, other chapters, Executive Summary: see comments above). The scaling-up of results on NEE and $CH_4$ emissions presented in Table 13.1 have not been demonstrated to be credible due to the issues with data representation and data quality control of the individual measurements listed in Table Annex 13B (see general comments above). Also, Table 13.2 uses different units for $CO_2$ flux (Tg $CO_2$/yr here vs. Tg C/yr elsewhere) and $CH_4$ emissions (Tg $CO_2$e/yr here vs. Tg $CH_4$/yr mostly elsewhere).

☐ Response: The reported values in Table 13.1 were on a C basis (e.g., C-$CO_2$ or C-$CH_4$), not on a $CO_2$ basis. This reporting convention is useful to report the fluxes on a C basis to facilitate compilation of a total C balance. To facilitate comparison with other chapters and the literature, we have added another column to Table 13.1 to convey the methane flux as Tg $CH_4$ $yr^{-1}$. The units for Table 13.1 have been changed from Mt to Tg, for consistency. Uncertainty estimates are provided for Table 13.1 and Fig. 13.1 so that the reader is aware of the variability in the compiled data; that variability is also discussed in the narrative, especially in reference to the large-scale simulation results.

Figure 13.1 is unclear and inappropriate. The ranges shown for $CH_4$ emissions are fundamentally unsupported by evidence. As noted above, the raw measurements of $CH_4$ emissions presented in table Annex 13B have a range spanning four orders of magnitude, and mean values were used for scaling up to the wetland-type specific total emissions shown in Table 13.1. The scaled-up values were divided by wetland areas to derive $CH_4$ emissions per unit area, and the $CH_4$ emission ranges apparently only show the ranges from various countries/territories per wetland type as in Figure 13.1, all within one order of magnitude. The presentation vastly underestimates the uncertainties.

☐ Response: The values in Fig. 13.1 have been changed. The stocks (vegetation and soil) now represent the minimum and maximum C density (Mg $ha^{-1}$); these are derived directly from the databases used to estimate the stocks. The fluxes are the 95% confidence range (e.g., mean ± two times the standard error) from the compiled data. The specifics are also included in the Supplement.

In Figure 13.1, there is no unit specified for carbon pools, and the ranges indicated for four wetland types are large and do not seem to reflect the values presented in Table 13.1. The apparent vegetation carbon pools shown in the figure do not appear in Table 13.1. The units for carbon fluxes are Mg C/hectare/yr while Table 13.1 shows units as Tg C/yr for NEE and Tg $CH_4$/yr for $CH_4$. The graphic quality can be improved. For example, the blue wavy line near the top of soils appears to show the water table, but this is not explicitly indicated. Also, some trees appear to grow in air.

☐ Response: The units for Figure 13.1 are g C $m^{-2}$ for the stocks and g C $m^{-2}$ $yr^{-1}$ for fluxes. These units are used because they closely align with those typically reported in studies; we also wanted to provide additional perspective on the data that is not readily evident from the presentation in Table 13.1. Hence the intent of this figure was to provide a convenient basis for comparing mineral and organic soils and forests and non-forests. The suggestions regarding the graphic are appreciated, and they have been addressed in the revision.

☐ *Are the research needs identified in the report appropriate?*

The stated research needs in Key Finding 4 appear to be based mostly on an old 10-page government report (Trettin et al., 2001).

- Response: Thank you, this Key Finding has been deleted.

The stated research need on model improvement is too general to be useful and does not appear to adequately consider much pertinent literature on wetland carbon models.

- Response: This section has been revised to better consider bottom-up and top-down approaches.

The stated research need on evaluating carbon sequestration and flux differences between restored and natural wetlands is a valid one but is too narrowly focused, without adequate justification why this stands out as a key finding. For example, how would the uncertainties in these differences impact the wetland carbon pool and flux assessments

- Response: We agree that the key finding regarding wetland restoration and creation is too narrow, so we broadened out the key finding to include research needs on disturbance as well.

- *Are the data and analyses handled in a competent manner Are statistical methods applied appropriately?*

Some synthesis results and findings are presented here have not gone through normal peer review processes. More justification is needed to average $CH_4$ emission rates that vary over four orders of magnitude from individual measurements/studies for the scaling-up used in the chapter, in particular to assess how these very different values would impact the uncertainties of $CH_4$ emissions (see comments above).

- Response: Most of the studies incorporated into the current synthesis have been used in previous syntheses (e.g., SOCCR-1 and IPCC – Wetlands Supplement), although we have incorporated additional studies. The wide variation in the measurement reports reflects the conditions on the ground; that variation needs to be recognized as it has bearing on the interpretations of compiled data, as provided here, as well as to simulation results. The approach that we've taken for SOCCR-2 is the same as was used in SOCCR-1, where observational data were synthesized and reported in the report and subsequently published in *Wetlands*. The wide variation in the reported data is acknowledged and discussed in the updated narrative. Correspondingly, uncertainty metrics have been included in the primary data presentation Tables (13.1) and Figures (13.1). Reducing the uncertainties in the $CO_2$ and $CH_4$ fluxes through expanded measurements and monitoring would significantly improve the basis of the wetland C cycle and provide a much needed foundation for model evaluation.

- *Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?*

The writing and organization can be much improved. There are many instances of scientifically inaccurate statements and lack of clarity in the use of terms and concepts. (Several examples are cited earlier in other sections of this chapter review).

Response: As a result of the reviewer comments we have done considerable reorganization and clarification of the narrative throughout. We are using standardized terms and believe we have taken out all inaccurate statements.

- *Are the key findings well stated and supported by the detail provided in the chapter?*

*Key Finding 1.*

The wetland carbon stock of 178 PgC is mostly from Canada at 130.5 PgC of wetland carbon stocks, including 116 PgC in peatlands (39.3 PgC in nonforested peatlands, plus 76.7 PgC in forested peatlands), as shown in Table 13.1. This peatland carbon stock in Canada is very different from the value used elsewhere—such as 153.7 PgC stated in Chapter 12 on P469 line 7 (cited Tarnocai, 1997). The chapter should consider all these estimates and come up with an assessment of likely range.

- Response: The country level C stocks are dependent on the estimates of the wetland area and the corresponding carbon density used to scale a country estimate. Accordingly, the variation in the reports of Canadian carbon stocks are attributable to those two factors. For example, there's a difference in the estimate provided by Tarnocai: 153.7 Tg (1998), 147.1 (2006), due primarily to a different basis in peatland area. The Canadian government reported estimate of total wetland area in Canada is approximately $1.5 \times 10^6$ $km^2$ (both peatlands and mineral soil wetlands), which is consistent with the area used in this report (Table 13.1). However, the total C stock is less than reported by Tarnocai; this is due to a lower C density resulting from a constraint used in the peatland stock reported here (150 cm soil depth). Accordingly, these differences in the reported values are discussed in the Supplement. We have also coordinated with other chapters so that the principal references for country-level data among chapters are used.

  - Need to explain how the development of a U.S. soils database would improve greatly the assessment of carbon stocks in North America, including Canada and Mexico. Was the lack of a U.S. soils database a major uncertainty previously during SOCCR1 assessment?

- Response: SOCCR-1 relied on carbon-densities derived from literature reviews and summarization of NRCS data from the STATSGO database. With the advent of the gSSURGO database, which provides spatially explicit distribution of soil types and compositional data, it is possible to have a more accurate estimate of the distribution of wetland soils and more specific information about the composition. Accordingly, we updated the chapter to convey the value of the current data, and we have incorporated estimates of uncertainty to provide context for comparison with other chapters and literature.

  - All figures/values stated here lack uncertainty statements.

- Response: Figure and tables now include explicit statements regarding uncertainty, as well as the presentation of uncertainty metrics.

  - There is an uneven and inconsistent treatment of numerical values; for instance, why focus on the global proportion of wetland area in North America, but not global percentage of total wetland carbon stocks in North America?

- Response: We have further elaborated on the reports for North America to place it in a global context.

  - Why does the last sentence focus on wetland area?

- Response: The reporting of wetland area facilities considerations of carbon as well as other ecosystem services. It was perceived that it made the narrative more accessible, given the C focus of the report.

*Key Finding 2.*

  - This key finding relies only on the authors' own new data compilation and analysis. The chapter should consider and assess other estimates in the peer-reviewed literature, as discussed above.

- Response: The finding were revised to reflect the combined evidence from the data conveyed in this chapter in conjunction with the literature.

  - These figures and values on $CO_2$ sinks and $CH_4$ sources are inconsistent with the ones in Executive Summary and Chapter 2.

- Response: The figures presented in this chapter have been provided to Chapter 2 and the Executive Summary writing teams.

  - Need some explanation of the significance of 43% and 40% mentioned here – why readers should consider this important?

- Response: Our intent was to provide some perspective regarding the contributions from different types of freshwater wetlands. The relevance of differentiating wetland types may be more useful by noting the type contributing the majority of the referenced metric. The key finding was re-worded accordingly.

*Key Finding 3.*

- It is unclear what rates were during historical times and to what historical period this refers.

Response: The finding was reworded to convey the periods implied by reference to "historical."

- The evidence for this key finding appears to derive from a 2011 government report (USFWS 2011), at least for the United States. The URL cited for the report appears to link to more than 20 web pages or documents of approximately 100 MB.

Response: That URL is for the source data, the reported values are available in a USFWS report; citation is provided.

- There is no traceable evidence presented in the Key Finding or Description of Evidence Base.

Response: The traceable evidence has been provided.

*Key Finding 4.*

- The stated research needs appear to be based mostly on an old 10-page government report (Trettin et al., 2001).

Response: This finding has been removed.

- This stated research need on model improvement is too general to be useful and does not appear to adequately consider the pertinent literature on wetland carbon models (e.g., Melton et al., 2013; Saunois et al., 2016).

Response: While the finding has been removed, the discussion of modeling the C cycle in freshwater wetlands has been revised, and now includes consideration of those references and others.

*Are there other key findings missing Any critical literature missing?*

It would be helpful for this chapter to include a key finding related to the impacts of climate change and natural disturbance (wildfires, permafrost thaw, drought and water-table drawdown) on wetland carbon fluxes ($CO_2$ sink and $CH_4$ emissions). That is, how could the flux rates stated in Finding #2 change in the future? There is abundant literature, at least on $CH_4$ emissions that can help inform projections of future wetland carbon balance under a warming climate and changing disturbance regimes.

Response: We have further developed the impacts of climate change and natural disturbance in the chapter. Sect. 13.3.5 has been revised succinctly to address implications of disturbance regimes, which draw on well-founded literature. Key Finding #2 was modified to include other disturbance regimes; this was preferable to adding another key finding.

*Are there any broader questions, such as the selection of the evidence and findings, weight of evidence, or the consistency of the application of uncertainty language?*

Evidence heavily relies on the authors' own new data compilation and analysis, and these new results have not been adequately evaluated in the context of available peer-reviewed literature. The quantitative statements in Key Findings lack uncertainty and range.

Response: The basis for the chapter has been described in responses to previous comments; we feel that presenting a synthesis of observational studies, interpreted with respect to basic wetland processes and simulation results, provides the basis for a comprehensive assessment of the cycle. Consideration of uncertainties has been provided both in the primary data table (Table 13.1) and also considered throughout the narrative.

**Line-Specific Comments** P503, Line

24-25

Perhaps state the wetland area first and then indicate the percentage of the global total. Why only indicate 42% of area, but not % of global total carbon stocks?

Response: Changed as suggested and added the percentage of the global C stocks.

P503, Line 25

It is not clear if 178 PgC represents both soils and vegetation, as Figure 13.1 appears to show vegetation/forest carbon stocks as well The value needs an uncertainty statement/range. There are two ways to distinguish wetlands: forested vs non-forested, and organic soil (peatlands) vs mineral soil wetlands. But this key finding only lists non-forested wetlands by area, and peatlands for both area and carbon stocks. There is no distinction made between soils and vegetation/biomass carbon stocks.

Response: It includes both the soil and vegetation. We have now clarified that in the statement. We also now incorporated uncertainty in Table 13.1. P503, Line30

All estimates need an uncertainty range. The stated $CO_2$ sink (53 Tg C/yr) is inconsistent with the value used in the Executive Summary (64 TgC/yr, (non-forested 36 + forested 28), p.37). Likewise, the $CH_4$ emissions value (18 Tg $CH_4$/yr) is inconsistent with the value in the in Executive Summary (21 Tg $CH_4$/yr, p.37). Also, Chapter 2 uses wetland $CH_4$ emission estimates with ranges in the peer- reviewed literature, rather than the values in Chapter 13.

- Response: The Executive Summary and Chapter 2 has been coordinated to ensure common perspective on reported values for the fluxes.

P504, Line 1-6 Quantification is needed: What is the current wetland loss rate? What is the time period for "historical rates?" Has there been any change in wetland loss rates in the last 10 years since SOCCR1? It seems imbalanced to focus on wetland loss and wetland restoration/creation without commenting on impacts of natural disturbance on wetlands and their carbon dynamics.

