Review of the Draft Climate Science Special Report

DETAILS

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CONTRIBUTORS

Committee to Review the Draft Climate Science Special Report; Board on Atmospheric Sciences and Climate; Division on Earth and Life Studies; National Academies of Sciences, Engineering, and Medicine

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Review of the Draft Climate Science Special Report

Committee to Review the Draft Climate Science Special Report

Board on Atmospheric Sciences and Climate
Division on Earth and Life Studies

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PHILIP W. MOTE (Chair), Oregon State University
SUSAN K. AVERY, Avery Consulting, LLC
BENJAMIN BOND-LAMBERTY, Pacific Northwest National Lab
ROBERT M. DeCONTO, University of Massachusetts, Amherst
ANDREW G. DICKSON, University of California, San Diego
PHILIP B. DUFFY, Woods Hole Research Center
CHRISTOPHER B. FIELD, Stanford Woods Institute for the Environment
JAMES L. KINTER, III, George Mason University
DENNIS P. LETTENMAIER, University of California, Los Angeles
LORETTA J. MICKLEY, Harvard University
DANIEL J. VIMONT, University of Wisconsin, Madison

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AMANDA STAUDT, Board Director
ERIN MARKOVICH, Senior Program Assistant

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ARLENE FIORE, Columbia University, Palisades, New York
WILLIAM B. GAIL, Global Weather Corporation, Boulder, Colorado
MARY GLACKIN, The Weather Company, an IBM Business, Washington, D.C.
LISA GODDARD, Columbia University, Palisades, New York
MAURA HAGAN, National Center for Atmospheric Research, Boulder, Colorado
TERRI S. HOGUE, Colorado School of Mines, Golden
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ROB GREENWAY, Program Associate
MICHAEL HUDSON, Senior Program Assistant
ERIN MARKOVICH, Senior Program Assistant

Acknowledgments

This report has been reviewed in draft form by individuals chosen for their diverse perspectives and technical expertise. The purpose of this independent review is to provide candid and critical comments that will assist the institution in making its published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank the following individuals for their participation in their review of this report:

CLAUDIA BENITEZ-NELSON, University of South Carolina, Columbia
GABRIELE HEGERL, University of Edinburgh, UK
BRIAN HOSKINS, Imperial College London, UK
RICHARD J. JACKSON, University of California, Los Angeles
JEAN JOUZEL, Institut Pierre Simon Laplace, Gif sur Yvette, France
JEFF SEVERINGHAUS, Scripps Institution of Oceanography, La Jolla, CA
KEVIN E. TRENBERTH, National Center for Atmospheric Research, Boulder, CO

Although the reviewers listed above have provided many constructive comments and suggestions, they were not asked to endorse the conclusions nor did they see the final draft of the report before its release. The review of this report was overseen by **Michael R. Ladisch,** Purdue University, and **Kenneth H. Brink,** Woods Hole Oceanographic Institution. They were responsible for making certain that an independent examination of this report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.

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Summary

The United States Global Change Research Program (USGCRP) is moving towards a sustained assessment process that allows for more fluid and consistent integration of scientific knowledge into the mandated quadrennial National Climate Assessment. As part of this process, the USGCRP is developing the Climate Science Special Report (CSSR), a technical report that details the current state-of-science relating to climate change and its physical impacts. The CSSR is intended to focus on climate change in the United States and to inform future USGCRP products, including the Fourth National Climate Assessment, Box 1.

The Committee to Review the Draft Climate Science Special Report ("The Committee") evaluated the draft CSSR and this document presents consensus responses to the Statement of Task questions (See the Introduction and Appendix B for the full Statement of Task). Broadly, these questions focus on determination of whether the draft CSSR accurately presents the scientific literature in an understandable, transparent and traceable way; whether the CSSR authors handled the data, analyses, and statistical approaches in an appropriate manner; and the effectiveness of the report in conveying the information clearly for the intended audience. Responses to the Statement of Task questions in this report include overarching comments that apply to the entire draft CSSR, as well as comments specific to the Executive Summary (ES) and individual chapters. A collection of line comments provided by committee members is also included in Appendix A.

The Committee commends the CSSR authors for producing an impressive, timely, and generally well-written draft report and was impressed with the breadth, accuracy, and rigor of the draft CSSR. The draft CSSR is new and significant in several ways. First, it focuses on changes in the climate system as they affect the United States. Previous reports on this topic, such as those produced by the Intergovernmental Panel on Climate Change (IPCC), have focused on global-scale changes, which may not always translate directly to climate changes occurring in the United States. Second, the report provides a synthesis of recent manifestations of continued climate change observed since the publication of the last IPCC report in 2013, including: a new global temperature record set in 2014, which was broken in 2015 and again in 2016 thanks in part to a strong El Niño event; continued decline in Arctic sea ice; and record high globally averaged atmospheric carbon dioxide which has now passed 400 ppm. Third, the draft CSSR includes several significant advancements that have been made in the science of climate change, including the rapid development of the field of extreme event attribution, and new evidence concerning the Antarctic ice sheet that raises and better quantifies the upper bounds of projected sea level rise. These recent observed changes in Earth's climate system and substantial advancements in the science of climate change underscore the importance of up-to-date assessments like the draft CSSR. The draft CSSR, by building on previous solid work and incorporating recent advances, provides a valuable update.

In this document, the Committee also provides recommendations for how the draft CSSR could be strengthened. Some notable overarching comments include:

- The key findings throughout the draft CSSR would benefit from greater inclusion of quantification statements, where possible. Values are provided for some key findings (usually related to temperature) and are effective in making the messages more impactful, but more values could be reported.
- The traceable accounts that support the key findings often contain an insufficient level of detail and should be better utilized. The "Description of Evidence Base" provided for many key findings across many chapters list citations to support the finding, but do not summarize the

BOX 1

The Front Matter "About This Report" section of the draft CSSR provides the following description of the goals and intended audience.

"As a key input into the Fourth National Climate Assessment (NCA4), the U.S. Global Change Research Program (USGCRP) oversaw the production of this special, stand-alone report of the state of science relating to climate change and its physical impacts. The Climate Science Special Report (CSSR) serves several purposes for NCA4, including providing 1) an updated detailed analysis of the findings of how climate change is affecting weather and climate across the United States, 2) an executive summary that will be used as the basis for the science summary of NCA4, and 3) foundational information and projections for climate change, including extremes, to improve "end-to-end" consistency in sectoral, regional, and resilience analyses for NCA4. This report allows NCA4 to focus more heavily on the human welfare, societal, and environmental elements of climate change, in particular with regard to observed and projected risks, impacts, adaptation options, regional analyses, and implications (such as avoided risks) of known mitigation actions.

Much of this report is intended for a scientific and technically savvy audience, though the Executive Summary is designed to be accessible to a broader audience."

evidence contained within those citations. This low level of detail makes it difficult for readers to understand the evidence base and lessens the impact of the finding.

- The draft CSSR includes many time series datasets and analyzes trends that have been observed or simulated, however the selected time periods for trend analysis are not presented in a consistent manner. The Committee recommends that the CSSR authors standardize the time periods used for the present and historical baseline, wherever possible, and include significance statements and/or ranges in values where appropriate.
- For select chapters, the Committee recommends expanding the discussion of specific topic areas, to better reflect the full breadth of literature and understanding of the subject.

The Committee appreciates the opportunity to provide recommendations for this important draft report and notes that attention to the suggestions provided here will further enhance this document and contribute positively to the foundational role the draft CSSR will play in the forthcoming National Climate Assessment.

I. Introduction

The United States Global Change Research Program (USGCRP) is overseeing the production of a technical report that details the current state-of-science relating to climate change and its physical manifestations. The draft Climate Science Special Report (CSSR) is intended to serve as technical input to the Fourth National Climate Assessment (NCA4), providing an updated detailed analysis of the findings of how climate change is affecting the weather and climate across the United States and its Territories, and reporting information and climate projections that can inform NCA4 analyses. The Executive Summary (ES) within the draft CSSR will also provide the basis for a NCA4 chapter summarizing the physical science basis for climate change. This draft report is designated as a Highly Influential Scientific Assessment (HISA).

The National Academies of Sciences, Engineering, and Medicine have a history of convening expert groups to provide independent review of USGCRP assessment reports and currently has a standing Committee to Advise USGCRP. The Committee to Review the Draft Climate Science Special Report ("The Committee") was convened in December 2016 and is composed of members with diverse climate science backgrounds that span the breadth of focus topics included in the draft CSSR.

The Committee was specifically charged with addressing the following Statement of Task questions (See also Appendix B for the Statement of Task):

- Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?
- Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?
- Are the findings documented 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?
- Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?
- Are the document's presentation, level of technicality, and organization effective?
- What other significant improvements, if any, might be made in the document?

The Committee had an opportunity to discuss the draft CSSR with the report's authors and USGCRP staff during a WebEx briefing on December 8, 2016 and reviewed the draft report concurrent with the public comment period. The Committee met in person in Washington, DC on January 9-10, 2017 to discuss the draft CSSR and had follow up discussions to reach a consensus on the Committee's responses to the Statement of Task questions. Reviews of individual draft CSSR chapters were conducted by small teams of committee members with the appropriate expertise, who then led the discussion of their comments with the full committee. The Committee reviewed the entire draft CSSR including figures, tables, and traceable accounts. This National Academies report provides a synthesis of overall recommendations and comments specific to the Executive Summary (ES) and individual chapters. A collection of line comments are also provided in Appendix A. Key findings presented in the draft CSSR that the Committee had specific comments for have been copied into this document, to provide context. As is the nature of these sorts of reviews, many of the comments recommend ways to improve the draft CSSR and the Committee offers these suggestions in the spirit of constructive criticism.

II. Synthesis of Comments on the Draft Climate Science Special Report

The Committee to Review the Draft Climate Science Special Report ("The Committee") commends the CSSR authors for producing an impressive, timely, and generally well-written report. The Committee was generally impressed with the breadth, accuracy, and rigor of the draft CSSR. The draft CSSR emphasizes the robust evidence that human emissions of greenhouse gases (GHGs) have substantially warmed the planet and are causing myriad changes to the Earth system, some of which are effectively irreversible on human timescales.

The draft CSSR draws on existing climate change assessments while also providing important new research findings and observations. Assessments of climate science are now routinely produced. Authoritative documents include the science volume of America's Climate Choices (NRC, 2012), the Working Group I contribution to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change (AR5WG1, IPCC 2013), and the climate science chapter of the Third U.S. National Climate Assessment (NCA3, Melillo et al. 2014). The draft CSSR is new and significant in several ways. First, it focuses on changes in the climate system as they affect the United States and provides a much more comprehensive evaluation of physical climate changes than was included in the climate science chapter of NCA3. Second, the report provides a synthesis of recent manifestations of continued climate change: a new global temperature record set in 2014, which was broken in 2015 and again in 2016 thanks in part to a strong El Niño event (e.g. Lean and Rind, 2008, who quantified the contribution of El Niños to global temperature); continued decline in Arctic sea[1]; and record high globally averaged atmospheric carbon dioxide (CO_2) concentration which has now passed 400 ppm[2]. Third, the draft CSSR includes several significant advancements that have been made in the science of climate change, including the rapid development of the field of extreme event attribution, which also was the subject of a recent National Academies report (NASEM, 2016a), and new evidence concerning the Antarctic ice sheet that raises and better quantifies the upper bounds of projected sea level rise (SLR).

These recent, observed changes in Earth's climate system and substantial advancements in the science of climate change underscore the importance of up-to-date assessments. By building on previous work and also by showing recent advances, the draft CSSR provides a valuable update. The CSSR will also serve as a useful resource for evaluating the implications of climate change for the United States and its territories, which will be the subject of NCA4, due for release in 2018.

II.1 OVERALL COMMENTS

The Committee agrees that the draft CSSR is largely accurate and generally represents the breadth of available literature pertaining to the state-of-the science at the time of writing, with the exception of some specific topic areas detailed in this report. Assessment reports like the draft CSSR are most effective when they convey sufficient detail using relatively simple language. This can be achieved by providing authoritative statements about the current state-of-science, which necessarily include some facts that have been well established for decades, and also recent observations and findings. Impactful assessments also

[1] See http://nsidc.org/arcticseaicenews/2016/10/rapid-ice-growth-follows-the-seasonal-minimum-rapid-drop-in-antarctic-extent/.
[2] See https://www.esrl.noaa.gov/gmd/ccgg/trends/global.html.

use scientific language that is accurate enough for the specialist to know exactly what is meant, while also being comprehensible to a broad audience. The draft CSSR generally demonstrates these characteristics, although the Committee notes below some ways that the draft report can be improved.

The draft CSSR could be strengthened by more clearly distinguishing, in the chapters and the ES, what is truly new and significant. Separating this new information from the longstanding foundational science that underpins the report would improve its impact and usability. A list of "what's new" appears at the end of the ES, but the Committee suggests that each chapter examine its key findings and find ways to delineate what is a new or significantly updated observation, a new or important line of evidence, or is simply an important and significant aspect of climate change that was already part of the foundation of the science. This emphasis could be achieved through specific language more clearly identifying which key findings are new, by reducing the amount of text devoted in key findings to long-accepted truths, by reordering the key findings, or by color-coding the text of the key findings.

The U.S. regions provided in the draft CSSR (that will also be used in NCA4) have been modified since NCA3. One result of this change is that a new Caribbean region has been created. The draft CSSR barely mentions the Caribbean and includes no results for the region that the Committee could find, apart from the maps of projected temperature and precipitation change (e.g., Figure 6.7). Any data and findings that can be provided would probably be useful to the authors of the Caribbean chapter of NCA4. If data and findings cannot be provided, that should be noted.

To strengthen the impact and message of the draft CSSR, the Committee recommends adding quantitative statements to the key findings throughout the report, where possible. Values are provided for some key findings (usually those related to temperature) and are effective in making the messages more impactful, but more values could be reported. More specific recommendations in response to the questions in the Statement of Task about data and statistics are provided throughout this report.

Throughout the draft CSSR, it would also be helpful to better link related topic areas across chapters, to provide guidance to the reader. For instance, in Chapter 10 where drought is discussed, it should be indicated that Chapter 8 covers drought in greater detail.

II.2 RESPONSE TO THE STATEMENT OF TASK

Are the Goals, Objectives and Intended Audience of the Product Clearly Described in the Document? Does the Report Meet Its Stated Goals?

The Front Matter (page 1, lines 2-13 of the draft CSSR, see also Box 1 of this report) adequately describes the goals and objectives and, with the exception of the omission of the Caribbean and other smaller examples provided later in this review, it meets those goals. The intended audience is described as follows: "Much of this report is intended for a scientific and technically savvy audience, though the Executive Summary is designed to be accessible to a broader audience." (page 1, lines 14-15 of the draft CSSR, also provided in Box 1 of this report). The Committee considers this description of the audience to be insufficiently clear. For instance, a technically savvy audience may be interpreted as those with familiarity with technological advancements, which is not necessarily equivalent to a general understanding of the physical sciences contained in the draft CSSR. As such, the Committee suggests rewording this statement as follows:

> The material presented in the chapters of this report is intended to be understood by a scientifically literate audience. The Executive Summary is designed to be accessible to a more general audience.

In some places, too many terms are unfamiliar to anyone but a specialist in the field, and in those instances the text fails to meet the goal of communicating effectively to the intended audience. Specific locations in the draft CSSR where this concern arises are noted in Chapter III of this report. Some such terms may be unavoidable, but should be explained and defined in the text or glossary. The table of contents of the draft CSSR includes a putative glossary but that glossary is missing. The Committee provides some specific words that should be considered for inclusion in a glossary and these are listed in the Line Comments (Appendix A).

Does the Report Accurately Reflect the Scientific Literature? Are There Any Critical Content Areas Missing from the Report?

The draft CSSR, in general, accurately reflects the scientific literature, with an emphasis on recent material, with the exception of some specific topic areas detailed in this review. In some instances, the Committee notes minor omissions or significant imbalances where the extent of existing literature on a given topic is not adequately cited or discussed. For instance, the treatment of hydrology in Chapter 8 needs to be more thorough. Some discussion of the concept and quantification of climate sensitivity and transient heat response would be useful to also include, perhaps in Chapter 2, where it is currently mentioned in one line. Recommendations are further detailed in Chapter III for individual draft CSSR chapters, with specific suggestions for improvements and some recommended publications to consider citing.

Are the Findings Documented in a Consistent, Transparent and Credible Way?

Most of the findings are well documented. However, the Committee provides a number of suggestions where documentation could be improved, with the most significant provided here and additional suggestions detailed in Chapter III.

The traceable accounts that support the key findings often contain an insufficient level of detail and could be better utilized. According to the draft CSSR, traceable accounts support each key finding and "document[s] the supporting evidence, process, and rationale the authors used in reaching … conclusions, and provides additional information on sources of uncertainty through confidence and likelihood statements." The description of evidence base provided in the traceable accounts for many key findings across many chapters list citations noted to support the finding, but do not summarize the evidence contained within those citations. This results in a low level of detail, making it difficult for readers to understand the evidence base and lessening the impact of the finding. This contrasts with the NCA3, in which many key findings were supported by a full page or more (in the final printed version). This issue needs careful attention throughout the report.

In some places, AR5WGI findings are cited simply as IPCC (2013). For traceability, it would be far better to follow recommended practice and cite the specific chapter, since the entire IPCC report is over 1,500 pages.

Many of the figures (specifically listed in the relevant sections of Chapter III) are presented with insufficient information on how a specific calculation was performed or which data or tools were used. This is a significant weakness, but one that should be straightforward to remedy.

Some chapters are very unevenly represented in the ES. For instance, there are 6 bullet points for Chapter 12's five key findings while no key findings from Chapter 10 are listed. This disproportionate representation might be reasonable and justified, but it is not obvious that this is the case. The Committee encourages the authors to consider whether the overall balance of the bullet points is appropriate.