- Response: Deleted the "23 times less" and added the rate of wetland loss from 2004-2009 to show most up-to-date estimates. Because these estimates point key findings, did not comment on the effect of natural disturbances which is generally small.

P504, Line 7-10
This finding is too general to be useful. Either indicate the specifics/nature of improvements or delete.

- Response: We agree and  we deleted Key Finding 4.

P504, Line 13-36
Clarify wetlands definition (probably better to call the subsection "Terrestrial Wetlands").

- Response: We added the Terrestrial to the header of the section.

P504, Line 13
Perhaps begin with a description of the chapter goals and objectives—to clarify if this is an assessment of available peer-reviewed literature or a presentation of a new data analyses.

- Response: We made a brief summary of the chapter and placed it right at the beginning of the Introduction.

P504, Line 16 A general scientific definition of wetlands seems more appropriate than "The United States defines…" Would it be better to define wetlands as ecosystems that include soils and vegetation? Also, from this definition, how does one distinguish "peatlands" and "mineral soil wetlands"—based on water table/hydrology?

- Response: We would like to maintain the US EPA definition as it is a generally accepted definition in other countries as well. The differentiation between peatlands and mineral soil wetlands is a couple of lines down.

P504, Line 25 The sentences are confusing "all peatlands are… classified as wetlands in Canada." Why is Gorham et al., 2012 used as reference for definition of wetlands? Would "*Wetlands of Canada* (Canada Committee on Ecological Land Classification; National Wetland Working Group, 1988)" be a better reference?

- Response: Good point; we added this sentence with regard to Canada, but left the Gorham et al. reference for North America.

P504, Line 31-32
Peatlands are ecosystems while Histosols are soil type. They should not be used interchangeably.

- Response: Good point; we differentiated the two a few sentences before to make this distinction.

P504, Line 33
40 cm is repeated here, which may not be necessary.

- Response: We left the depth indicator of 40 cm as it is put in a slightly different context in each place it is mentioned.

P504, Line 41
Chapter 11 also discusses boreal carbon, so not only Arctic as stated. This subsection/paragraph should be rewritten in a clearer manner. Peer-reviewed scientific literature is available to cite in addition to government agency documents.

- Response: Chapter 11 is focused on the permafrost zone, which is not overlapping with this chapter.

P505-507, Section 13.2
This subsection discusses historical views on wetlands and regulation/policy, carbon stocks and fluxes, and wetland area change; yet it has only one labeled heading [13.2.1 on change in Wetland Area]. It may help to instead re-organize the paragraphs under three sub-subsections as follows:

- 13.2.1. Regulations on wetlands (including lines 6-33, P506 and lines 17-26, P505).
- 13.2.2. Change in Wetland Area

- 13.2.3. Carbon Stock and Fluxes (the paragraph on P505-506)

Response: We revised as suggested and added the new headers including a section called "Overview of Disturbance Effects on Carbon Stocks and Fluxes."

This subsection does not adequately reflect the literature, especially the paragraph on wetland carbon stocks and fluxes.

Response: We called it an overview; we did not intend to get into details on disturbance effects here. This is still part of the introductory material.

P505, Line 30-31 The statement that "*In undisturbed wetlands, carbon stocks are relatively stable over time…"* is inaccurate. Many published papers document that peatlands have continued to accumulate carbon since at least the end of last ice age, and so carbon stocks continue to become larger over time (e.g., Gorham, 1991; Harden et al., 1992; Loisel et al., 2014; Yu et al., 2010, among others).

Response: We changed to say that carbon stocks are relatively stable over short time intervals to clarify (which they are).

P505, Line 32-34
There are more data available that have been synthesized than just the single site in Roulet et al. (2007). For instance, Yu (2012) and Ratcliffe et al. (2018) both summarize net carbon balance data from several sites in Canada/North America.

Response: Again, here we are just using examples to demonstrate our point of the variation in $CO_2$ fluxes, not a complete literature review. We left it as is.

P505, Line 34-36 It is confusing to describe $CO_2$ fluxes as "$CO_2$ emissions:" do you mean C release/respiration? If so, what about C uptake/photosynthesis/GPP? Similar wording appears on line 42. Would be better to replace "emissions" with "fluxes" in this context.

Response: We agree and so we replaced emissions with fluxes throughout as fluxes better characterize the balance between uptake and release.

P505, Line 37-38 "*Similarly, both carbon stocks and fluxes are very sensitive to disturbance*." This should clarify that carbon stocks and fluxes are not independent, as a change in carbon stocks would be caused by imbalance in carbon fluxes (uptake and release).

Response: We clarified this point by indicating that changes in carbon fluxes lead to changes in carbon stocks.

P505, Line 41
Among many references available, why cite here a study (Drexler et al., 2009) on California Delta about wetland drainage impacts on wetland decomposition?

Response: San Joaquim River Delta is inland (i.e., freshwater) and has extensive peatlands, many of which have been drained and used for agriculture and are now subsiding. It is a good example.

P506, Line 1-5
References are needed here. Note this is Key Finding #3.

Response: We think the referee means lines 1-2 on page 507, regarding the rate of wetlands loss (i.e. Key Finding #3). We have added an appropriate reference from USFWS (2011) that was cited throughout the paragraph.

P507, Line 9-18
Provide discussion on oil sands exploration impact on wetlands/peatlands in Western Canada, especially since 2007.

Response: Added a reference and the areas of peatland lost to oil sands mining.

P507, Line 39
The first sentence about "rooting zone" is problematic. What about Sphagnum or other moss- dominated peatlands in Canada and northern U.S. states These moss-dominated peatlands are major C storage and sink, but they have no roots and rooting zone. This statement is not general enough as an opening sentence for the subsection; This paragraph overall is rather loose and lacks a single citation.

Response: Agree with rooting zone being problematic because of the importance of Sphagnum. Changed to upper soil layers. Also added a reference regarding similarities between wetland and upland NPP.

P508, Line 10
Change "Methane flux" to "Methane emission."

Response: We are now going to maintain the use of the flux instead of emission as flux denotes the net of both uptake and release.

P508, Line 19-21
The sentence is unclear. The sentence could mean large $CO_2$ flux dynamics, $CO_2$ uptake, or $CO_2$ release.

Response: We have revised the narrative to ensure a consistent convention of fluxes. We added a sentence in the third paragraph of the Introduction defining how we refer to flux.

P508, Line 21
Change "from the perspective of " to "considering organic and mineral soils wetlands separately"

Response: Made the change

P508, Line 22
Delete "quite."

Response: Deleted

P508, Line 32
Change "reported literature" to "reported values in the literature."

Response: Made the change

P508, Line 40
The appropriate terms here should be "$CO_2$ uptake" and "$CO_2$ release," not "$CO_2$ sequestration and emissions."

Response: Made the change

P509, Line 4-9
The authors attempt to define the net ecosystem carbon balance (NECB) concept as defined by Chapin et al., 2006, but this paragraph lacks clarity. A distinction between respiratory carbon loss and non- respiratory loss (due to disturbance) is needed.

Response: We inserted a sentence at the end of the paragraph that addresses the distinction between respiratory losses and other losses.

P509, Line 7
It should be clarified that carbon monoxide is due to fires.

Response: We added "(e.g., from fires)."

P509, 13.3.1. Peatlands C stocks and fluxes
There are several improvements needs for this sub-subsection:
-Compare their "new" estimates of C stocks and fluxes with what is in the peer-reviewed literature

Response: We compared the new estimates to Bridgham et al. 2007, which is the same as SOCCR1, in addition to other recent reports.

-Address the poor organization, lack of a reasonable global overview, and lack of proper references.

Response: Developed a new paragraph comparing North American peatlands to global peatlands and incorporated comparative statements in other sections to provide further context at the regional and global scales.

-Be more consistent use of $CH_4$ units (Tg C as $CH_4$/year on p.509, line 24 vs. Tg $CH_4$/yr in Key Finding #2).

Response: Fixed Key Finding #2 to be consistent.

-Address the lack of proper documentation of the value 20-30 gC/m$^2$/yr.

Response: Not sure what the reviewer is alluding to; the 20-30 gC/m2/yr is referenced (Manies et al. 2016).

P509, Line 11-12
The distinction of fens and bogs as described here (based on water source and pH) is incomplete and inaccurate. As this chapter is about wetland carbon, it should state the difference in dominant plants in fens (sedges, and brown mosses mostly) and bogs (dominated by peat moss Sphagnum).

Response: Added the suggested vegetation description to the sentence.

P509, Line 21-22
The 116 PgC in Canadian peatlands are inconsistent with the value of 153.7 PgC used in Chapter 12 and other literature. This difference needs to be discussed.

Response: We have cross-walked with Chapter 12.

P509, Line 19-11
It would better (here and throughout the chapter) to present the values and then indicate the percentage.

Response: We decided to keep the many percentages in the text because the values are in Table 13.1

P509, Line 24
The $CH_4$ unit is inconsistent with elsewhere in the chapter and report.

Response: We have done a global replace to make the convention $CH_4$-C when discussing values.

P509, Line 19-35
The values as presented in Table 13.1 should be discussed and compared with the peer-reviewed literature, such as Tian et al. (2015), Saunois et al. (2016), and Bloom et al. (2017) – all these references were cited and discussed in Chapter 2.

Response: The discussion of the stocks and fluxes presented in Table 13.1 has been expanded to include additional comparisons with literature estimates and uncertainties.

P509, Line 39 It is unclear what is meant by "mode of primary production".

Response: Deleted, was not necessary

The two references cited here appear to focus narrowly on specific macromolecules in peat, but this paragraph is supposed to talk about decomposition in general. Some more general discussion is needed to provide that unstated macromolecule examples.

Response: Added a sentence and reference regarding the importance of the water table on peat decomposition.

P509, Line 40
The term "carbon density" is unclear. Does this mean carbon concentration (% carbon) or soil carbon density (kg C/m2) or bulk carbon density (g/cm3)?

Response: We mean soil carbon density here. Soil was inserted throughout when discussing carbon density.

P509, Line 41
The term "peat accretion" is not commonly used outside discussion of mineral soil wetlands, such as salt marsh, as in these mineral-rich systems mineral sediment transport and deposition are important part of peat and carbon accumulation process.
"Peat accumulation" is a better term, as most or all materials are derived from dead plant litter.

Response: Changed to accumulation as suggested.

P510, Line 2-4
The sentence is confusing. The term "carbon stocks" is unclear, as the subheading (Peatlands – Carbon Stocks and Fluxes) indicates it refers to the size of carbon pools in PgC. In this context, it may be better to refer as "soil carbon density" (kgC/m2). It is so obvious the values will depend on peat depths. Also, what is the range of 200-3000 MgC/hectare represented by peat depths Finally, do these values represent North America or other geographic regions Yu (2012) provides a range of peat carbon density values from the literature that were used in peatland carbon stock estimates.

Response: We use the term stock to refer to an amount and pool as the place where C is stored. Appears the reviewer would prefer" pools." Also added general depths to the statement. Yu et al. cites many of the same studies as Kolka et al.

P510, Line 4
Time frames need to be indicated for the carbon accumulation rates of 7-300 gC/m2/yr. Also, Loisel et al. (2014) presents the synthesis carbon accumulation data from a large database from northern peatlands that discuss the change in carbon accumulation rates over time.

Response: Added "since the last glaciation" to provide the time context. Loisel et al. cite many of the same studies as Kolka et al.

P510, Line 5-6
The geographic region for the conclusion that bogs accumulate carbon faster than fens should be clearly indicated here to evaluate the relevance and applicability. Does this conclusion refer to peatlands in Finland (Tolonen and Turunen, 1996). A recent study on several peatlands in western Canada show the opposite conclusion that fens accumulate the same or more peat than bogs (Yu et al., 2014).

Response: We removed the statement because it is uncertain.

P510, Line 9-22

The terms describing $CH_4$ fluxes are confusing in this paragraph. $CH_4$ emissions are the difference between $CH_4$ production and $CH_4$ oxidation in soil column. So their uses should be clear. $CH_4$ effluxes in line 14 should be replaced by "$CH_4$ emissions."

- Response: We replaced emissions and effluxes with fluxes throughout and defined fluxes as the balance between uptake and release. P511-512, Section 13.3.2

This subsection has the similar issues as for 13.3.2 on Peatlands.

- Response: We revised accordingly, similar to 13.3.2.

P514, Line 11-13

The 52.5 Tg/yr carbon sinks are not consistent with the value stated in Executive Summary on page 37 (non-forested wetlands 36 + forested wetlands 28 = 64 TgC/yr) and with the value of 36 TgC as presented in Chapter 2 (page 78, line 7 and Figure 2.3). Also, it appears that all these individual values (NEE, $CH_4$ and DOC) use different units (Tg C, Tg CH4, or Tg DOC, respectively).

- Response: We have cross-walked with the Executive Summary to ensure consistency. And the units are all based on C and now added a column for $CH_4$ to be more consistent with the literature in Table 13.1; the notation has been clarified in the document.