The topic of extreme events should be presented with greater detail and further consideration should be given to the most appropriate metrics to report. The current approach, especially as used to construct figures, could be better connected to the peer-reviewed literature (by using widely accepted methods and considering multiple metrics). In many cases, an insufficient amount of information is provided for the reader to understand underlying methods. For example, Figure 6.3 (also included as ES.5) contains two time series (bottom panels), but the text in Chapter 6 and associated traceable accounts do not provide any details on how the spatially averaged time series were calculated. Attempts by committee members to reproduce the plot were unsuccessful. In general, because there are several possible metrics for extreme heat in the literature (e.g., Hartmann et al. 2013, page 221), the draft CSSR should assess the consistency of conclusions across metrics and present only those that fairly represent robust conclusions across studies and metrics. For heat, in addition to "Txx" (warmest day of the year), Hartmann et al. (2013) also uses Tx90p (90th percentile day), and various studies have used definitions of heat waves like highest 3-day minimum temperature, heat index, etc. Since conclusions across metrics are inconsistent in some cases, the discussions of changes in extremes should summarize the state of knowledge and describe how/whether the results depend on metrics chosen (e.g. Txx vs. Tx90p).

A related issue of clarity with regard to extremes is spatial consistency. Studies of changes in extreme precipitation at individual weather stations find a wide variety of trends (and results can depend profoundly on which metric is selected); spatially aggregating the trends to a relatively large scale does seem to result in a regionally averaged increase in extreme precipitation (e.g. Min et al. 2011 and Zhang et al. 2013) and as shown in Figures 7.3 and ES.4. But, the underlying message of the spatial complexity is not well articulated in the draft CSSR, especially when accompanied with language like "Heavy precipitation events across the United States have increased...". The Committee recommends careful consideration of the appropriate level of detail concerning spatial complexity (e.g. plotting station-level or climate-division trends), robustness across metrics (e.g. plotting multiple time series of different metrics), and traceability. These issues appear in at least Chapters 6, 7, 8, and 9.

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?

Comments on individual figures are given in Section II.3 (for the ES) and in Chapter III (for individual chapters). Some of this Committee's recommendations apply to multiple figures. See also the points made previously about clarity and supporting evidence for heat and precipitation extremes.

Some maps presenting climate model outputs use a Mercator projection that leads to a low ratio of data to map area (e.g., Figure ES.3). This results in a majority of the map consisting of information-free gray oceans and more space given to Canada than to the continental United States. Using a different projection, and including Hawai'i and Alaska (but not necessarily devoting space to place them in their correct locations), would allow the reader to learn more about changes projected for the continental United States. Also, the contour intervals used for plotting colors on the maps could be a bit finer to aid the reader. If links could be provided to online plotting tools that NCA4 authors could use, that would further increase the utility of these figures.

The Committee noticed that there are nine graduations of likelihood provided on page 4, but only five are used in the draft report, so they may not all be needed.

As with any report written by a committee, an editing pass will improve consistency and readability. Some chapters achieve excellent readability for the intended audience by minimizing use of jargon, appropriate word choices, and clear language including sentence construction. Chapters that do not read as clearly are noted in this report. The word 'robust' is in some respects a term of art with specific connotations, but is used with different meanings in different contexts in the draft CSSR. The draft also reports carbon (C) in units of both PgC and GtC which are identical, and using both units is needlessly confusing. There may be some advantages to using a CO_2 metric such as Gt CO_2 throughout, as it is consistent with that used in IPCC AR5 2013. Regardless, the Committee recommends choosing one reporting approach for carbon emissions and using it consistently throughout the CSSR.

Are the Data and Analyses Handled in a Competent Manner? Are Statistical Methods Applied Appropriately?

The previous comments above about extreme events also apply here.

In some places, time periods over which change is discussed are somewhat different. While these constraints sometimes result from citation of published literature and data records, in other cases (which the Committee tried to identify and note) they seem to be more amenable to standardization. The draft CSSR uses a metric of 20th century change defined as the 1986-2015 average minus the 1901-60 average. The Committee recommends that the CSSR authors recompute the values, where possible, using a different method, detailed next.

The Committee recommends using the following guidelines that would improve the statistical treatment of data throughout the draft CSSR, and encourages all individual-chapter authors to consistently apply this approach:

- Be clear enough about how each calculation is done that a reader could reproduce or find the reported value or plot.
- Be consistent. As much as possible, minimize differences in baseline time periods and methods (cf pages 13-14).
- Include significance statements and/or ranges as appropriate.
- When consistency is not possible, use methods or baseline time periods established in literature (e.g. IPCC 2013 uses 1850-1900 as a baseline for global mean temperature).
- When discussing rates of change, use slope-based methods (e.g. regression or Theil-Sen, that minimize end effects), rather than comparing time periods, if appropriate for the metric being discussed. Since slope-based methods incorporate all available data, they can better represent rates of change.
- Wherever possible, figures depicting observed trends should indicate the statistical significance of those trends, or confidence intervals.

If these recommendations are incorporated, the “Guide to the Report” section could then be updated to describe the statistical approaches. If the current approach is retained, the descriptors of 1901-60 should be carefully checked, as there were examples referring to it as “early 20th century” and the like.

Are the Document’s Presentation, Level of Technicality, and Organization Effective? What Other Significant Improvements, If Any, Might Be Made in the Document?

Generally, yes, the level of technicality and organization are effective. Chapter III discusses where specific chapter edits could improve the presentation, level of technicality, or organization, and where other improvements could be made.

II.3 COMMENTS ON THE EXECUTIVE SUMMARY

The ES is strong, well-written, and in most cases accurately represents the consensus and breadth of viewpoints. In this section the Committee focuses comments primarily on the figures and the “New Understanding” and “Better Tools and Approaches” sections of the ES. It is the expectation that authors will address chapter-specific comments provided in Chapter III and then edit the ES further to integrate those recommendations, along with the explicit recommendations for the ES given here.

Figure ES.1: It appears there are missing data in the Arctic and Antarctic, but the color is indistinguishable from 'no warming' which is certainly not the case. The Committee suggests introducing a different color, perhaps gray, to indicate missing data more clearly. The figure should also show statistical significance of the trends and add the data source.

Figure ES.2: Since the Paris Agreement aims to implement GHG emissions reductions that would achieve a concentration pathway similar to Representative Concentration Pathway (RCP)2.6, it would be useful to illustrate the RCP2.6 scenario in this figure. One possible approach to including this could be to have the figure include the boxes to the right indicating the ranges for all four RCPs, as in the IPCC 2013 equivalent (SPM.7).

Figure ES.4: The Committee suggests indicating which, if any, of the trends shown are statistically significant, in addition to considering the previous comments about observed trends, baseline periods, and spatial aggregation of data. Moreover, this figure visually resembles Figure 2.18 presented in NCA3, but the numbers are quite different, perhaps because of the use of a different metric of extreme precipitation. It is fine to show a different figure, but this underscores the previous point about consistency and robustness of measures of extremes, and would benefit from some explanation. It would also be appropriate to explain any other figures that resemble NCA3 graphics but convey a different impression.

Figure ES.5: This figure is problematic for a number of reasons outlined in the previous comments on extremes. Also see Section III.6.

Figure ES.6: This figure does not convey new or important science and could be removed.

Figure ES.8: This figure does not appear in Chapter 12 as foundational material. Additionally, it is busy, hard to read due to small font, and too complicated. A single panel could be chosen for the ES, and an improved version could appear in Chapter 12. If retained, the maximum value on the y-axis should be set to 365 and the caption should explain that this is an upper limit and results in some curves displaying an inflection point (and in some cases small differences between scenarios, which is counterintuitive at first).

Figure ES.9: This figure provides a compelling illustration of observed sea ice change in the Arctic, but would benefit from a comparison of 2016 (or an average of recent years) with a multi-year average from early in the satellite era, for more robust statistical representation. See also Section III.11.

None of the material from Chapter 10, and too little of the material from Chapter 2, appears in the ES. This may be deliberate, but the Committee considered some of the findings from Chapters 2 and 10 to be worthy of representation in the ES. In particular, a simplified version of Figure. 2.6 would improve the ES (see Section III.2 for more details).

The bullet regarding limiting the global mean temperature increase to 2^{o}C (page 27, lines 17-24) that states, "cumulative emissions would likely have to stay below 1,000 gigatons carbon (GtC)" is given without a citation and is inconsistent with the 790 Gt C cited in IPCC AR5 2013. See also Section III.14.

The ES would have more impact if it more clearly emphasized what is new in the draft CSSR relative to previous climate change assessments. The "Summary of What's New Since NCA3" at the end of the ES is not prominent, lacks quantitative values, and is weakened by the inclusion of methodological changes. The full list of "Better Tools and Approaches" is more appropriate for Chapter 1. The "New Understanding" sections on extremes (page 29) could particularly benefit from re-ordering the bullets (e.g. moving lines 24-27 later, adding material to lines 21-23 and/or 28-29 to emphasize the large number of types of extremes for which a human contribution has been identified with confidence), and including quantitative statements. The Committee is skeptical about the value of extensive discussion on the 'hiatus' given that any time series with a trend and nonzero variance has short periods when the trend is opposite the underlying trend. Rather than continuing to focus on the hiatus, the Committee recommends shortening the discussion for this topic and rephrasing page 29 line31- page 30 line 2 with a statement to the effect that short-term variability (resulting in either strongly positive or flat trends) is not the best indicator of whether climate is changing in response to GHGs. The text could also note that conversely a recent string of 3 record warm years (2014-2016), occurring in part as a consequence of a strong El Niño, also does not prove an acceleration of warming - both are artificial statements that result from focusing too much on short periods of record. See Section II.1 of this report for additional recommendations on this topic.

III. Comments on Each Chapter of the Draft Climate Science Special Report

III.1 CHAPTER 1: OUR GLOBALLY CHANGING CLIMATE

Summary

Overall, Chapter 1 provides a solid introduction to the topic of climate trends and associated confidence that accurately reflects current understanding. The focus is appropriate, with most of the emphasis on observed trends, but some discussion of projections. The treatment is compact, but mostly at a sufficient level of detail to effectively communicate both the conclusions and the nature of the underlying evidence.

The emphasis on multiple lines of independent evidence, featured in Key Finding 3, is central to the chapter's impact. Throughout the chapter, an increased emphasis on documenting the findings that are based on multiple lines of independent evidence would make the chapter more effective.

The Committee thinks that the chapter can be improved in three major ways. First, the topic of extreme event attribution, a major development over the last decade, should be discussed. The introduction to extremes in Section 1.2.4 provides an appropriate discussion of trends in extremes, but the lack of consideration of extreme event attribution is a missed opportunity. Second, the long section on the hiatus in Box 1.1 of the draft CSSR gives that event much more prominence than is warranted. The main point of Box 1.1 is that internal variability can distort short-term trends. This is an important point, appropriately emphasized in Key Finding 5. Box 1.1 could be made more useful and consistent with the broad sweep of climate knowledge if it were retitled to address the role and magnitude of internal variability and if shortened substantially to provide more focused support for Key Finding 5. Third, the chapter would be substantially easier to read with a renumbering that creates a series of top-level sections. The current numbering somewhat awkwardly places most of the chapter contents in several subsections of Section 1.2. Renumbering as 1.3, 1.4, etc. would be a straightforward way to improve readability.

In addition to those three major points, the Committee has some further recommendations for improvements. Throughout Chapter 1, greater use of quantitative language, even with findings presented in qualitative terms, would be beneficial. A good example is Key Finding 5, where it is very hard to interpret "important, but limited influences on global and regional climate over timescales ranging from months to decades." In cases like this, where the goal is to indicate that something plays a small (or a large) role, the point would be clearer and more complete with more quantitative framing. For example, rewording as "influences that can have important impacts, especially regionally, over months to years but are limited to a small fraction of global climate trends over decades" would better convey the message of the key finding.

Chapter 1 includes a somewhat awkward mix of observations and projections, most of which are discussed in greater detail in later chapters. Specifically, Key Findings 2 and 4, and Figure 1.4 concern projections. Chapter organization such that the text flows smoothly from observations to projections is appropriate for the chapter, but the introductory paragraphs could better prepare the reader.

A challenge in any climate assessment is how to present observed (past) trends in important climate variables, like global mean surface temperature. There are three important considerations in constructing such a quantity: illustrating the possible role of human influence for scientific purposes, aligning with policymakers' needs, and data availability. For illustrating human influence, one could either compute a mean rate of change over the period of anthropogenic forcing or compare recent with baseline, or "pre-industrial" averages (subject to data limitations). The period 1850-1900 is widely accepted in both scientific (e.g. IPCC, 2013) and policy circles as "pre-industrial" and therefore is a good baseline to use for a "before-and-after" comparison, where the "after" would be the most recent 20-30 years. Furthermore, this baseline period minimizes the influence of anthropogenic GHG emissions on climate, but is recent enough that an adequate observational record exists.

In the draft CSSR, however, 1901-60 is generally used, in effect, to define "before", despite the fact that considerable growth in anthropogenic forcing occurred during this period (see e.g., draft CSSR Figure 2.6). In other words, this approach to characterizing change suffers from the weakness that it is both too recent and too long to characterize "before", in addition to the statistical weaknesses of this metric discussed in Section II.2 of this report. The chapter makes the legitimate point that global mean temperature is better known after 1900 than before, but an earlier period can be safely used.

Specific Review Comments Related to the Statement of Task

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

Overall, the chapter is well balanced and reflects the relevant scientific literature. The chapter could be made considerably stronger with discussion of extreme event attribution and reduced emphasis on the hiatus. The general area of extreme event attribution is so important that it may warrant a separate key finding. Alternatively, a sentence or two on extreme event attribution could be added to Key Finding 2. This should be coordinated with the recommended increased emphasis on event attribution in Chapter 3 (see Section III.3). Discussion of the risks of multiple interacting impacts, and the large magnitude of past sea level excursions could also be considered. A brief discussion of changes in ocean heat content would also be beneficial in this chapter.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

In general, the key findings are clear, appropriate, and well documented, however some attention is needed.

> **Key Finding 1:** The global climate continues to change rapidly compared to the pace of the natural changes in climate that have occurred throughout Earth's history. Trends in globally averaged temperature, sea-level rise, upper-ocean heat content, land-based ice melt, and other climate variables provide consistent evidence of a warming planet. These observed trends are robust, and have been confirmed by independent research groups around the world. (*Very high confidence*)

Of the list of indicators, changes in ocean heat content and SLR are only mentioned in Key Finding 1; some discussion elsewhere in the chapter would be appropriate. For Key Finding 1, other candidate indicators that could strengthen the list include decreasing Arctic sea ice, depth of seasonal permafrost thaw, earlier snowmelt in rivers, and start and end dates of growing seasons. Also, the phrase "rapidly compared to the pace of the natural changes in climate that have occurred throughout Earth's history" could be improved, with an adequately detailed explanation in the traceable account. Specifically, what does "rapidly compared to..." mean? Is there enough information to quantify past rates of change and their uncertainties, and compare them with recent changes?

> **Key Finding 2:** The frequency and intensity of heavy precipitation and extreme heat events are increasing in most regions of the world. These trends are consistent with expected physical responses to a warming climate and with climate model studies, although models tend to underestimate the observed trends. The frequency and intensity of such extreme events will *very*

> *likely* continue to rise in the future. Trends for some other types of extreme events, such as floods, droughts, and severe storms, have more regional characteristics. (*Very high confidence*)

For Key Finding 2, neither the text nor the traceable account provides justification for the phrase, "…although models tend to underestimate the observed trends." The way the key finding is worded, a reader cannot determine whether the mismatch between observations and simulations is a serious issue. This should be clarified.

> **Key Finding 3:** Many lines of evidence demonstrate that human activities, especially emissions of greenhouse gases, are primarily responsible for the observed climate changes in the industrial era. There are no alternative explanations, and no natural cycles are found in the observational record that can explain the observed changes in climate. (*Very high confidence*)

The concept of "no alternative explanations" needs further discussion to be understood by the intended audience. There are lots of alternative explanations. It is just that, for a number of very solid reasons, they are not credible or cannot contribute more than marginally to the observed patterns. It may be that the authors have conflated attribution of global temperature changes since mid-20th century (for which it is true that there are no alternative explanations) and attribution of "observed climate changes." The missing elements are the requirements that explanations be grounded in understood physical mechanisms, appropriate in scale, and consistent in timing and direction. Saying there are no alternative explanations invites a strong (even if incorrect) rejoinder. This recommendation also applies to the similar statement in the ES. Additionally, some identifiable natural cycles (e.g. ENSO, northern annular mode) may themselves be influenced by human activities. Rewording Key Finding 3 to address these recommendations would strengthen its impact.

> **Key Finding 4:** Global climate is projected to continue to change over this century and beyond. The magnitude of climate change beyond the next few decades depends primarily on the amount of greenhouse (heat trapping) gases emitted globally and the sensitivity of Earth's climate to those emissions. (*Very high confidence*)

In the major uncertainties provided in the traceable accounts for Key Finding 4, the text should emphasize the uncertainty in the magnitude of climate feedbacks. It would also be helpful to name the major feedbacks, including the ice-albedo and cloud cover feedbacks and refer to the feedbacks discussion in Chapter 2 of the CSSR.

Are graphics clear, and do they appropriately reflect the major points in the text?

Chapter graphics are generally informative and appropriate, although clarification or additional detail should be provided for a few.

The caption for Figure 1.2 indicates that the temperatures are plotted relative to the 1901-1960 average. However this cannot be the case, because almost all of the temperatures from 1901 to 1960 are blue (negative). Instead, it looks like the reference temperature for the zero line is probably the 20th century average. Inclusion of standard deviations for each decade and explanation in the caption would improve this figure.

Figures 1.3 and 1.7 would benefit from an indication of the location of statistical significance of trends, and Figure 1.6 should show the envelope of model results in the time series of temperature anomalies.

Are likelihood / confidence statements appropriate, and justified?

The Committee did not identify any issues with the chapter's confidence statements.

Are statistical methods applied appropriately?

In general, the Committee encourages analysis of trends based on regression or related slope-based techniques (that minimize end effects, and/or quantify uncertainty in the slope), rather than on differences between average conditions between a reference period and a later period. In Chapter 1 and throughout the report, it would be helpful to standardize time windows as much as possible, recognizing the intrinsic importance of calculating total warming since pre-industrial. See Section II.2 of the report for more detailed recommendations on this topic.

Is the chapter balanced? Are there areas that should be expanded, or removed?

The Committee recommends reframing and shortening Box 1.1 on the hiatus. One option is to discuss the hiatus within the context of other aspects of internal variability. This could include discussion of the limitation of evaluating short periods of record when looking for GHG signatures because of the difficulty in attributing trends to short periods. Further, short-term trends are not particularly useful for model evaluation because in many cases, we do not entirely understand what drives the short-term trends. A recent paper by Yan et al. (2016) would also be a valuable citation to consider including in the discussion of this topic.