P514, Line 16 The phase "carbon accretion in biomass" should be changed to "carbon accumulation."

- Response: Made the change

P514, Line 16-18

This is a too simplistic an approach to estimate peatland carbon sequestration. First, the rates of 20-30 gC/m2/yr are likely apparent rates of peat carbon accumulation, rather than actual carbon accumulation rates (see Turunen et al., 2002 and Yu, 2011 for discussion). So these rates cannot be directly used to estimate contemporary peatland carbon sequestration rates. Second, it should be indicated for what time periods (for example over the last several thousand years) these rates were derived and applicable. The apparent rates (see Loisel et al., 2014) and modeled actual rates (Stocker et al., 2017) of peat carbon accumulation show highly variable values throughout the Holocene (the last 12,000 years). The Holocene means in two recent large-scale syntheses on northern peatlands are approximately 20 gC/m2/yr (Loisel et al., 2014; Yu et al., 2010).

- Response: We provide an example using long-term rates that is not very different than the 20 $gC/m^2/yr$ (we use 25) in the papers the reviewer suggested and now provide a range in accumulation rates as suggested by another reviewer.

P514, Line 21-24

This sentence is confusing. More discussion on vegetation/biomass is needed to make this simple calculation credible.

- Response: Broke into two sentences for clarity.

P514, Line 25

The authors realize that 120% value does not make sense, because the approach described here is not scientifically reasonable.

- Response: We removed the percentage and just used the term greater. The example approach is scientifically reasonable.

P514, Line 28

Is this (13.3.5) still under subsection 13.3

- Response: Agree that this section doesn't fit under Wetland Stocks and Fluxes, so we started a new section and adjusted the following sections accordingly.

P515, Line 6-7

"Moist soil management:" The wording is awkward. Is that wet soil management?

- Response: It is the wording by the USFWS. Added quotes and a bit more description.

P517, Line 31 – P518, Line 32

The discussion in this subsection is inadequate.

- Response: We think that the section is well-written and discussed.

P518, Line 34

This statement requires a reference citation.

- Response: Added appropriate reference.

P518, Line 35
This is inaccurate. Many models as discussed in Melton et al. (2013) use observational wetland areas, rather than simulate wetland extent directly.

- Response: Revised to include both observational and modeling studies.

P518, Line 36-37
Change to "between … and …."

- Response: Made the change.

P519, Line 2
It would be useful to provide a global context of peatland and mineral soil wetland areas separately.

- Response: Added the areas in parenthesis.

P519, Line 12-13
The value of 21 Tg $CH_4$ /year is not consistent with other values used in the report (see general comments above). The top-down estimates for North America in Saunois et al. (2016) are 17-52 Tg $CH_4$/year. The 21 Tg $CH_4$/year value should be considered along with top-down and bottom-up estimates as presented in Saunois et al. (2016). These estimates focus a specific relevant time period from 2003-2012, almost identical to the decade between SOCCR1 and SOCCR2, so the values are most relevant to the SOCCR2 assessments. On the other hand, the value of 21 Tg $CH_4$/year as derived by the chapter authors have no specific time period that can be assigned.

- Response: We changed our estimate to reflect new data present in Table 13.1. Our new estimate is 45 Tg $CH_4$-C or 60 Tg $CH_4$/year, which is greater than the modeling studies, but, as stated, likely a result of the few observational studies.

P519, Section 13.6.3.
The section is very weak. See general comments above.

- Response: This section has been revised to better consider bottom-up and top-down approaches, along with the basis for validating model predictions.

P536, Table 13.1. Line 2
Change "$CH_4$ flux" to "$CH_4$ emissions." Also, the table needs to provide a global context as well, including global wetland areas, carbon stocks, $CH_4$ emissions, etc.

- Response: We are now using flux throughout as suggested by a reviewer earlier. We have not added the global information to the table although it is now discussed in the text.

P538, Table 13.2.
The units used here are inconsistent with ones in the text, such as Tg $CO_2$ per year, and Tg $CO_2$e per year for $CH_4$ flux.

- Response: The units are now Pg C throughout.

P540-560, Appendix Tables
These tables and the appendix text are not necessarily useful or appropriate for this report. Considering all the issues with the approach, data representation, data quality control, and large range of individual measurements, the authors should consider take a different approach to assess the available peer- reviewed literature.

- Response: We believe that providing a measurement-based perspective is useful, particularly with respect for the ability to assess not only the variation among different wetland types but also with respect to the high degree of uncertainty due to the relatively limited data. Accordingly, these data provide context that is relevant in comparison with simulation results, and it may also provide perspective to improve the field-based measurements that can provide a basis for model evaluation.

P541, Line 4-5
This statement is misleading, as both Dahl (2011) and this chapter use the same NWI database, so the tiny difference (of 0.09%) could be simply due to minor updates or rounding differences. Perhaps Chapter 13 can simply use the value by citing "Dahl (2011), with update NWI 2015)". Reproducing the content of the database here, even in a summarized form, may not be necessary. The level of details presented in this chapter is incomparable to other chapters in the SOCCR2 report.

- Response: We are using the same data base as Dahl. We have deleted this sentence.

P545, Line 13
Are these two estimates independent? If not, why the difference? Are they supposed to be the same?

Response: No, these are two independent estimates. The sentences have been reworded to clarify the comparison.

P546-547, Tables 13A.7 and 13A.10
These tables use different units for area ($km^2$ vs. ha). Need more consistency.

Response: Converted all to $km^2$.

P556-558
See the general comments on the data representation, data quality control and data variability/range. Similar issues may exit on mineral soil wetlands in Table 13B.4. Also, these tables use different units on $CO_2$ and $CH_4$ fluxes (Mg C/ha/year).

Response: All units are now consistent throughout using g C $m^{-2}$ $yr^{-1}$.

P504, Line 3
What does "23 times less" mean? Do you mean 1/24 here? This should be modified to avoid confusion.

Response: Deleted 23 times less and just indicated it was much less and put the percent loss in parentheses.

P511, Line 14-30
You may want to consider citing Lu et al. (2017), which synthesizes the annual carbon fluxes (GPP, ER, and NEP) for a number of wetland sites globally.

Response: Additions to the annual NEE fluxes for the terrestrial wetlands summarized by Lu et al. (2017) have been included.

P514, Line 11-13
"A net sink for atmospheric $CO_2$" should be changed to "a net carbon sink." The net ecosystem carbon balance here includes not only $CO_2$ but also $CH_4$.

Response: Made the change.

P514, Line 16-18
It would be more reasonable and informative to use the range (20-30 g C/$m^2$/year) to derive a range for the peat soil carbon annual accretion rate in Tg C, rather than a single value.

Response: Made the change, now provide a range.

P517, Line 30-32
Surprisingly this section does not quantitatively assess the historical and future trends of wetland carbon fluxes/stocks. Qualitatively assessing the potential effects of climate change on wetland carbon dynamics is certainly informative. But it would be much better if new or even existing model ensembles could be used to assess the trends of wetland fluxes/stocks.

Response: Narrative has been revised to incorporate recent simulation studies characterizing past, current, and projected trends in emissions.

P521, Line 34 – P522, Line 29
It is unclear whether the current models are adequate or not. If not, in what aspects of these models should be improved?

Response: This section has been revised to describe the adequacy (or inadequacies) of the wetland carbon models.

P522, Line 23-29
The one sentence on data issues does not seem sufficient. It is better to have a separate section on data needs. For example, it would be useful to have spatially and temporally explicit, high-resolution datasets that characterize the type, extent, and seasonal dynamics of wetlands.

Response: Agreed. This has been revised into a paragraph to address the data issues / needs, which are also critical to reducing uncertainties in the estimated fluxes.

P525, Line 11-12
"A significant carbon sink" should be changed to "a significant $CO_2$ sink" to avoid confusion because wetlands are a source of $CH_4$ (as indicated by the first half of the sentence).

Response: Deleted Key Finding 4

P525, Line 17-19
The authors should mention another source of major uncertainty - the imperfect model structure and underlying processes (or model uncertainty).

Response: Deleted Key Finding 4

P525, Line 27-29
It is certainly important to evaluate models, but the community needs to move forward by improving these models. Improving models should be explicitly mentioned here.

Response: Deleted Key Finding 4

**Chapter 14 Inland Waters**

.
CHAPTER 14 SOCCR2 Response to NAS Reviews Response Overview:

The authors of Chapter 14 would like to thank the review committee for the overall positive comments provided. In general, the NAS reviews concern centered on how the new information on carbon emissions from inland waters can be integrated into a total carbon balance of North America. In particular, the NAS identified that as written, and as presented within the Executive Summary, Chapter 14 suggests that the magnitude of the emission of $CO_2$ from inland waters is similar to the terrestrial forest carbon sink. We agree with the comment that this comparison is inappropriate and could lead to a conclusion that the terrestrial ecosystem is offset by aquatic carbon emissions and lateral flows. This is not the case, and we have removed all text to suggest that this would be the case. Furthermore, the rewriting of the ES has removed similar text, and figure ES5 that bolstered this improper comparison. We appreciate the NAS review committee identifying this point across both the ES and Chapter 14.

The authors of Chapter 14 express appreciation for the suggestion to add, as a component of Key Finding 3, the need for a concerted, and coordinated effort to establish international standard operating procedures, reference materials, and analysis protocols similar to those that have been successfully implemented within the oceanographic community. We have added specific text to both Key Finding 3, and section 14.7.2 that suggest that the inland water community learn from the ongoing efforts of the International Ocean Carbon Coordination Project, supported by science agencies in participation nations that has established both SOP's and data management plans for ongoing carbon related measurements.

We have also added, per suggestion within the NAS review, a figure to illustrate the major components of the aquatic carbon cycle, as Figure 14.1. This makes it necessary to change the number for the previous single figure and a caption has been added to the text document.

The future integration of inland water carbon fluxes with terrestrial carbon cycling will require a new suite of data and models, outside of the science that has contributed to this report. We appreciate the acknowledgement that the past decade has produced new findings that significantly improve the understanding of carbon cycling through inland waters since the first Sate of the Carbon Cycle Report. The authors look forward to further reducing the large uncertainties that remain regarding the emissions of $CO_2$ and $CH_4$ from inland waters, establishing partnerships with collaborators from Canada and Mexico, and developing the necessary tools to test these fluxes within an ecosystem modelling environment.

CHAPTER 14 SOCCR2 Response to NAS Reviews

**Response to Line Specific Comments:**

P565, Line 15 Perhaps change "The total flux" to "The total emission."

**In response to NAs comment - we have decided to keep the language here as total flux – since this is inclusive of the emissions term. This is not only emissions. We have also added text to clarify the inclusivity of the term flux.**

P567, Line 12 Change "…suggest inland…" to "…suggest that inland …."

**We have made this change in the text**

P571, Line 18 Change to "… suggests that…" **We have made**

**this change in the text.**

P573, Line7 Change to "assuming that 25%..." **We have made**

**this change in the text.**

P567, Line 12 Change "…suggest inland…" to "…suggest that inland …." **We have made this**

**change in the text.**

P571, Line 18 Change to "… suggests that…"
**We have made this change in the text.**

P573, Line7 Change to "assuming that 25%..." **We have made**

**this change in the text.**

P574, Line 10 It is not clear what "regional" refers to, in addition to "North America." There are no other subsections, except 14.5.1 on global perspective.

**In response to the NAS review, we have removed the regional term from the 14.5 heading. This was identified as unclear, since there were no 'specific regions' being discussed. We have added Continental US Context as the only comparisons being discussed are Global, North America and the more thorough work estimating aquatic carbon fluxes from the US, both CONUS and Alaska.**

P592, Figure 14.1. This is a nice figure.

**Thank you** P565, Line 15-25 "flux … from inland waters across the coterminous U.S. …" could be interpreted as lateral flux. Also, it is not clear (until later) whether flux refers to a source or a sink here. Please restate the first sentence of both Key Findings 1 and 2.

**In response to NAs comment - we have decided to keep the language here as total flux – since this is inclusive of the emissions term. This is not only emissions. We have also added text to clarify the inclusivity of the term flux.**

P565, Line 28 Per meter2 of what? Inland water? Or of continental area?

**In response to NAS review, we have clarified the areal flux to ensure that this is meant to reflect continental area, inclusive of both aquatic and terrestrial ecosystem area.**

P572, Line 40 Regarding the increase in discharge: how about reduced precipitation or droughts?

This statement has been added in response to the NAS review that called for an acknowledge of drought impacts.

*"However, future changes in precipitation that lead to regional drought will reduce the transfer of carbon from terrestrial ecosystem into the aquatic environment, while simultaneously decreasing the total area of aquatic ecosystems."*

Although the state of the science does not provide peer reviewed research on the impact of drought on aquatic carbon cycles are the national or continental scale, the reduction in surface waters and water flows will reduce aquatic carbon cycling across landscapes.