Recommended changes to structure

Section 1.2 is longer than many chapters and subsections are uneven in effectiveness of conveying the intended message. The strongest sections quantify the trends they describe, are clear about the time periods under consideration, and attempt to provide brief explanations for the phenomena observed or modeled. Sections that need strengthening include those on precipitation, extreme events, and land processes. Integration of ocean heat content into the discussion of SLR and quantifying the changes and trends described in these sections would also benefit the chapter. The Committee further recommends reorganizing to make subsections into sections, with associated content changes based on comments provided earlier in this section for the chapter.

III.2 CHAPTER 2: PHYSICAL DRIVERS OF CLIMATE CHANGE

Summary

Chapter 2 provides an essential overview of the mechanisms of climate change. Much of the text is sufficiently detailed so that a scientifically literate audience can begin to understand how increases in GHGs can lead to large perturbations in the earth-atmosphere-ocean system. Text on the importance of feedbacks to this system is helpful. For example, the chapter makes clear the importance of water vapor in amplifying the radiative effects of CO_2 and other GHGs.

The Committee has some suggestions for improvement of the chapter. First, the text should emphasize from the start the interconnectedness of the Earth-atmosphere-ocean system. As written, there is too much emphasis on atmospheric processes, at least initially. The role of changing land cover is not mentioned until 11 pages into the chapter, and the role of the ocean is not described until 18 pages in. Second, there is little mention of Chapter 2 in the ES. The Committee suggests that Figure 2.6 (with suggested edits provided here) could be included in the ES, along with Key Finding 1. Together, this material provides a strong demonstration of the changes in the drivers of climate. Key Finding 1 should also mention that anthropogenic forcing accelerated rapidly in the 1960s.

Specific Review Comments Related to the Statement of Task

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

A clear statement about the interconnectedness of the Earth-atmosphere-ocean system is needed early in the chapter. Climate change can be considered a redistribution of heat, water, and carbon within this interconnected system. The long-term consequences of anthropogenic climate change should be emphasized in the beginning paragraphs, with up-to-date references (e.g., Clark et al., 2016).

The chapter should clarify that the scientific and policy communities have devised a set of metrics with which to compare the relative effects of different perturbations to climate. These metrics include radiative forcing (RF), effective radiative forcing (ERF), global warming potential, and global temperature potential. Brief descriptions of each metric are warranted. In addition, the definition of ERF is not in line with that in the IPCC AR5 (Myhre et al., 2013). While ERF can be calculated in several ways, Myhre et al. (2013) clearly favor the approach that allows many rapid adjustments to forcing to take place, including that of land surface temperatures. Box 8.1 of Myhre et al. (2013) illustrates this widely accepted definition of ERF. The definition of climate sensitivity should be more detailed and the range of estimates for this important metric given. Finally, the text could refer to the envelope of climate projections for particular scenarios in Chapter 4 as a measure of how climate sensitivity varies across models. A succinct discussion of how climate sensitivity differs from transient climate forcing would also be helpful. Mention of the sources of uncertainties illustrated in the relevant figures in the draft CSSR, such as climate sensitivity, future GHG emissions, and ocean heat uptake, could also be useful.

Regarding the effect of aerosols on climate, the scientific community has moved on from the complicated and overlapping definitions of “direct effect,” “first indirect effect,” “semi-direct effect,” and so on. The text should adhere more closely to the new (and simpler) classifications of these effects: aerosol-radiation interactions and aerosol-cloud interactions, as described in IPCC AR5 (Boucher et al., 2013). Old terms should be mentioned once at most.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** Human activities continue to significantly affect Earth’s climate by altering factors that change its radiative balance (known as a radiative forcing). These factors include greenhouse gases, small airborne particles (aerosols), and the reflectivity of the Earth’s surface. In the industrial era, human activities have been and remain the dominant cause of climate warming and have far exceeded the relatively small net increase due to natural factors, which include changes in energy from the sun and the cooling effect of volcanic eruptions. (*Very high confidence*)

This finding affirms the scientific consensus that anthropogenic emissions of GHGs have perturbed the radiative balance of the Earth. The Committee recommends clarifying the text by revising to state that “…humans activities have been, and increasingly are, the dominant cause…” The evidence base should include up-to-date references to changes in heat storage and other properties of the ocean. The Committee also recommends that the finding emphasize the rapid acceleration in anthropogenic forcing since the 1960s, as indicated by Figure 2.6.

> **Key Finding 2:** Aerosols caused by human activity play a profound and complex role in the climate system through direct radiative effects and indirect effects on cloud formation and properties. The combined forcing of aerosol–radiation and aerosol–cloud interactions is negative over the industrial era, substantially offsetting a substantial part of greenhouse gas forcing, which is currently the predominant human contribution (*high confidence*). The magnitude of this offset

> has declined in recent decades due to a decreasing trend in net aerosol forcing. (*Medium to high confidence*)

Key Finding 2 confirms the large uncertainties in quantifying the effects of aerosols on climate, but uses a mix of old and new terminology to describe the interactions of aerosols with the climate system, making it confusing and hard to follow. The description of evidence base should adhere to IPCC AR5 terminology (Boucher et al., 2013) and references should be updated. As written, some of the evidence base is listed in the "uncertainties" section of the traceable accounts instead of in the "description of evidence base" section. Revising the traceable accounts to clarify the evidence vs. uncertainties would strengthen the finding. This finding should also emphasize the large regional forcings of aerosols over polluted areas and the potentially large consequences of these forcings. While global aerosol concentration is decreasing over recent decades, there is also much evidence that aerosol is increasing in developing countries, with potentially large consequences for regional climate. The text should also clearly state that the net effect of aerosols is cooling. Finally, the albedo effect of light-absorbing aerosols deposited on snow and ice should be mentioned.

> **Key Finding 3:** The climate system includes a number of positive and negative feedback processes that can either strengthen (positive feedback) or weaken (negative feedback) the system's responses to human and natural influences. These feedbacks operate on a range of timescales from very short (essentially instantaneous) to very long (centuries). While there are large uncertainties associated with some of these feedbacks, the net feedback effect over the industrial era has been positive (amplifying warming) and will continue to be positive in coming decades. (*High confidence)*

This finding emphasizes the importance of feedbacks to the climate system, and is important for the intended audience. Examples of climate feedbacks would also be helpful in conveying this finding. More attention should be paid to the earth-atmosphere-ocean as an interconnected system, with changes to the ocean likely persisting for millennia. The Committee discourages ranking of the uncertainty in feedbacks e.g., "Cloud feedbacks carry the largest uncertainty of all the feedbacks..." Relative magnitudes of these uncertainties are not known. A graphic that specifically illustrates Key Finding 3 would also be helpful to the reader.

Are graphics clear, and do they appropriately reflect the major points in the text?

The Committee recommends that the Figures be updated to include more recent years, if possible. Figure 2.2 is very difficult to interpret, and relies on a non-standard definition of ERF. All feedbacks also appear to follow from temperature when in fact, could feedbacks can arise directly from aerosol-cloud interactions and land albedo change can follow directly from land use change. The Committee suggests the diagram be revised and simplified to look more like Figure 8.1 in Myhre et al. (2013) or Figure 2.1 in Forster et al. (2007).

Figure 2.4 is outdated now that atmospheric CO_2 concentration has passed 400ppm. Figure should either be updated or deleted.

Figure 2.6 is an interesting figure that would benefit from clarification of some of the legend text in the caption, e.g. "Aer-Rad Int." and "BC on Snow + Contrails."

Figure 2.7 is probably unnecessary, as it adds little to the central message of the chapter.

The Committee recommends including a graphic that specifically illustrates Key Finding 3. Examples of existing relevant graphics include Figures 9.43 and 9.45 in Flato et al. (2013).

Are likelihood / confidence statements appropriate, and justified?

Yes, these statements are appropriate and justified in Chapter 2.

Are statistical methods applied appropriately?

Most Figures and Table 2.1 show confidence intervals, although the error bars in Figure 2.4 are not defined. A few values in the text lack an indication of uncertainty, as noted in Appendix A, Line Comments.

Is the chapter balanced? Are there areas that should be expanded, or removed?

Section 2.1 should be expanded as described previously, to include more information on the interactions of the ocean and land cover with the atmosphere. Section 2.2 should focus on all metrics of climate change, not just RF and ERF.

Recommended changes to structure

None beyond those previously described.

III.3 CHAPTER 3: DETECTION AND ATTRIBUTION OF CLIMATE CHANGE

Summary

This chapter is intended to convey the message that the observed changes in global climate since the mid-20th century are detectable and largely attributable to human influences, which is an important point that is referenced in other parts of the draft CSSR. There have been several advances in detection and attribution of climate change, particularly the capability to attribute regional-scale climate change, extreme weather and climate events (or classes of events) to human influences. The fact that this chapter has only one key finding, which is focused only on the change in global mean surface temperature, is indicative of a missed opportunity. Both the IPCC Fourth Assessment Report and AR5 contain chapters that collected detection and attribution findings across a wide range of subjects. This information is distributed across several chapters in the draft CSSR. By the logic of including detection and attribution results in the chapters that covers that topic, the key finding in Chapter 3 should appear in Chapter 1.

The Committee recommends the following substantive changes to Chapter 3:

- The chapter should contain a more comprehensive evaluation of detection and attribution, refer more to IPCC reports, and place greater emphasis on the latest detection and attribution advances in both methodology and results. Input from an expert in detection and attribution could be beneficial in ensuring the latest understanding and advancements in the field are appropriately captured in the draft CSSR. The chapter should also clearly identify and provide a substantially more in-depth discussion of the major scientific questions that have received attention since IPCC AR5 and NCA3, particularly with regard to attribution of extreme weather events.
- The introduction, which the Committee found extremely dense and rather unintelligible for the intended scientifically literate audience, does not serve the intended purpose of introducing the reader to the topic. The introduction should include a better explanation of the conceptual approach to detection and attribution, and the detailed description of the methodology should be encapsulated in an appendix on methods.

- The remainder of the chapter should better link examples of detection and attribution to the discussion of these topics in other chapters of the draft CSSR by referencing relevant sections. There is now a rich literature on detection and attribution of climate change that should also be cited in this chapter, and where appropriate in other chapters. Some recommended citations are provided in the next section.
- The chapter could also benefit from some emphasis on the importance of this detection and attribution science for determining whether human influence on climate variables (and on individual extreme events or classes of extreme events) can be distinguished from natural occurrences. This discussion could then inform decisions on climate policy, adaptation, legal liability, etc.

Specific Review Comments Related to the Statement of Task

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

Much of the material in Chapter 3 is drawn from the IPCC AR5, (Bindoff et al., 2013). There are several other specific topics and papers that could also be cited to strengthen the message and content of this chapter. For example, discussion of the optimal fingerprinting technique and recent updates and applications of this method (e.g., Zwiers et al., 2011), as well as studies that use data assimilation as an underlying technique (e.g., Hannart et al., 2016) should be included. Citation of other attribution papers could include Schurer et al. (2013), Stone et al. (2013), Stern et al. (2014), Zwiers et al. (2013), Andres and Peltier (2016), and Hulme (2014).

Greater emphasis on the most recent advancements in detection and attribution is also warranted. The Committee recommends reviewing the NASEM report, "*Attribution of Extreme Weather Events in the Context of Climate Change"* (2016a) and references therein.

The Committee strongly recommends including a discussion of the nature of, or challenges in, detection and attribution, e.g., detecting and attributing changes in means vs. trends or extremes. Other examples of how detection and attribution approaches have evolved in the recent literature are also warranted. This is similar to the text already in the draft CSSR indicating that changes in extreme temperature now can be detected with greater confidence (NASEM, 2016a). Finally, some discussion is needed of the extreme values associated with a given averaging period (e.g. daily, monthly, seasonal, or annual records).

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** The *likely* range of the human contribution to the global mean temperature increase over the period 1951–2010 is 1.1° to 1.3°F (0.6° to 0.7°C), which is close to the observed warming of 1.2°F (0.65°C) (*high confidence*). It is *extremely likely* that more than half of the global mean temperature increase since 1951 was caused by human influence on climate (*high confidence*). The estimated influence of natural forcing and internal variability on global temperatures over that period is minor (*high confidence*)

This key finding includes three statements that describe, in different ways, the human influence on the global mean surface temperature and does not go much beyond what was already documented in IPCC AR5. The three statements are also, to some extent, redundant with each other and with findings in other chapters.

An additional key finding about extreme events, which is the topic of many of the more recent detection and attribution studies, would substantially improve the chapter.

Are graphics clear, and do they appropriately reflect the major points in the text?

Figure 3.1 (the only graphic in the chapter) is very clear and makes its point well. However, it could be better linked to the chapter text.

Are likelihood / confidence statements appropriate, and justified?

Yes, the likelihood and confidence statements are appropriate and justified.

Are statistical methods applied appropriately?

In general, detection and attribution methods are statistical in nature, and this is conveyed in the chapter. On the other hand, there is a basic question about the amount of data (length of record) necessary to detect a trend in a climate time series. A statement to that effect should be included in this chapter, which would also be relevant to Chapter 1. The statement should be clear about how much more data is needed to detect a change in a trend (e.g. the hiatus) vs. detecting a trend. In addition, the description of multi-step attribution and attribution-without-detection methods is vague and hard to follow. Even the example is too abstract and does little to help the reader understand the material. The description of the risk-based approach to attribution is likewise vague and overly general. The section describing this approach would benefit from a mathematical expression to quantify the discussion and make it more concrete.

Is the chapter balanced? Are there areas that should be expanded, or removed?

As noted previously, the introduction should include a better explanation of the conceptual approach to detection and attribution, and the detailed description of the methodology should be encapsulated in an appendix on methods. The bulk of the chapter should then be devoted to describing examples of detection and attribution that are relevant to the other chapters of the draft CSSR. This could include a timeline, table, or other way to indicate how much the field of detection and attribution has changed in recent years. The challenges associated with model dependence and difficulties with attribution of extreme events could also be articulated more fully. The Committee suggests that the chapter would be strengthened by adding a key finding that highlights advances in the detection and attribution of features of climate change that go beyond simple global mean surface temperature. For example, "The science of event attribution is rapidly advancing with the understanding of the mechanisms that produce extreme events and the development of methods that are used for event attribution." (paraphrased from NASEM, 2016a).

Recommended changes to structure

See Summary comments and response to previous question.

III.4 CHAPTER 4: CLIMATE MODELS, SCENARIOS, AND PROJECTIONS

Summary

Chapter 4 provides necessary background about the growth of CO_2 concentrations, both in the recent past and projected in the future. The chapter also describes how global climate models (GCMs) and regional downscaling, either using regional dynamical climate models (RCMs) or statistical methods, transform information about changes in forcing by GHGs and aerosols into information about the climate system, in the past, present, and future. It is important to characterize the nature of the changing concentrations of

GHGs and aerosols and the implications these have for the physical climate system, so this chapter represents a valuable portion of the report.

As written though, the chapter is difficult to read. The three topics named in the title of the chapter are treated in quite different depth: emissions scenarios are much more prominent than models and projections. Moreover, the draft is not balanced in terms of the discussion of GCMs and RCMs—the regional performance of GCMs is given short shrift, and RCMs are given much more prominence than is commensurate with the rest of the draft CSSR. In particular, there is insufficient discussion of the limitations of RCMs, which could result in inadequate support for Key Finding 4.

It is important that Chapter 4 carefully articulate the advancements in climate modeling over time, including the evolution from atmosphere-centric to Earth system models, and focus that discussion on recent advancements such as are represented in the step from Coupled Model Intercomparison Project (CMIP) 3 to CMIP5. The discussion of the difference between the IPCC Special Report on Emissions Scenarios (SRES) approach and the RCP approach to emissions scenario development should be clearer, and the choice in the draft CSSR to focus on RCPs 4.5 and 8.5 should likewise be clarified. For example, it is implied that RCP4.5 represents a low emissions future, but RCP2.6 defines a much lower emissions future and one roughly consistent with the Paris Agreement. There are hypothetical scenarios (such as constant concentration and zero emissions) that should also be more clearly defined and described. There are ample reports and published papers documenting the similarities and differences in the two generations of emissions scenarios and GCMs (i.e. SRES-CMIP3 and RCP-CMIP5) that can be cited. Finally, the chapter is overly dependent on a single report (Kotamarthi et al., 2016) for much of the assessment discussion. Citation of the research literature underpinning the state of assessment science should be substantially increased.

Specific Review Comments Related to the Statement of Task

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

An important omission from this chapter is a discussion of the advances in climate modeling, both in GCMs and RCMs, that have been made since IPCC AR5 and NCA3. In particular, the CMIP5 generation of coupled model experiments has been executed and published and whose results were not extensively used in NCA3. This chapter would benefit from a pointwise description of the differences between CMIP3 and CMIP5, including both modeling advances and scientific findings. Such comparisons have been made and are published. For example, two recent reports from NOAA are available comparing the two generations of models and their results for North America (https://docs.lib.noaa.gov/noaa_documents/NESDIS/TR_NESDIS/TR_NESDIS_144.pdf and http://cpo.noaa.gov/ClimatePrograms/ClimateandSocietalInteractions/COCAProgram/COCAArchive/TabId/390/ArtMID/1263/ArticleID/358942/Comparing-Two-Generations-of-Climate-Model-Simulations-and-Projections-of-Regional-Climate-Processes-for-North-America.aspx), and the papers cited in these reports are useful resources for this chapter.

It is unclear what value there is in including discussion of the World Climate Research Programme COordinated Regional climate Downscaling Experiment (CORDEX) in the draft CSSR. Results from CORDEX are not available and the RCM simulations in that experiment are run at 50-km spatial resolution, which is no longer significantly higher than typical GCM resolution, and based on a very limited and older set of GCM runs with a single SRES scenario.