P573, Line 16-18 "The rate of change" refers to the pCO2 increases in 6 lakes or decreases in 3 lakes. Also, recovery from acid rain should increase pH, and hence decrease pCO2.

**We have clarified this sentence to read:**

**"***The rate of change in both the positive and negative direction was found to be in excess of 12 µatm per year, well outside the rate of increase in the atmosphere. Increasing trends in these lakes were attributed to basin-scale recovery from acid precipitation, resulting in an increase in soil $CO_2$ production in systems with little buffering capacity where $CO_2$ can be a large contributor of inorganic carbon exported from the catchment as well as changes in DOC concentrations, export and remineralization rates within the lake environment."*

Page 577, Line 16 Add reference to McKinley et al. (2011). This paper is referenced in Chapter 19 and should also be here, as this statement refers directly to this work.

**In response to the request from the NAS review to cite this sentence with McKinley et al 2011 – we have decided not to include it. The authors do not understand the relevance of the cited material to this identified gap in knowledge.**

Page 577, Line 33 - Could add a reference to Baehr and DeGrandpre (2002) to illustrate that probes have been around for a while.

**In response to the NAS review, we have added the following citation to the statement:**

**"**One major methodological advancement in past years is *in situ* probe systems (Baehr and DeGrandpre, 2004)**"**

**Baehr, M. M., & DeGrandpre, M. D. (2004). In situ $pCO_2$ and $O_2$ measurements in a lake during turnover and stratification: Observations and modeling. Limnology and Oceanography, 49(2), 330-340.**
**doi:doi:10.4319/lo.2004.49.2.0330**

.

1 **NAS COMMENTS Chapter 15: Tidal Wetlands and Estuaries**

**Overview/Main Issues**

This chapter synthesizes the latest scientific information on the carbon budget and dynamics of estuaries and tidal wetlands. The chapter provides an excellent summary of the state of knowledge in these coastal systems and the significant challenges posed by knowledge gaps. The Committee provides several ways that the chapter could be improved, including balancing the chapter with more estuarine information, including methane fluxes where possible, and expanding to include information from Hawaii, Pacific Islands, and Puerto Rico. The Committee also recommends expansion of Key Findings to reflect estuarine information and gaps in knowledge.

**Statement of Task Questions**

· *Are the goals, objectives and intended audience of the product clearly described in the document Does the report meet its*

*stated goals*

This chapter is in alignment with the overall goals and objectives of the report as stated in the Executive Summary and Report Preface. This chapter is generally well written, is scientifically sound, and provides an excellent, thorough review of carbon cycling in tidal wetlands and estuaries.

**RESPONSE: We appreciate the review.**

· *Does the report accurately reflect the scientific literature Are there any critical content areas missing from the report?*

The chapter provides ample guidance on processes in tidal wetlands but is in need of additional information on estuaries. For example,

- The majority of the "Findings" are focused on tidal wetlands, not estuaries.

**RESPONSE: We agree and have updated Finding 2, and now the 5 Findings are equally distributed.**

- Methane production is covered for tidal wetlands, but not estuaries – the chapter is in need of an introduction to processes that produce methane in estuaries and quantification of fluxes.

**RESPONSE: Given the limited data availability overall, we have updated the methane discussion in section 15.7 subsection "Other Greenhouse Gases" to include estuarine methane emission understanding and predictions. We discuss necessary methodological advances for constraining $CH_4$ release from estuaries, as per Borges and Abril (2011). There is very little information on CH4 release (5 studies known to date), and when examined globally these rates appear to be very small compared to other ecosystems (<1% of emissions).**

- "Key Areas" on page 612 (bulleted list) also focus primarily on tidal wetlands.

**RESPONSE: We agree and now include a key research area as "Estuarine gas flux monitoring".**

The section on Pacific Coast estuaries needs a more thorough discussion of "low inflow" estuaries such as Tomales Bay, Elkhorn Slough, Newport Bay, that are very common on the West Coast of the U.S.

**RESPONSE: This actually is discussed in the Pacific Coast section, and now is highlighted as "lagoonal" systems.**

The only really thorough treatment of methane source/sink is in the Pacific Coast Estuaries section; the Committee recommends the expansion of discussion of $CH_4$ in other sections where possible.

**RESPONSE: We have now removed this methane discussion from the regional section (15.3). Methane dynamics are discussed now in sections 15.4-15.7.**

While this chapter focuses on the continental U.S., the Executive Summary of the report indicates that Hawaii, Pacific Islands, and Puerto Rico are part of the report's purview. If that is indeed the intended scope, the report should incorporate known information on tidal wetlands and estuaries from these locations in both the text and the figures.

**RESPONSE: We have included general discussion of processes in section 15.2 but with limited availability of C flux data and mapped products on North American tropical coasts (Hawaii, Pacific Islands, Puerto Rico) we are not able to include quantitative discussion of these subregions. This is now stated in the text.**

The discussion on p597 about why estuaries are unique systems to understand carbon cycling and acidification provides the basis for why some scientists refer specifically to this problem as "estuarine acidification". The authors may want to specifically use this term and explain why these processes are distinct from open ocean acidification that is dictated purely by influx of anthropogenic $CO_2$.

**RESPONSE: We agree and have included a discussion of "estuarine acidification" in section 15.2, with more local implications of eutrophication and large amplitudes of "natural" pH changes.**

· *Are the findings documented in a consistent, transparent and credible way?*

The Findings are generally appropriate and are in alignment with the supporting text. However, they are primarily focused on tidal wetlands.

**RESPONSE: We agree and now we have included estuarine sedimentary C burial in Finding 2, creating parity in the treatment of Tidal Wetlands and Estuaries.**

Finding 4 could be expanded to two bullets, one for tidal wetlands and one for estuaries.

**RESPONSE: We disagree, as the point of Finding 4 is to illustrate the difference between tidal wetlands and estuaries.**

The chapter would benefit from an additional Key Finding focused on research needs/gaps (for example, as seen in Chapter16)

**RESPONSE: We have now included a 5$^{th}$ Key Finding, which points out the most pressing observational and regional data gaps reported herein, and similar to what is reported in the CCARS 2016 report.**

An additional finding could focus on the loss of these critical habitats and the importance that has for the magnitude of the coastal carbon sink.

**RESPONSE: We disagree, because the uncertainty of coastal soil C vulnerability (erosion and emission) is so large we don't think it rises to the level of a Key Finding.**

It is unclear why the authors rate Key Finding 2 with "high confidence" but "likely".

**RESPONSE: We now clarify that we report high confidence but only "likely" for our accounting procedure, because the estimates are based on syntheses of real measurements but those measurements are fewer than desired to characterize the full extent of the coastal environment.**

· *Are the report's key messages and graphics clear and appropriate Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively*

Tables are effective and clear, and figures are appropriate. The addition of one figure showing an example observational dataset would be helpful.

**RESPONSE: We agree and since we have focused primarily on $CO_2$ exchange with the atmosphere, we provide two figures with similar axes (Fig 15.5a&b) as examples of multi-annual time series of net ecosystem exchange for one estuary (Neuse River, NC) and one tidal wetland complex (Hackensack River, NJ).**

In Table 15.1 it seems possible that Pacific Coast seagrass and tidal marsh extent have been underestimated. As this value is not well-established in the literature, we urge the authors to carefully review relevant databases to assure this is as robust an estimate as possible[1].

**RESPONSE: We clarify both in the table and in the text that the all spatial extents are likely underestimated, most especially for seagrass acreage for the Pacific Coast (Section 15.3.3).**

Figure 15.2 is not consistent with the figure in the Executive Summary that indicates that Hawaii, Pacific Islands, and Puerto Rico will be considered.

**RESPONSE: As we report only the continental coastlines, due to data limitations, we use this map to clarify the specific subregions we are referring to in the text of Chapter 15. It may not match up with the Executive Summary but we are unable to include those island coastlines in our analyses. We now include two sentences (line 38-42) in section 15.1 that clarify our exclusion of data from those regions but the applicability of our process-based discussion. We also highlight the lack of data in Hawaii, Puerto Rico and Pacific Islands as a Regional Gap in 15.7**

Figure 15.1 is very complex but is a useful and important figure.

**RESPONSE: We agree.**

The tables in the Appendix are important and provide valuable information to support the findings in the report. Can Table 15A.4 be expanded to include Pacific estuaries (see text below about further underscoring research gaps and needs if this is the case)?

**RESPONSE: We did consult our contributing authors and found one publication only that was appropriate for this Appendix table. It is now included in Table 15.A.4 but we do not scale up to a regional estimate due to the limited data available for Pacific estuaries.**

· *Are the research needs identified in the report appropriate*

Research needs should be highlighted as a Finding if possible. In general, while the Synthesis, Knowledge Gaps & Outlook section is well written, these needs could be more significantly emphasized in the report.

**RESPONSE: We have now included Key Finding 5, which describes important knowledge gaps.**

Additional information is needed on how sea level rise will impact carbon storage in these environments, and/or a discussion of the fact that this is a gap/need in knowledge.

**RESPONSE: We have included more text in section 15.6 on the marsh loss and the implications for carbon fluxes. More information has been included from Mexico, which is experiencing the greatest rates of tidal wetland loss. Also, in section 15.7 within the burial and $CO_2$ flux subsection, we discuss the importance of sea level rise as a driver of carbon fluxes depending on a given wetland's "tipping point."**

Also in Section 15.7: protection of habitats that provide carbon storage potential shows up in both Chapter 17 and the Executive Summary. The Committee recommends providing a discussion here of the value of conserving tidal wetlands and estuaries given their carbon storage potential.

**RESPONSE: We removed earlier text to avoid advocating for conservation, but we now reinsert rewritten language that clarifies the carbon storage value of avoiding coastal wetland degradation, thus more directly linking to discussions in the executive summary and Chapter 17.**

In general, because some "gaps" have been discussed in previous sections (which is appropriate), the Synthesis/Knowledge Gaps section (15.7) is slightly redundant. One way to streamline this section would be to provide a brief synthesis section and then bullet points for "gaps."

**RESPONSE: We disagree on changing this to bullet points because it is fairly compact as it is, and if we changed to bullet points it would be difficult to clarify the reasoning behind why these particular knowledge gaps are expressly reported herein.**

· *Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?*

The authors appropriately summarize and cite published analyses.

**RESPONSE: We appreciate the review.**

· *Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?*

The chapter is well written, very through, and a concise point of reference for the state of the science on Estuary/Tidal carbon cycle. The level of technicality is appropriate for an interested scientific audience (undergraduates, graduate students, or scientists from other fields).

**RESPONSE: We appreciate the review.**

**Line-Specific Comments**

P595, Line 1-3
This Key Finding could be split into two.
We disagree, please see our above rational.

P601, Line 17-28
The "low inflow estuaries" that are commonly found on the U.S. West Coast are not described and discussed here.
This has been addressed, please see the rational above.

P604, Line 18
This should be corrected to "organic rich" not "organics rich."
Thank you, this has been done. .

P612, Line 18
Key Areas should be broadened to reflect the full chapter.
This is now addressed with a final bullet for "estuarine gas flux."

P641 Line 2
The map should reflect / align with Executive Summary in including Hawaii, Pacific Islands and Puerto Rico.

This has now been addressed in the text. Please see the above rationale.
P596, Line 12
Replace "dropped" to "slowed" - to avoid .."rise dropped." DONE.

P596, Line 39-40
This topic - the loss of habitat that serves as carbon storage - could be quantified further and included as a key finding.
This has now been addressed in section 15.6 but is not included as a finding due to the above rationale.

P597, Line 1-24 This discussion summarizes why some scientist specify "estuarine acidification" processes as unique and distinct from ocean acidification. Authors may want to incorporate and utilize that term into their explanation. This has now been addressed, please see the above rationale.

P597, Line 35
Also, methane emissions could be discussed here.
Thank you, we have chosen to address methane emissions in sections 15.4-15.7 only.

P601, Line 3
All other sections in this part of the chapter use regional abbreviations (GMx, MAB, etc) but Pacific Coast does not.

Thank you, we do not feel that more acronyms will help the reader, however, we are using "SMR" and "SCB" for the subregions of the Pacific coast.

P601, Line 17
What is a "large marine ecosystem?" Does large refer to area?
Yes, it refers to area - this has now been addressed as "spatially large."

P602, Line 30-40
Thorough treatment of $CH_4$ here, could be expanded to other sections.

Thank you, we include references to the few existing methane studies in section 15.4. The state of knowledge and process is in section 15.7 but is not within the regional categories section (15.3).

P611, Line 12-39
Could an example dataset be included as a figure here to provide a time series of observational data.
This has now been addressed in Figures 15.5a and 15.5b.

P613, Line 32

Some of this section is redundant with previous material; may be able to streamline by providing a summary of "gaps" in bullets.
Thank you, we feel that the current format (short subsections) is more appropriate for the discussed, please see our above rationale.

**Chapter 16 Coastal Ocean and Continental Shelf**

.

**Responses to Reviewer Comments**
(Reviews are pasted and use black and *purple* font; responses are in blue font.)

**Overview/Main Issues**

This chapter is a thorough review of data available on carbon cycling and storage in ocean and coastal systems. In general, the summary is accurate and well supported by evidence presented here.

The Committee identified just a few ways that the chapter could potentially be strengthened.
This chapter should make clear at the outset that the fluxes presented here are total (pre-industrial/background + anthropogenic) carbon cycle; i.e. in the absence of anthropogenic perturbations, there would still be a net flux to (or from) the atmosphere, and the net flux would be balanced by lateral transports. The authors need to figure out how the findings from this chapter should be integrated into the North American carbon budget (e.g. Figure ES5), which is commonly thought of as that related to anthropogenic perturbations.

**Response:**

We agree that it is important to make clear that the Chapter deals primarily with total fluxes. We have modified the following paragraph in Section 16.2 with this in mind:

"The motivation for quantifying permanent burial of carbon and export of carbon from the coastal to the deep ocean is that both remove $CO_2$ from the atmospheric reservoir. A more relevant but harder to obtain quantity in this context is the burial or export of anthropogenic carbon. The anthropogenic component of a given carbon flux is defined as the difference between its preindustrial and present-day magnitude. Total carbon fluxes have been considered thus far and represent a superposition of the anthropogenic flux component and the natural background flux. Only total fluxes—the sum of anthropogenic and background fluxes—can be observed directly. Distinction between anthropogenic fluxes and the natural background has not been attempted in regional observational or regional modeling studies. Observation-based estimates of the global anthropogenic uptake have been made by Sabine et al. (2004) and Sabine and Tanhua (2010). Bourgeois et al. (2016) were the first to estimate coastal anthropogenic carbon uptake in their global model."

With regard to integration into Figure ES5: This Figure has been removed and replaced with a figure focusing on aquatic systems and lateral fluxes.

**Statement of Task Questions**

- *Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?*

This chapter is in alignment with the overall goals and objectives of the report as stated in the Executive Summary and Report Preface. This chapter is generally well written and is scientifically sound.

**Response:** We appreciate the comment. No action taken.

- *Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?*

The Chapter is generally accurate and is well supported by scientific literature. The Committee did find a few content areas that could be expanded or included further. For example:

- "Hypoxia is a linked process to much of what is discussed here, can it be reviewed and discussed here."

**Response:** The occurrence of hypoxia is linked to acidification because respiratory consumption of oxygen coincides with an addition of respiratory $CO_2$. This type of acidification, which is not driven by uptake of atmospheric $CO_2$, occurs in conjunction with hypoxic events along the Pacific margin, driven by upwelling, and on the northern Gulf of Mexico shelf, driven by excessive nutrient inputs. We discuss both of these phenomena in section 16.4.2:

"Intensified upwelling supplies deep water to the shelf that is rich in DIC and nutrients but poor in oxygen. Ocean acidification and hypoxia thus are strongly linked ecosystem stressors because low-oxygen, high-$CO_2$ conditions derive from the microbial respiration of organic matter (Chan et al., 2016; Feely et al., 2008; Feely et al., 2016)."

"In the northern Gulf of Mexico [..] excessive nutrient inputs from the Mississippi River result in hypoxia and eutrophication induced acidification of near-bottom waters (Cai et al., 2011; Laurent et al., 2017). Similar to the California Current System, low-oxygen and high-$CO_2$ conditions coincide and derive from microbial respiration of organic matter (Cai et al., 2011; Laurent et al., 2017). Currently, aragonite saturation states are around 2 in hypoxic bottom waters and thus well above equilibrium. Projections suggest that aragonite saturation states of these near-bottom waters will drop below saturation levels near the end of this century (Cai et al., 2011; Laurent et al., 2018)."

- Methane from coastal / continental shelf sources is not discussed. Should be introduced, explained, discussed, quantified.

**Response:** Methane is introduced in section 16.2 and discussed in each of the regional subsections in section 16.3 (see lines 26 to 34 on page 657 in 16.3.1, lines 15 to 18 on page 659 in 16.3.2, lines 21 to 26 on page 660 in section 16.3.3, and lines 3 to 11 on page 662 in section 16.3.4).

- In ocean systems, one can trace anthropogenic vs. natural carbon perturbation/ fluxes. Can a discussion of the parsing of these sources be added here?.

**Response:** To the best of our knowledge the only study where estimates of anthropogenic carbon fluxes in coastal oceans have been made is the recent global model analysis of Bourgeois et al. (2016). We provide and discuss estimates for the North American EEZ based on their model in sections 16.3.5 and 16.3.6.

- Hawaii & Pacific Islands are not discussed; nor is Caribbean (Puerto Rico should be included). These are included in the map in the Executive Summary, so should be incorporated here. Similarly, trends, fluxes and gaps in knowledge for the Arctic coastline should be further discussed.

**Response:** Estimates for the Hawaiian Islands and other islands as well as Puerto Rico are reported. They make up region 17 in Tables 16.3 and 16.4 and are now labeled in Figure 16.2. However, the budget in Table 16.5 excludes the EEZ from these islands (this is explicitly stated). We also included a new figure (16.3), which shows $pCO_2$ trends near Hawaii in comparison to the atmospheric $pCO_2$ observations from Hawaii. Section 16.3.4 is dedicated to the Arctic coastline and discusses trends, fluxes and gaps.

- There could be a more specific connection to social science added here, as many of the economic impacts on people will be felt in coastal zones (recreation, fisheries, tourism).

**Response:** Most relevant to social science and coastal ecosystem health, fisheries, and aquaculture in this chapter is ocean acidification, which is identified as one of the two main subjects of this chapter in the second paragraph of section 16.1 where we define the scope of the chapter as describing: "drivers of and trends in coastal ocean acidification," because these are "directly relevant to coastal ecosystem health, fisheries, and aquaculture."

In section 16.4.2 we define ocean acidification as a threat to ecosystems and discuss projections for the Arctic and Pacific Oceans, where acidification is an immediate threat:

"Ocean acidification most directly affects marine calcifiers, organisms that precipitate $CaCO_3$ to form internal or external body structures. When the carbonate saturation state decreases below the equilibrium point for carbonate precipitation/dissolution, conditions are said to be corrosive, or damaging, to marine calcifiers. These conditions make it more difficult for calcifying organisms to form shells or skeletons, perform metabolic functions and survive."

"Model projections based on the Intergovernmental Panel on Climate Change high-$CO_2$ emissions scenario, Representative Concentration Pathway 8.5 (RCP 8.5), suggest the Beaufort Sea surface water will become undersaturated with respect to aragonite around 2025 (Steinacher et al., 2009; Steiner et al., 2014). As these conditions intensify, negative impacts for calcifying marine organisms are expected to become a critical issue reshaping ecosystems and fisheries across the North American Arctic domain (Mathis et al., 2015b; Moore and Stabeno 2015)."

"In the northern California Current System, $pCO_2$, pH, and aragonite saturation reach levels known to be harmful to ecologically and economically important species during the summer upwelling season (see Ch. 17: Consequences of Rising Atmospheric CO2; Barton et al., 2012; Barton et al., 2015; Bednaršek et al., 2016; Bednaršek et al., 2014; Feely et al., 2008; Feely et al., 2016; Harris et al., 2013)."

- *Are the findings documented in a consistent, transparent and credible way?*

The Findings appear to be in alignment with the supporting text.

**Response:** No action taken.

- *Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?*

Tables are effective and clear, and figures are appropriate.

**Response:** No action taken.

The addition of one figure showing an example observational dataset from the coastal ocean would be beneficial.

**Response:** We included two new figures one showing $pCO_2$ trends in the atmosphere and surface ocean near Hawaii (**Figure 16.3**), and surface-ocean pH trends near Hawaii and from a coastal site in the North Pacific (**Figure 16.4**). These figures illustrate a couple of important points: 1) surface ocean $pCO_2$ and pH are keeping pace with atmospheric trends in the open ocean, and 2) the coastal site in the North Pacific has a much faster rate of increase in pH due to the intensifying upwelling bringing more carbon-rich and oxygen-poor deep waters onto the shelf.

The map needs to align with the Executive Summary regarding Pacific Islands and the Caribbean.

**Response:** Hawaiian Islands and Caribbean are on the map and are now properly labeled.

Key Finding 2: What does "high confidence" apply to? The fact that the number is not well constrained

**Response:** The confidence statements were removed because they were deemed unnecessary. It is felt that the sentences sufficiently express the level of confidence.

- *Are the research needs identified in the report appropriate?*

The research needs and gaps were appropriate. The Committee expressed some concern that these and other key findings were not appropriately covered in the Executive Summary, so the authors should consider aligning with Executive Summary text.

**Response:** The Executive Summary has been revised accordingly.

- *Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?*

The authors appropriately summarize and cite published analyses.

**Response:** No action taken.

- *Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?*

The chapter is well written, very through, and a concise point of reference for the state of the science on coastal carbon cycling. The level of technicality is appropriate for an interested scientific audience (undergraduates, graduate students, or scientists from other fields).

**Response:** No action taken.

- *What other significant improvements, if any, might be made in the document?*

While Chapter 1 (Key Finding 2) states that global average temperature increase is found to be 0.85 degrees C, Chapter 16 avoids mentioning ocean temperature observations, despite the fact that this is the prime driver of the observable poleward migration of marine species, and the fact that oceans take up ~95% of all anthropogenic heat generated. It would be best to add a note or disclaimer about these omissions.

**Response:** In section 16.4.1 we added text and a reference about warming trends in NA coastal waters as follows:

"Furthermore temperature trends in coastal waters around North America show complex patterns with some regions having cooled between 1982 to 1997 followed by warming from 1997 to 2013, e.g. the MAB, some regions having warmed from 1982 to 1997 followed by cooling from 1997 to 2013, e.g. the SAB and Gulf of Alaska, some regions showing consistent warming from

1982 to 2013, e.g. the NAA, and some locations showing no significant trends in the same period (Liao et al. 2015)." In the Arctic section (16.3.4)

we are commenting as follows:

"Globally, the pace of increasing air temperatures is the highest in the North American Arctic and adjacent Arctic regions, resulting in significant reductions in both summer and winter sea ice cover that profoundly affect the marine ecosystems across the northern extent of the continent (Moore and Stabeno 2015; Steiner et al., 2015)."

In the section on future trends (16.4.1) we state: "Ocean warming reduces the solubility of gases and thus directly affects gas concentrations near the surface" and

"Rates of warming clearly are faster in higher latitudes, but predicting the net effect of these warming-induced changes in the North American Arctic is not easy. Furthermore, warming in the Arctic leads to reductions in ice cover and longer ice-free periods, both of which directly affect air-sea gas exchange (Bates and Mathis 2009). Another profound effect of Arctic warming is the melting of permafrost, which leads to the release of large quantities of $CH_4$ to the atmosphere, from both the land surface and the coastal ocean (Crabeck et al., 2014; Parmentier et al., 2013)."

There are ongoing efforts by industry to re-inject below the sea floor large amounts of $CO_2$ associated with oil and gas production (wells with 30% co-produced $CO_2$ are now common). Such efforts are on the upswing and today are the industry standard. The report should perhaps acknowledge this progress, although this is not specifically a North American phenomenon.

**Response:** This chapter is concerned with the cycling of carbon in shelf waters and fluxes across its interfaces. Geological carbon reservoirs, including human manipulations of them by petroleum extraction and $CO_2$ injection, are not considered because they do not directly interact with the shelf water carbon reservoir.

Note that ocean $CO_2$ uptake and loss is not credited to any nation under IPCC $CO_2$ accounting. Ocean uptake is viewed as a "public good" — so that land-locked nations and small nations with large EEZs are treated equally in this matter. The chapter should make this distinction more clearly and avoid sending a confusing message by commingling these coastal ocean uptake terms with other North American sectors (industrial, agricultural, forestry) that can absorb carbon.

**Response:** We removed the potentially misleading statement in section 16.1 that the EEZ "is thus pertinent to carbon accounting at the national level" and added the following clarification.

"It is worth noting here that ocean $CO_2$ uptake or loss is not credited to any nation under IPCC $CO_2$ accounting; instead, ocean uptake is viewed as an internationally shared public commons."

**Line-Specific Comments**

P653, Line 27-32
The sentence (photosynthesis in the spring, and respiration in summer and fall) could easily lead a reader to conclude (wrongly) that the biological carbon cycle is seasonal. The biological pump is much faster—with continuous grazing the residence time of phytoplankton is two weeks or less.

**Response:** We removed mention of spring and fall and rephrased the sentence as follows:

"Major internal transformations are the conversion of DIC into organic carbon (POC and DOC) through primary production, and respiration throughout the water column returning most of the organic carbon into inorganic forms, primarily DIC."