One of the new advances heralded in Chapter 4 is the use of unequal weights in combining multiple climate models to arrive at consensus results. While it is true that previous studies have used equal weighting, it should be mentioned that this is not only due to expediency or to a desire not to offend certain modeling groups—there are studies indicating that equal weighting of climate model output is statistically unsurpassed by any unequal weighting scheme in terms of prediction skill, at least for some applications (e.g., Peng et al., 2002; Peña and van den Dool, 2008; DelSole et al., 2012). The model weighting discussion in Flato et al. (2013) may also be appropriate to reference in this chapter. Finally, the scientific and statistical advantage of the new method by Sanderson et al. should be highlighted.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** Merely maintaining present-day levels of greenhouse (heat-trapping) gases in the atmosphere would commit the world to at least an additional 0.3°C (0.5°F) of warming over this century relative to today (*high confidence*). Projections over the next three decades differ modestly, primarily due to uncertainties in natural sources of variability. Past mid-century, the amount of climate change depends primarily on future emissions and the sensitivity of the climate system to those emissions.

This key finding is not linked to the rest of the chapter or to Figure 4.1, where it could be illustrated. The key finding is presented with no uncertainties and only one citation (granted, an IPCC chapter), and yet is given "high confidence". The supporting language in the rest of the chapter should be clear that this finding refers to a "constant concentration" scenario, not a "zero emissions" scenario—the latter would result in almost immediate, if gradual, decline in CO_2 concentration. The message for this key finding is better worded as it appears in the ES and the same language could be used here.

> **Key Finding 2:** Atmospheric carbon dioxide (CO_2) levels have now passed 400 ppm, a concentration last seen about 3 million years ago, when average temperature and sea level were significantly higher than today. Continued growth in CO_2 emissions over this century and beyond would lead to concentrations not experienced in tens to hundreds of millions of years. The rapid present-day emissions rate of nearly 10 GtC per year, however, suggests that there is no precise past climate analogue for this century any time in at least the last 66 million years. (*Medium confidence*)

There are multiple statements in Key Finding 2. The first part about the current level of CO_2 concentration and its future growth should be given a separate confidence level (probably "high", given the body of evidence cited).

> **Key Finding 3:** The observed acceleration in carbon emissions over the past 15–20 years is consistent with higher future scenarios (*very high confidence*). Since 2014, growth rates have slowed as economic growth begins to uncouple from carbon emissions (*medium confidence*) but not yet at a rate that, were it to continue, would limit atmospheric temperature increase to the 2009 Copenhagen goal of 2°C (3.6°F), let alone the 1.5°C (2.7°F) target of the 2015 Paris Agreement (*high confidence*).

The evidence base for this key finding is consistent with the confidence levels indicated. However, more evidence is needed in the traceable account for the statement that economic growth has begun to decouple from fossil fuel combustion.

> **Key Finding 4:** Combining output from global climate models and dynamical and statistical downscaling models using advanced averaging, weighting, and pattern scaling approaches can result in more relevant and robust future projections. These techniques also allow the scientific community to provide better guidance on the use of climate projections for quantifying regional-scale impacts (*medium to high confidence*).

This finding is more of a methodological decision than a finding and the evidence base provides inadequate support. It relies entirely on a single federal report in the gray literature (Kotamarthi et al., 2016), with a vague reference to a large body of literature—key examples from the latter should be cited. The portion of the key finding that "These techniques allow the scientific community to provide better guidance on the use of climate projections for quantifying regional-scale impacts" is given "medium to high confidence". However, the science, as documented in the traceable accounts, does not support high confidence on this broad statement. Confidence depends on the specific guidance, and the specific impact, so the statement is overly vague and should be revised. The statement in the traceable accounts that downscaling is "broadly viewed" as robust should also be documented or deleted.

Are graphics clear, and do they appropriately reflect the major points in the text?

Figure 4.2 is confusing and could be deleted. The statement "calculated in 0.5°C increments" is not appropriate for the intended audience and the essential information is already conveyed much more effectively in Figure 4.1.

Figure 4.3 is an effective graphic, but would be better placed in Chapter 12 (Section III.12).

The Committee was divided about the value of Figure 4.5, with some asserting that it does not add to the report narrative. It depicts results with an RCM run at different resolutions, so it is not a good choice for demonstrating the difference between GCMs and RCMs. A replacement that specifically illustrates differences between GCMs and RCMs could be more useful.

Figure 4.6 adds little to the draft CSSR because it is stripped of the context provided in the original Hawkins and Sutton paper, where the regional uncertainties are visibly different from the global uncertainties, and where the total uncertainty grows with time. While it is important to show results for Alaska and Hawai'i when such results are relevant, the results for these regions in Figure 4.6 are not sufficiently different from the results for the contiguous United States (CONUS) to warrant inclusion. Moreover, even though the point made by the figure is important, it is not well linked with the relevant Chapters. This figure could be revised and included in Chapter 5, where it would make sense to complement Figure 5.4, or it could be moved to an appendix.

Are likelihood / confidence statements appropriate, and justified?

As stated in the discussion about key findings, this chapter would benefit from including uncertainties wherever possible, stronger traceable accounts, and greater balance in discussion of GCMs and RCMs.

Are statistical methods applied appropriately?

The discussion of the rate of change of CO_2 concentration in Section 4.2.5 suggests that finding an analogue in the paleoclimate record requires a match to the rate of change. The last sentence in Section 4.2.5 conflates magnitude of change and rate of change, without comment. As mentioned several times in the draft CSSR,(e.g. page 158, lines 18-19), the long-term impact of human activities on climate can be assessed in relation to the paleoclimate record only in equilibrium, so the rate of change of CO_2 concentration seems to be irrelevant. Some clarification of the relationship of these two seemingly different statements is needed.

Is the chapter balanced? Are there areas that should be expanded, or removed?

The treatment of GCMs and RCMs is uneven. For example, the list of features that are represented in a GCM on page 160 is rather odd in that it is neither comprehensive nor particularly representative of the important features that one expects a GCM to faithfully reproduce. Some clarification of the nature of this list is needed.

The description of RCMs and their advantages is much more coherent and comprehensive, but the list of shortcomings of RCMs is incomplete. In addition to what is mentioned, the chapter should discuss the mismatch between the way that GCMs and RCMs represent subgrid-scale physical processes and the fact that many RCMs lack two-way interaction, which results in an inevitable gradient in important quantities between the domains of the GCM and RCM. For example, unmatched boundary conditions on the downstream side of RCMs lead to unique biases; the grid spacing of RCMs, e.g. 50 km in the North American Regional Climate Change Assessment Program (NARCCAP) and CORDEX, is not very different from the grid spacing in GCMs being used in CMIP6, so the advantage of RCMs is not clear; the specification of GCM output at the lateral boundaries of RCMs introduces uncertainty and error; and considerable "hidden physics" is included at the lateral boundaries in the form of sponge conditions or other engineering accommodations for the mismatch in dynamic features at the interfaces.

Recommended changes to structure

The Committee recommends a number of revisions and reorganization of sections to better focus the chapter scope and improve the readability.

Section 4.2 should include an introductory paragraph specifically mentioning that there are different ways of addressing scenario uncertainty, depending on the objective. Sections, 4.2.1 through 4.2.4 describe different ways of approaching the relationship between emissions, concentration, and temperature change, and this should be summarized in the introduction.

Section 4.2.1 second paragraph (page 154, lines 1-10) is difficult to follow and the purpose of the calculation is not described. The paragraph should be rewritten for clarity and motivation, and it should reference Swain and Hayhoe (2015).

Section 4.2.2 on Shared Socioeconomic Pathways seems out of place and adds little to the report. This section could be omitted.

Section 4.2.3 discusses the global mean temperature scenario approach and pattern-scaling, but it is unclear whether this technique is used in the rest of the report. Also, the approach seems more related to impacts, in that it bypasses uncertainty in scenario evolution and deals more with specific impacts. It could be omitted as it pertains more to NCA4 than to the intended scope of the draft CSSR. Or, if kept, it should be revised.

Section 4.2.4 is back to cumulative C emissions, which again relates to mitigation policies. This fits better with Section 4.2.1, so omitting 4.2.2 and 4.2.3 would lead to a more logical order.

Section 4.2.5 does not fit well in its current location and would be more appropriate in Section 4.3.

Sections 4.3 and 4.4 contain materials that would fit better in a methods appendix (Appendix B of the draft CSSR is already a start).

Section 4.3.2: The paragraph that discusses CORDEX (page 161, lines 25-33) could be omitted. See the earlier comment noting that the value of including CORDEX in the draft CSSR is not apparent.

Section 4.3.3 focuses on Empirical Statistical Downscaling Model (ESDM), but results do not figure prominently in the draft CSSR. The abbreviation is not used elsewhere, and outside of traceable accounts, "downscaling" appears only in Chapter 8. There is also no discussion of how ESDMs are evaluated, e.g., is there any dependent/independent data testing? If so, how well do these models perform in such tests? Finally, the section is overly reliant on Kotamarthi et al. (2016). This section should only be retained if considerably revised.

III.5 CHAPTER 5: LARGE SCALE CIRCULATION AND CLIMATE VARIABILITY

Summary

This chapter is well written and flows nicely. The chapter covers modes of climate variability in the tropics and mid-latitudes, and discusses recent advances in quantifying the role of internal variability on past and future climate trends. Some of these topics have seen advances in science and conceptual understanding since the NCA3 and IPCC AR5. The Committee has some suggestions for improving the chapter that are included here.