Chapter 17 Biogeochemical Effects of Rising Atmospheric CO2

.

| page-line | Comment | Reponse |
|---|---|---|
| | ... for the chapter to be useful, it needs to be more well-rounded in terms of considering the full impacts of rising atmospheric $CO_2$ emissions. One possible solution would be to summarize key insights from the Climate Science Special Report more significantly at the beginning of this chapter, and to build the discussions from there. The authors may also consider changing the title of this chapter to “Direct Consequences of Rising Atmospheric $CO_2$ on the Biosphere"=,” to contain the expectations of the reader. | Thank you for the comment. We have attempted to improve the utility of the chapter by 1) modifying the expectations by changing the title 2) adding a clear section at the start of the chapter that explains the scope, and also briefly highlighting the findings of the reports mentioned by the reviewers. We have also refered the reader to Chapter 19 where the effects of $CO_2$ and Climate change are compared. |
| | However, as stated above, the Committee finds a chapter solely on $CO_2$ consequences for ocean chemistry and terrestrial vegetation—without considering the inevitable broader consequences of climate change, sea level rise, changes in ecosystem structure etc.—to be not very useful for policy makers. | Thank you for the comment. The rationale for the chapter was to highlight the important biological and chemical effects of rising $CO_2$ on terrestrial and ocean systems. We have taken steps (described above) to make the chapter purpose clearer and more useful. |
| | The section labelled “Limits in Ocean $CO_2$ uptake” does not actually address that topic (this is further addressed in another chapter, but should be summarized here). | We have added some new text to this paragraph ". Models show that the suite of ocean changes (e.g., circulation, biological productivity, ventilation) associated with atmospheric $CO_2$ absorption and the thermal effects of $CO_2$ and other greenhouse gases on the oceans is likely to decrease the oceans’ future ability to take up atmospheric $CO_2$ (See Ch. 19, Section 19.6.1 ) " |

| | | |
|---|---|---|
| | The chapter needs an expanded view of biological impacts of ocean acidification and $CO_2$ increase (not limited to calcification). | The section "Changes in Ocean Biology and Ocean Biological Processes," has been modified to reduce emphasis on calcification and emphasize other biological impacts on marine organisms - for example: "Population-scale projections of ocean acidification's effects have been developed for a few high-value, intensively managed single species fisheries, including Tanner crab (Punt et al. 2016) and sea scallop (Cooley et al. 2015). More broadly, physiological and behavioral changes could alter predator-prey relationships and other species interactions, driving changes in species abundance and composition of ecological communities. Ocean acidification contributes to net loss of corals, and this destroys reef habitats and displaces associated marine communities (Hoegh-Guldberg et al. 2007)." |
| | Section discussing Geologic History is oversimplified to the point of potentially being confusing. | DRAFT: We have attempted to clarify the text based on line specific comments below. |
| | Also, the paragraph at the bottom of this page is misleading— the multi-stressor context of the geologic record is a strength, not a weakness. | We agree, and we have reorganized this passage to better point out that the existence of multiple stressors simply prevents definitive attribution of extinctions to one cause. |
| | Report should reflect this important role that the geologic record may play in understanding impacts of future change. | Thank you for the comment - the inclusion of this section was intended to highlight the importance of historical context. We have edited this section to emphasize this. |
| | Also, this section does not result in a Finding, which is inconsistent with the rest of the chapter. | This section lacks a "KEY FINDING" because it was included in SOCCR1 and is just to provide background and context. There was no chapter on biological consequences of rising $CO_2$, so that's what we focused on for the chapter and the Key Findings. |

| | | |
|---|---|---|
| | Sea level is discussed in terms of “coastal protection,” but not impacts on carbon sinks—please expand this section to discuss impacts on future carbon sinks/sources. | Thank you for the comment. After deliberation, we decided that the best place to include the impact of SLR on carbon release was in the section “Carbon Storage in Vegetation and Soils.” We added a sentence to describe how SLR could affect carbon storage and coastal acidification. |
| | The Chapter should address acidification in freshwater (not only in oceans). Though the data<br>are insufficient to offer conclusive proof of acidification trends, the basic chemistry clearly<br>indicates the potential for acidification is the same as in the oceans (Phillips et al., 2015). There<br>is growing evidence of likely ecosystem impacts (Hasler et al., 2015; Weiss et al., 2018). Data<br>are very sparse, and long timeseries are needed to understand these trends. Thus, it is important to discuss this emerging evidence in SOCCR2. | Thank you for the comment. We included a short paragraph on this under the subhead “Acidification of fresh water” to include these points. |
| | The discussion of fisheries/aquaculture impacts appears as a Finding but not extensively discussed in text. | Thank you for pointing this out. After deliberation we decided to add detail to clarify this point in the “Biodiversity” section; Barton et al. 2015 describes the fisheries/aquaculture impacts observed and was already cited. We added a stronger statement in “Food, Fiber, and Fuel provision,” pointing out that the Pacific NW example remains the primary example of OA impacts on fisheries to date. We also made sure that the wording was clarified in the key finding as per public comments and that the evidence base for that key finding was also more clear. |
| | Finding 4 needs rewording for accuracy. | This key finding has been reworded. |
| | Finding 2 is so generic as to be meaningless. | This key finding has been reworded. |

| | | |
|---|---|---|
| | Findings, generally, are not very quantitative here compared to other chapters (could use some quantification and statement of uncertainties). | We have included uncertainty language for each key finding but the nature of our KF's makes it difficult to make the statements themselves more quantitative or numerical than they are now. |

| | | |
|---|---|---|
| | The Key Findings could be re-ordered: Finding 1 and 3 on the oceans, Finding 2 on land, and Finding 4 on carbon-climate feedbacks. | After revising our key findings in response to other comments to make them more specific and reflective of the chapter content, we have decided to leave them in the current order because that reflects the order in which the ideas were covered. |
| | The level of technicality is appropriate for an interested scientific audience (undergraduates, graduate students, or scientists from other fields). The main problem is that the chapter does not directly refer to or summarize other impacts of rising $CO_2$, so as written the information is hard to interpret. | In response to this and other comments, we have rewritten the Introduction to clearly present the context of changing climate on which all carbon cycle changes happen. Where relevant in the chapter, we have also identified impacts of rising $CO_2$ that may be affected by temperature or other climate-driven feedbacks. |
| P695, Line 28-32 | We suggest using "would increase," "would likely change…" Without climate change and anthropogenic disturbances, "will" conveys too much confidence. | Thank you for the comment. This change has been made. |
| P696, Line 13-20 | Need to bring in downstream effects— increased litter and greater decomposition/respiration. | We discuss this in the "Terrestrial Consequences," section and we have added the term "downstream effects" to draw this link more clearly. "Because of these complications, whether rising $CO_2$ will lead to larger standing biomass and carbon storage is unclear, in part because of complex downstream effects (Norby and Zak 2011; Leuzinger and Hattenschwiler 2013)." |
| P696, Line 22-23 | Need to add brief statement/paragraph on the impact of $CO_2$ on ocean biota. | We have added this wording to make the paragraph content parallel. |

| | | |
|---|---|---|
| P697, Line 2 | Have "at times" been well in excess of … | This has been modified "Over geological time (i.e., the last 500 million years), atmospheric $CO_2$ levels have at times been well in excess of current $CO_2$ concentrations (see Figure 17.2)." |
| P697, Line 3 | Add something like "Human civilization (which developed approximately X thousand years ago) during a time..." | However, human civilization developed during the last 10,000 years; a time when atmospheric CO2 was never higher than it is today (Augustin et al., 2004). This is the time period cited in Olofosson & Hickler now cited in the next sentence. |

| | | |
|---|---|---|
| P697, Line 34 | Replace "rapid rise …" with "Solution of atmospheric $CO_2$ in sea water forms carbon acid..." | We have modified the sentence as follows: "Atmospheric CO2 dissolves in seawater and creates carbonic acid, which lowers pH and decreases the concentration of carbonate ions present in solution." |
| P697, Line 1-5 | The decrease in atmospheric oxygen confirms the combustion. | There does not appear to be a change requested by this comment. |
| P700, Line 28 | Define residual land sink | We have added "The residual carbon sink is carbon that is stored on land but is calculated as the remainder of other observed carbon sinks rather than observed itself." |
| P701, Line 15 | Add reference to Swann et al. (2016). | The references cited in this paragraph are direct observations of changes in water balance. The Swann reference is a modeling study and while it is consistent with these observations, we do not think that adding it adds materially to the report. |

| | | |
|---|---|---|
| P701, Line 19-28 | Need to mention water here. | We elected to illustrate the many factors that limit plant growth by referencing CO2 experiments that illustrate the interaction between N limitation and CO2 fertillization. Water and C interactions are discussed in the previous paragraph and at length in the section on crops "Food, Fiber, and Fuel Provision" |
| P702, Line 1 | Suggest title change to "Indirect thermal effects of rising $CO_2$ on ecosystems." | This section was removed and incorporated into a section at the start of the chapter to better explain the scope of the chapter and to highlight the important effects of climate change on the carbon cycle. |
| P702, Line 23 | Taken up or released by ecosystems **and the oceans**. | Changed to "Climate and rising atmospheric $CO_2$ can alter the amount of carbon taken up or released by ecosystems and the oceans." |
| P705, Line 14-27 | Burke et al. (2015b) shows nonlinear dependence of agriculture on temperature. | This paragraph has been drastically shortened in response to requests to clarify the scope of this chapter, so this section is no longer there. |
| P706, Line 6-8 | The carbon sink varies with climate change as well. | We agree. We have added a section to the start of the chapter to make sure the reader is aware of this point. |
| P706, Line 18 | Coastal wetlands as well | Coastal wetlands are indeed important for protecting coastlines, but the point of this paragraph is to mention the impact of acidification on coastal protection via effects on stony coral reefs. There isn't a significant known effect of rising $CO_2$ on the coastal protection service provided by wetlands, so we revised the subhead to say "Coastal Protection by Corals" |

| | | |
|---|---|---|
| P707, Line 1-3 | Add references to Burke et al. (2015a); Hsiang et al. (2011, 2013). | This paragraph has been deleted as part of our clarification of the chapter's scope. |

Chapter 18 Carbon Cycle Science in Support of Decision-Making

.

*Chpt 18 response to NAS review of SOCCR2*

*4/21/2018*

Chapter 18: Carbon Cycle Science in the Support of Decision-making

**Overview/Main Issues**

This chapter examines how scientific knowledge about carbon cycle dynamics is currently used, and can be more effectively used, to inform different types of decision making needs. The chapter is focused mainly on decision-making in agriculture, forestry and other land use (AFOLU); it does not consider decisions made in many other sectors and activities that affect the carbon cycle. The chapter would be strengthened by more coverage of ways that AFOLU components of the carbon cycle are integrated into broader considerations, such as models and analysis of national GHG mitigation policy that balance AFOLU measures with other types of mitigation measures.

**Statement of Task Questions**

- *Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?*

The chapter would best open with a clear statement about the decision domain it addresses (drawing from P740, lines 39-41), followed by a statement about the goals of this chapter (drawing on P741, lines 6-9). The reader should be informed that the discussion mainly concerns AFOLU. And it should be made clearer at the outset what attention is given to issues such as climate change impacts, vulnerability and adaptation, in addition to the focus on carbon fluxes, stocks, and emissions mitigation. It would be helpful in the examples used to provide some sort of explicit framework that clarifies questions such as: What information, how is it presented, and to whom Who are the decision makers What aspects of the carbon cycle do these decisions affect (sinks, sources, stocks, flows) What information do decision-makers get, need, and act upon?

*RESPONSE:*

1. *We have restructured the chapter to provide specific examples of decision-makers, clarifying the question of what information and for whom. In order to clarify the decision domain addressed, we also eliminated many of the general statements about the value of collaboration in favor of examples of user-driven research, descriptions of where userdriven research tends to occur most often, and discussion of some of the challenges user-driven research faces.*
2. *We have added a sentence at the end of the introduction to clarify the focused subject matter of this chapter.*

- *Does the report accurately reflect the scientific literature Are there any critical areas missing from the report?*

Noted below are some particular areas where additional discussion would be helpful:

**Full Carbon Cycle.** The text does not devote sufficient attention to ways that AFOLU (the main focus of the chapter) is integrated into carbon-cycle decisions, particularly those concerning mitigation of CO2 emissions. For example:

- Section 18.2.1 (*Science Support for Decision Making*) reads as if only the natural sciences are relevant to the carbon cycle – not economics and other social sciences.
- Sections 18.3.1, 18.3.2 and 18.3.3 focus on data, models and accounting for the land use components of decision — even for decisions about land use that involve the energy sector and its emissions. The long list of tools (P748-749) ignores the integration of AFOLU into the overall carbon cycle, even for the decisions mentioned (e.g., P748, line 23).
- It is not sufficient to say that decision tools with cycle-wide coverage are dealt with in other reports (see P747, lines 12-20). The text should give the reader some insight into the ways that integrated assessment methods and models are used to explore the interaction of land use with other components of the carbon cycle. These issues could be addressed by adding text/references that point to the mention of integrated assessment models in Box 18.2.

*RESPONSE:*

1. *We have added a section on Integrated Assessment and an example of recent results which integrates AFOLU into the larger carbon cycle, including fossil energy. This section shows how the results can inform decision-making. However, IA typically informs national decision-making, where other sub-national, high-resolution natural resource models are most often used to inform local to regional decision-making.*
2. *We have provided examples of specific institutions that use carbon cycle science, including the types of decisions for which they use this information.*
3. *We have included a specific example of social science research that pertains directly to carbon management, analysis of stakeholder views about efforts to reduce deforestation in Mexico. We have also pointed to analyses of the business case for managing carbon, highlighting both the role for economic and financial research as well as the need for partnerships between diverse institutions to create usable, actionable information.*
4. *We have expanded our list of how researchers have contributed to carbon management to include examples from economic and social science analysis.*

**Biofuels.** The discussion of biofuels (P745) — a key U.S. national decision area — could do a much better job of explaining the interactions and feedbacks in biofuels development, and the multiple disciplines that are relevant to decision support. For example, this could include:

- more explicit mention of biofuels production effects on agriculture, grasslands and forestry, and influence on food prices;
- technology issues (e.g., cost of cellulosic ethanol; non-AFOLU fuels such as algae; use of direct air capture to produce synthetic hydrocarbon fuels)
- consideration of the fossil fuels used in biofuel conversion, and their emissions
- international trade in biofuel products (e.g., forest products, Brazilian ethanol), which are integrated into national models of energy and emissions (see comment above).

Also, given that this section (18.2.3) is on “Examples . . . for Decision Making”, the discussion could be cast in the context of decisions about U.S. ethanol policy.

*RESPONSE: We have added a phrase regarding the complexity of land management and land-use change decisions associated with biofuels. Already listed in the paragraph are the multiple sources of biofuels, along with the consideration of economics, energy, environment, food, and societal challenges. Citations are provided for recent analyses that document some of the more contentious issues surrounding trade-offs among agriculture, forests, energy production, and the accounting of $CO_2$ emissions.*

**Ozone Damage**. Mention should be made of feedbacks of carbon emissions (i.e., the resulting air pollution and climate effects) on ozone damage to agriculture and natural vegetation. Detailed discussion is not needed, but the effect deserves to be mentioned.

*RESPONSE: We agree with this suggestion. However, we are reluctant to get into the impacts of $CO_2$ on environmental conditions, whether it be ozone, associated pollutants that influence allergies and respiratory illnesses, climate, etc. An emissions impacts report is likely important and timely, since it seems there has been little focus on this topic since the 1970s. However, we worry that this would be a bit tangential to the current chapter material.*

**Communication.** Section 18.2.2 (Science of Communicating Science) is incomplete in that it fails to call attention to the challenge posed by intentional dissemination of misinformation about climate change and efforts to undermine the public's trust in scientific institutions. Examples of appropriate references for such a discussion might include, for instance, Anderegg et al. (2010); Farrell (2016); Supran and Oreskes (2017). The authors might also want to consider that the challenge is not only understanding how the public interprets available science, but also understanding how carbon-cycle (and climate) science can be more accessible and relevant to individual and collective decision making. If it is not possible to deal with these important issues in this report, it might be worth considering dropping this "communication" section from the chapter altogether.

*RESPONSE: We have added some text to document research analyses regarding misinformation, as this is associated with 'attitudinal inoculation" that is described in this section. We are also moving this section to the social science chapter.*

**Culture.** The discussion of knowledge co-production could be better linked to the discussions in Chapter 7 (Tribal Lands) about different forms of decision making, and different information and communication needs, in different communities. The chapter should also acknowledge the importance of factors such as cultural and economic diversity in decision making. An example is the South Florida Regional Climate Change Compact (P743, L26-38), which works because the counties "are tightly linked socially and economically." This aspect of the chapter could be enhanced by links to the earlier chapters on *Tribal Lands* and on *Social Science*.

*Response: We have attempted to constrain the discussion and findings to "carbon cycle science" in support of decision-making, as opposed to the science of decision-making itself. As such, we have moved items and text that are specific to decision-making science into the social science chapter.*

• *Are the findings documented in a consistent, transparent and credible way*
Because this chapter does not present new learning from research or empirical results, it does not lend itself well to findings akin to those of other chapters. Presumably however, the authors were required to come up with at least 4 or 5 key findings, and as a result, some of these findings seem forced or weak. Some specific notes below.

*Key Finding 1 (Co-produced Knowledge).* This finding should be edited to provide a more coherent message. (e.g., More relevant than what) It is not the case that all information relevant to decisions *must* be co-produced by the scientific community and stakeholders. The language in P743, lines 20-21 is more helpful: "*continued communication among different shareholder communities and the scientific community . . .*" Perhaps also state that collaboration can help ensure the science is relevant to decision-maker needs. The confidence statement seems odd, given that the "finding" is just a sensible proposition, not a finding based on empirical evidence of decisions made with and without coproduction of the science inputs.

*RESPONSE: As many of the authors are quantitative, experimental scientists, we share your concern on this topic. However, as noted throughout the chapter, many areas of research at the intersection of carbon cycle science and decision-making do not lend themselves well to statistical analyses. Additionally, given the broader audience for this report, it is fair to say that while some things seem to be a sensible proposition, there remains opposition or disagreement on a number of these topics as evidenced by the lack of forward movement on a number of these topics. The key finding was edited, as suggested.*

*Key Finding 2 (Integrating Human Knowledge).* The obvious point that human drivers are the main reason for study of the carbon cycle does not merit elevation to a key finding.

*RESPONSE: Human drivers are not the main reason for the study of carbon cycle science. In fact, one of the main reasons for the study and financing of carbon cycle science is to understand the function of ecosystems, atmosphere, and oceans since we derive our natural resource commodities and life from these three systems. The point is that incorporating human drivers into the study and modeling of these systems IMPROVES the models. Results indicate the usefulness of this inclusion and continued need for support.*

*Key Finding 3 (Attribution, Accounting & Projection).* The point is also obvious and does not seem to merit elevation to a key finding. Without information on carbon-cycle fluxes and their origin, there is no carbon-cycle science, much less science to support decision making.

*RESPONSE: There is a prevailing thought among some portion of the research community that attribution, accounting, and projections (i.e., economics) is not true carbon cycle science. We reject this notion and call out the fact that much of the carbon cycle science is not particularly useful for decision-making, because much of the science ignores these key components. We appreciate that this is obvious to the reviewer. We are confident that it is not to the broader community.*

*Key Finding 4 (Strong Links among Research).* Reasonable, but it is not clear what "medium likelihood" means in this context.

*RESPONSE: We have removed the likelihood estimates in our key findings.*

*Key Finding 5 (Improved Understanding).* This finding should take account of the problem of intentional programs of misinformation (see above). Also, the evidence base refers to improving communications, whereas the finding involves understanding the public: these two different parts of the problem are not clearly explained.

*RESPONSE: We added this as suggested. However, we have moved this entire section, along with the key finding and the added text, to chapter 6.*

- *Are the report's Key messages and graphics clear and appropriate Specifically, to they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?*

Messages and graphics are generally clear. The terminology in Figure 18.2 (Mode 1 and Postnormal) needs explanation, either in the text at P741 line 31 or in the caption.

*RESPONSE: Explanation in now provided in the caption.*

- *Are the Research needs identified in the report appropriate?*

Yes, but many "needs" are listed with no sense of priority. Some rough ranking in importance would be useful. Additional effort is needed to provide guidance on research related to decision making. The main references to research needs are in short phrases in Boxes 18.2 and 18.3. The authors' view of research tasks should be elaborated in the text, with discussion of how the work would contribute to particular decision-making challenges.

*RESPONSE: We appreciate the need for prioritized needs. We have included 4 boxes (18.1, 18.2, 18.3, and 18.4) to list the few high priority needs related to each section of the chapter. Given the limited number per section, we are not sure that prioritizing them would be of great value.*

- *What other significant improvements, if any, might be made in the document?*

The chapter would be improved by illustrating the use of carbon-cycle data and analysis for support of one or more specific decisions at the national level. It would be useful for the text to give more attention to issues that are particularly important in informing decision making; for instance, discussion of the "fat" upper tail of climate response and threatened damage, and the implied urgency for action.

*RESPONSE: We have tried to stay within the bounds of carbon for this chapter and the report. Broader climate research is usually covered by the National Climate Assessment.*

**Line-Specific Comments**
P739, Line 36
This should state "different from, although complementary to."
*Response: Edited as suggested.*

P740, Line 13-15
To say "optimal is most effective" is a tautology. Rewording is suggested.
*Response: Edited.*

P740, Line 5-12
The point being made in this paragraph is not clear. How is it that definitions enable eliminating gaps between science and decision making? What are the gaps and why are the Management and Technology Drivers in Figure 18.1 mostly about agriculture, when something more general is called for? Why is renewable energy or nuclear energy not mentioned (and energy transition)?

*Response: Funding sources have different definitions for "policy", "science", "basic- and applied science". These definitions are often presented as the reason for or against funding research, and yet the definitions are not agreed upon and are used at random to support or discourage research in a given area. Getting into the nuances of how funding works among funding sources is not needed here. The point has been made and documented.*

*Response: The point on figure 18.1 is valid. Management and technology drivers are intended to be examples. The final point under management drivers (Managed efficiencies in transportation, industry, buildings, and utilities) is intended to be a catch-all that would include any management strategies for fossil fuels, non-fossil fuels, or other energy sources. Also, "efficiency in fuel production and consumption" would include nuclear fuel or renewable fuels. We have edited the figure caption to clarify that these are examples.*

P741, Line 12
Explain what in particular has changed over the last decade. Is it the items on P752, lines 32-40
*Response: The authors agree that this is an ambiguous statement and have deleted it.*

P741, Line 13
Explain what is meant by "traditional science supply paradigm."

*RESPONSE: We have added a phrase for clarification. Essentially, Dilling (2007) describes this as the model for basic science where one develops the science, places it on a "loading dock", and then waits for another to pick it up. It's considered a linear pathway for research.*

P741, Line 23
Does this mean communication with economics and the other social sciences, as well as among natural sciences If so, specify. *Response: Yes. Most definitely. The sentence currently states, "researchers from other disciplines". The authors prefer not to list all current and potential disciplines of researchers.*

P742, Line 22
Add a phrase to explain "attitudinal inoculation."

*Response: We have added this. Thank you. But we have also moved this, along with the entire section, to Chapter 6.*

P743, Line 14-15
There is repetition of a citation.
Response: This is fixed as far as we know. It was not in our original manuscript but was generated during development of the public review document.

P743, Line 4-18
For this section, need to add an example of a decision where NACP was involved or relevant.
*Response: In this case, there is no example of NACP involvement in decision making. NACP is a science-based organization that is intended to communicate and organize science on the carbon cycle. In doing so, it includes agencies, NGOs, and stakeholders in the process and as collaborators on funded research. NACP organizes that science that supports decision-making; it does not support the decision-making itself. This in fact goes to the point that was made earlier: What is the definition of science, decision-making, policy, etc? Where are the boundaries? What are the expectations?*

P748, Line 12-13
It is not true that a robust process to develop projections is "relatively new." The work goes back a quarter century or more in the climate arena alone. If the authors believe this statement, the text should define "robust" and "new."
*Response: The statement is true when considering the need for consistent projections at the local to regional to global scales and integrating sub-national models with global models for decision-making purposes on carbon-related issues. The statement has been edited for clarification.*

P755, Line 2-3

Fluxes not useful for decision making - Why not? This statement is inconsistent with the finding.
*Response: Carbon cycle fluxes or estimates are not useful by themselves. Measure carbon fluxes and publish it, and it will not be very useful. Compare that to another flux, under different land management or different fossil fuel technologies or efficiencies over time, and then you have something useful for decision-making. Baselines, comparisons, and future trajectories are important. This is often lost on the experimentalist. This is the point. We have clarified the text.*

P755, Line 20-21
Statement about emissions estimates is not true.
*Response: It is generally true that most analytical comparisons of fossil fuel emissions do not include estimates of energy and emissions associated with the production, transport, and consumption of the fossil fuel. Whereas the life cycle analysis of more complex systems (e.g., managed ecosystems) do include this level of accounting upstream and downstream. However, the sentence has been deleted.*

P755, Line 12
Statement implies that carbon accounting is not done for forestry, agriculture, and fossil fuels, which is not true.
*Response: Agreed. This is misleading and has been removed.*

P757, Line 2-3
A good deal is known about this. What is needed is an understanding of how portions of the public are misled and what can be done to persuade those individuals and groups to trust scientists and scientific institutions. *Response: This section is being move to the chapter on Social Science.*

Chapter 19 Future of the North American Carbon Cycle
.

**<u>NAS Review Comments</u>**

**Chapter 19: Future of the North American Carbon Cycle**

**Overview/Main Issues**

This chapter nicely summarizes current understanding of future changes in carbon fluxes and stocks in North America and over the globe. The chapter also examines the various factors that will control future carbon fluxes/stocks such as climate, atmospheric composition, land use change, nutrient availability, and resource management. Critical carbon cycle vulnerabilities and key research needs are also identified. Finally, this chapter briefly describes the future methane cycle and the improvement of model projections. The Committee has some suggestions on how the chapter can be improved; in particular: the methane cycle deserves more attention; some tables and figures that do not seem fully relevant could be removed; the topic of research to improve terrestrial biosphere/earth system models should be included; and it would be helpful to add a summary/outlook section at the end of the chapter.

**Statement of Task Questions**

- Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?

The goals and objectives are implicitly referred to in the second paragraph of the Introduction, but it would be better to describe these more explicitly. The report meets the goals that were implicitly mentioned.

- Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?

This chapter accurately reflects the scientific literature to a large extent. However, the methane cycle (sinks as well as sources) should receive more attention.

- Are the report's key messages and graphics clear and appropriate? Specifically, do they reflect supporting evidence, include an assessment of likelihood, and communicate effectively?

Table 19.2 is good. Tables 19.1 and 19.3 do not seem appropriate for this chapter; they might be a reasonable fit for Chapter 2. Table 19.3 is incomplete particularly for the major drivers of change. The Table should include certainty bounds, and the timeframe for "future' should be specified. Figures 19.1-19.4 are not necessary or relevant to this particular chapter. These figures may be appropriate for Chapter 1 or Chapter 2. At first glance, Figure 19.5 suggests negligible trend in the different land cover categories. Could the figure be replotted to highlight the growth of urban areas?

Figures 19.8 and 19.9 are good. Perhaps even more useful to the reader however, would be additional analyses/figures related to projections of carbon stocks and trajectories of the percentage of fossil fuel emissions offset by carbon sinks (focusing on North America and stratifying by country and biome type).

- Are the research needs identified in the report appropriate?

The key research needs identified in Section 19.8 are appropriate. We suggest, however, that this section also address the topic of research to improve terrestrial biosphere/earth system models and coastal ocean biogeochemistry models. This is important given the large uncertainty in model simulations and large discrepancies among models.

- Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?

The Introduction section provides insufficient information on why we need to project the future of the North American carbon cycle. It would be good to have one paragraph focusing on the impacts of the carbon cycle on regional/global climate and one paragraph focusing on the effects of climate change (and other global change agents) on the carbon cycle. The information presented on p.776 largely looks at the global scale, but given the focus of the SOCCR2 assessment, would it not help to also offer some discussion that focuses specifically on North America, including Canada and Mexico.

The chapter ends rather abruptly. Some sort of summary/outlook section could perhaps be added at the end of the chapter.

- What other significant improvements, if any, might be made in the document Section 19.2 (Historical Carbon Cycle Changes) is not needed, as this has been covered by Chapter 1 and/or Chapter 2. Removing this section will help shorten this chapter. The methane cycle is only briefly described as a sidebar at the end of the chapter. More attention should be paid to the methane cycle, given that this chapter is about the future of the North American carbon cycle. It would also be nice to include one figure on the future projections of methane fluxes based on published data.

- Are the key findings in your chapter well stated and supported by the detail provided in the chapter

The Key Findings are well stated and supported by the detail provided in the chapter. But it may be best to switch Key Findings 1 and 2, so that fossil fuel comes first, followed by sinks. And Key Finding 4 – Line 6 - need to mention hydrologic changes.

**<u>NARRATIVE RESPONSE</u>:**

We thank the reviewers for their constructive comments, which have helped improve the scope and the overall scientific relevance of the chapter. Based on the narrative provided by NAS for Chapter 19, we have identified 5 main issues (outlined below). We have outlined the steps that we have taken to address these review comments.

(1) ***Switch the order of key findings and shorten section (including figures, tables) on historical changes in the carbon cycle*****:** We have switched the order of the Key Findings, so that fossil fuel comes first, followed by sinks. In addition, we have substantially reduced Section 19.2 (Historical changes in the carbon cycle). Associated figures (Figure 19.2 and 19.3) and tables (Table 19.1) were offered to Chapter 1. Finally, we have pointed the reader to two new reports - CCSR (NCA Vol I) and NCA4 that summarize the impacts of the carbon cycle on global and regional climate and the effects of climate change (and other global change agents) on the carbon cycle. These impacts are also discussed in detail in Chapter 17 and we have highlighted those crosslinks with Chapter 17 (wherever appropriate). We expect that all these changes address the overall concern from NAS about refining the scope of the chapter and making it more relevant to the reader.

(2) "***The chapter ends rather abruptly. Some sort of summary/outlook section could perhaps be added at the end of the chapter:"*** This is an excellent comment. Section 19.8 was intended to be a summary/outlook section for the chapter. We recognize that the title of Section 19.8 may have been misleading, hence we have renamed this section to 'Syntheses, Knowledge Gaps and Key Research Needs.' We have also made concerted efforts to align this section with the Executive Summary provided at the beginning of the report.

(3) ***Focusing discussion about the future of the land and ocean carbon cycle on North America along with presenting them in a global context*****:** In Sections 19.5 and 19.6 (now sections 19.4 and 19.5), our main focus has been to discuss the projections, from both a global and a North American perspective, based on the RCP scenarios used as part of the CMIP5 effort and the latest IPCC assessment. However, we realize that the discussions of projected global and North American changes in these sections were not made distinct and likely contributed to confusion of the reviewer(s). We have restructured the content of all sections in the chapter so that we present global results/context first and then highlight projections specific to North American. These and other changes should make it easier for the reader to identify discussions centered around the North American carbon cycle. Note that Section 19.3 was removed, and its content was incorporated into (now) Sections 19.4 and 19.5 to streamline the discussion, and avoid redundancies.

(4) ***Paying more attention to the methane cycle, given that this chapter is about the future of the North American carbon cycle*****:** We have retained the discussion on the methane cycle as a sidebar but we have bolstered the discussion by adding the following topics: (1) we clearly acknowledge the complex suite of sources and sinks that contribute to the atmospheric methane concentrations, and the large uncertainties in those source-sink terms that hamper robust projections, (2) we have referenced a newly released report from the National Academies of Sciences (*Improving Characterization of Anthropogenic Methane Emissions in the United States*, NAS [2018]) and the 2016 US 2nd Biennial Report (US Department of State 2016) to capture projections of total U.S. methane in 2020 and 2030, (3) we have added a section on the increasingly important issue of melting clathrates on ocean shelves and the need for further studies to better characterize future climate-hydrate synergy along the coastlines.

(5) ***Providing a more balanced perspective about uncertainties in land and ocean modeling***: We thank the reviewers for pointing this out. We have augmented the discussion in Sidebar 19.2 (*Improving Model Projections of Future Carbon Cycle Changes*) by addressing the topic of research to improve both terrestrial biosphere/earth system models and coastal ocean biogeochemistry models. Specifically, we have highlighted that the development of three-dimensional biogeochemical models with interactions among tidal wetlands, estuaries, sediments, and shelf waters to scale up limited observations and integrate across the land-ocean continuum is imperative to improve future projections of ocean carbon cycle changes. We have also pointed the reader to specific sections in *Chapter 15* (*Tidal Wetlands and Estuaries*) and *Chapter 16* (*Coastal Oceans and Continental Shelves*) in this report, where more detailed discussion about uncertainties related to ocean physical and biogeochemical models are discussed. Finally, we have augmented the discussion in the modeling sidebar to better articulate that the drivers of model uncertainty are different for the land and the ocean models.

**Line-Specific Comments**

P771, Line 22-25 The"";" should be replaced with ",."

**RESPONSE:** Thank you for the comment. We have made the change.

P771, Line 27 The word "that" is needed after "meaning."

**RESPONSE:** Thank you for the comment. We have made the change.

P773, Line 20-24 The discussion of CO2 fertilization should be accompanied by mention of enhanced respiration.

**RESPONSE:** Thank you for the comment. This entire paragraph has been modified and folded with the discussion in Chapters 1 and 2.

P775, Line 4-15 These are the recent and current impacts of land use/land cover change, rather than changes for the future. Thus, they are more appropriate for Chapter 1 and/or 2.

**RESPONSE:** These sentences (others like it in this section) highlight recent stats and some drivers for LU/LUC (not impacts) to set the stage for the projections. We can reference Chapters 1 and 2 for more information but will retain this snapshot text here.

We have inserted this text to address this comment: "In this section we highlight some recent land use data for readers to gauge against the projections. For more detailed discussion on current land use and land cover change, please see Chapters 1 and 2.

P776, Line 5-10 These results are for the global scale, not for North America. It would be helpful for the authors to also focus specifically on North America (by looking at Canada and/or Mexico).

**RESPONSE:** We agree with this comment. However, we would like to point out that we haven't found such projections for

Mexico and Canada similar to what is available for the US. We have added: "The Government of Canada's official 2016 emissions projections to 2030 do not include emissions or sequestrations from land use, land-use change and forestry."

P776, Line 20-27 Again, why not offer some focus on North America specifically (by touching Canada and/or Mexico), in addition to the global focus

**RESPONSE:** We have added references to both Canada and US strategies. Specifically, we have added the following lines -

According to Canada's Midcentury Strategy, "analyses show that a substantial reduction in emissions and increase in removals by 2050 is possible through measures such as changes in how we manage forests, greater domestic use of long-lived wood products, greater use of bioenergy from waste wood, and afforestation (Government of Canada, 2016). To help attain its climate goals, Mexico seeks to achieve a zero-deforestation rate by 2030 (Government of Mexico, 2015).

P777, Line 26 "In summary," should be removed, as the sentence is not a summary of the paragraphs above.

**RESPONSE:** Thank you for the comment. We have made the change.

P778, Line 3-18 As above, would be helpful to put this in the context of North America specifically, in addition to the more general global-scale context.

**RESPONSE:** In the revised version of the draft, we have made concerted efforts to highlight our findings for North America. We had reported this in the $4^{th}$ order draft but it was buried within the discussion about the global perspective.

P780, Line 1-38 Here too, this section (19.5.1) could focus on North America, in addition to the focus on the globe in general. It would also be better to use model ensembles to specifically examine the future responses of land carbon cycle to rising atmospheric CO2 over North America.

**RESPONSE:** In the revised version of the draft, Figure 19.6 shows the spatial distribution of the sensitivity of the land and ocean carbon sinks to a) rising atmospheric carbon dioxide and b) a warming climate. These are derived from multi-model simulations of seven CMIP5 models used in the IPCC AR5 (Ciais et al., 2013). These seven models were BCC-ESM1, CanESM2, CESM1BGC, HadGEM2-ES, IPSL-CM5a-LR, MIP-ESM-lR, and NorESM1-ME.

P789, Line 15-37 The growing evidence that acidification may put ecological pressure on freshwaters could be cited here (Haslr et al., 2015; Phillips et al., 2015; Weiss et al., 2018).

**RESPONSE:** Thank you for pointing this out. We have included these references and made the reader aware of the increasing impacts of acidification.

P791, Line 23 – P792, Line 21 This section lists three sources of uncertainties in models: model structure, model parameterization, model evaluation. While model evaluation is important to discuss, it is confusing to characterize model evaluation itself as a "source of uncertainty." We suggest this list instead discuss 'model inputs' as a source of uncertainty. The

SOCCR authors may wish to also consider recently published work that formally separates uncertainty into Model Structure,

Internal Variability and Scenarios (Bonan and Doney, 2018; Hawkins and Sutton, 2009; Lovenduski and Bonan, 2017;

Lovenduski et al., 2016). Scenarios could potentially be discussed as part of the "model input" category recommended above, and the authors may wish to add a paragraph on internal variability.

**RESPONSE:** In the revised version of the draft, we have made model inputs (including scenarios) as a source of uncertainty. We have re-organized the sidebar so that model evaluation does not fall under a "source of uncertainty" but rather a way to potentially reduce model uncertainty. We argue, however, that the uncertainty due to model internal variability is already covered as part of the discussion on model structure and model parameterization.

P791-792, Section 19.2 This section should be revised to be more balanced between land and ocean perspectives. In the ocean, uncertainty in the projections of ocean circulation change, in addition to biogeochemical changes, should be mentioned. **RESPONSE:** We have augmented the discussion to provide a more balanced perspective between land and ocean modeling uncertainties as well as captured the various drivers of uncertainty for ocean models. We have also pointed the reader to specific sections in *Chapter 15* (*Tidal Wetlands and Estuaries*) and *Chapter 16* (*Coastal Oceans and Continental Shelves*) in this report, where more detailed discussion about uncertainties related to ocean physical and biogeochemical models are discussed.

P798, Lines 6-12 This section does not adequately explain the expected future slowing of the ocean sink. The current discussion of changing biology in the open ocean has minimal impact on carbon uptake. References such as Randerson et al. (2015) can provide an adequate global context.

**RESPONSE:** We thank the reviewer for this comment. However, we would like to point out that this assessment report focuses on North America; hence we do not feel the need to discuss in details the open ocean carbon sink and its projected future evolution. The focus in this report is on the US and North American EEZ (Exclusive Economic Zone), which is what we have captured in this section.