Specific Review Comments Related to the Statement of Task

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

The Committee thinks that the chapter accurately reflects the scientific literature, except in details of the discussion of the Atlantic Multidecadal Oscillation (AMO) and Pacific Decadal Oscillation (PDO). In particular, Newman et al. (2016) strongly caution against the interpretation that U.S. temperature and precipitation variations that occur concurrently with the PDO are indeed an impact of the PDO. Also, Newman et al. (2016) indicate that the PDO does not have a preferred time scale. The AMO has been defined different ways (average sea surface temperature over a region or leading pattern from Empirical Orthogonal Function analysis), and the instrumental record is too short to detect an oscillation with a putative 50-70 year period. It may be a statistical artifact, or it may result from interdecadal fluctuations in aerosol concentrations. Language should be changed as appropriate to reflect this literature, either removing references to these quasi-oscillations or including alternate judicious views for balance. See the Line Comments in Appendix A with additional suggestions for the AMO. The Committee did not think that any critical content areas were missing from the chapter.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** Under increased greenhouse gas concentrations, the tropics are *likely* to expand with an accompanying poleward shift of the subtropical dry zones and midlatitude jets in each hemisphere (*medium to high confidence*). While it is *likely* that tropics have expanded since 1979 (*medium confidence*), uncertainties remain regarding the attribution of these changes to human activities.

This key finding is generally presented clearly and well documented, but authors could consider adding that storm tracks are shifting poleward, (e.g. Norris et al., 2016). Also, because this finding states only "medium to high confidence", the inclusion of a likelihood statement could be confusing to interpret and may not be appropriate. Finally, it is not clear why the "Low" confidence box is checked in the traceable accounts.

> **Key Finding 3:** Increasing temperatures and atmospheric specific humidity are already having important influences on extremes (*high confidence*). It is still unclear, however, to what extent increasing temperatures and humidity have influenced and will influence persistent circulation patterns, which in turn influence these extremes.

Key Finding 3 is not well grounded in the text. The relationship between temperature and atmospheric specific humidity is not discussed in the chapter and should either be discussed, removed, or moved (and discussed) to Chapter 6.

Are graphics clear, and do they appropriately reflect the major points in the text?

Figure 5.1 is not referenced in the text and depicts an old and unreasonably over-simplified zonally averaged picture of the general circulation that is not realized in nature, other than to some degree in the Hadley cell. The Committee suggests removing the figure and instead explaining the processes briefly in the text (see detailed recommendation in Line Comments, Appendix A).

Are likelihood / confidence statements appropriate, and justified?

Likelihood and confidence statements are appropriate and justified, but see previous comment for Key Finding 1.

Are statistical methods applied appropriately?

A discussion of the statistical significance (even if qualitative) should be added where appropriate. In particular, the discussion of teleconnections to Central Pacific or Eastern Pacific El Niño-Southern Oscillation (ENSO) events is based on a very small number of events, and should be caveated.

Is the chapter balanced? Are there areas that should be expanded, or removed?

The chapter is relatively well balanced in its content. The Committee recommends expanding the discussion of model fidelity in simulating natural modes of variability, and as appropriate, the connection with temperature or precipitation over the United States. This is cited as a source of uncertainty for the Key Finding 2 justification and therefore needs to be supported by the text.

III.6 CHAPTER 6: TEMPERATURE CHANGES IN THE UNITED STATES

Summary

This chapter addresses changes in mean temperature and extreme temperature in the United States, which are of foundational importance in discussing climate change and informing the development of NCA4. Results are generally consistent with NCA3, though some differences have arisen because of changes in model weighting, variables considered, and averaging period. Chapter 6 is generally well written and flows nicely, but could be improved by expanded discussion of extreme heat, the influence of the Dust Bowl on the observed record, and other topics detailed here.

The Committee has the following concerns about the treatment of extreme events in this chapter:

- The extreme metrics were often difficult to understand, especially the definition of warm and cold "spells". How brief are "brief periods"? And how much above- or below-normal temperature?. To clarify, a box or text should be added that explicitly defines each of the extreme metrics that are discussed and provides a precise definition (see Appendix A for additional extreme metrics that should be defined).
- The Committee strongly recommends additional discussion and justification of extreme heat changes. The data presented in Table 6.2 and Figures 6.3 and 6.4 seem inconsistent with Key Finding 2, apparently because of the extreme high temperatures during the Dust Bowl years. Some of this confusion with Key Finding 2 comes from the statement that "In recent decades … intense heat waves have become more common". This could mean that the frequency of heat waves in the last couple of decades is greater than the frequency in the 1901-1960 period; or it could mean that there has been an upward trend in the last few decades. The latter is probably

intended, but the language needs to be clarified and the issue needs to be addressed in more detail.

- The metric shown in the line plots in Figure 6.3 is confusing and needs further explanation. The Committee's understanding was that each point represents the average (over all stations) of the highest temperature recorded during a particular year. This metric will be extremely sensitive to spatial distribution of stations and therefore to the approach of spatial averaging. The Committee recommends removing the line plots, using an area based approach (for example, EPA metric at https://www.epa.gov/climate-indicators/climate-change-indicators-high-and-low-temperatures or references therein) for depicting variations in U. S. temperature, or using a metric that is less susceptible to spatial inhomogeneity, such as days exceeding a local percentile threshold. Whichever approach is taken, the text or traceable account should include enough information for the reader to find or reproduce the plot.
- Other analyses have shown that even if the Dust Bowl is neglected, extreme high temperatures in the Midwest do not appear to have increased as they have in the western United States. This may be due to increased agricultural intensity (Mueller et al. 2016). It is important that the key findings accurately represent and explain this discrepancy.
- The Committee also suggests adding a paragraph that discusses maximum and minimum temperatures, or at least adding more language as to where this change is likely to be true (see page 224, lines 13-15). In the Midwest and Great Lakes region, the opposite may be true. Is this difference due to the model weighting, or is it spatially variable?

SPECIFIC REVIEW COMMENTS RELATED TO THE STATEMENT OF TASK

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

The Committee thinks that Chapter 6 generally reflects the scientific literature with accuracy. An exception is the discussion of extreme heat, as detailed previously. Additional discussion of changes in minimum and maximum temperature (daily highs vs. lows, and / or trends in winter vs. summer), both for past variations and future projections (page 224, lines 13-15) is also suggested.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** The annual-average, near-surface air temperature over the contiguous United States has increased by about 1.2°F (0.7°C) between 1901 and 2015. Surface and satellite data both show rapid warming since the late 1970s, while paleo-temperature evidence shows that recent decades have been the warmest in at least the past 1,500 years. (*Extremely likely, High confidence*)

The change in annual average temperature should be expressed as a range that reflects the uncertainty in the estimate. Also, the estimated increase between 1901 and 2015 is less than the low end of Key Message 3 in NCA3 that stated, "U.S. average temperature has increased by 1.3°F to 1.9°F since record keeping began in 1895". This difference needs to be discussed in the text. It would be useful to note that for most of the United States, the observed warming is consistent with anthropogenic forcing (Figure 6.5).

The Committee thinks the portion of the key finding referencing the paleo record and recent warming is likely overstated. The IPCC AR5WG1 provided a similar finding and attributed only medium confidence. Further, uncertainties associated with proxy records and reconstructions make it challenging to assign such a high confidence.

The description of evidence base does not contain appropriate information to support Key Finding 1. While it is true that previous assessments demonstrate that the United States has warmed, the specific amount of warming - and more importantly, the actual data sources and their uncertainties—are not given, and extremes are covered in Key Finding 2, not 1.Sea surface temperatures are barely discussed in the chapter and are not mentioned in Key Finding 1, so it is odd the topic is included in the traceable account, and while the data sources are given, no details are provided to show how the main conclusions are reached.

> **Key Finding 2:** Accompanying the rise is average temperatures, there have been—as is to be expected—increases in extreme temperature events in most parts of the United States. Since the early 1900s, the temperature of extremely cold days has increased throughout the contiguous United States, and the temperature of extremely warm days has increased across much of the West. In recent decades, intense cold waves have become less common while intense heat waves have become more common. (*Extremely likely, Very high confidence*)

Key Finding 2 requires clarification and consistency of extreme events with the figures and evidence described, as stated in the Summary comments for this chapter. The statement that “the temperature of extremely warm days has increased across much of the West”, and “intense heat waves have become more common” is in direct contradiction (in message) to Table 6.2, which shows decreases in the warmest day of the year, and decreases in the warmest 5-day 1-in-10 year event. This discrepancy needs to be addressed. Also, the description of evidence base provided for this key finding should include a discussion of how extreme temperatures during the Dust Bowl years have impacted relative changes in extreme temperatures over the recent period. The role of this event needs to be discussed in key findings (perhaps given its own key finding, or a discussion box). Further, it is difficult to understand how a statement that includes increases in extreme warmth can be associated with a high confidence or extremely likely statement, given that most of the graphics in this chapter show a decrease in extreme warmth in the historical record.

> **Key Finding 3:** The average annual temperature of the contiguous United States is projected to rise throughout the century. Increases of at least 2.5°F (1.4°C) are projected over the next few decades, meaning that recent record-setting years will be relatively “common” in the near future. Increases of 5.0°–7.5°F (2.8°–4.8°C) are projected by late century depending upon the level of future emissions. (*Extremely likely, Very high confidence*)

The Committee recommends expressing the projected change in terms of a range, rather than “at least 2.5°F”. The range of 5.0°F-7.5°F is due to scenario uncertainty, and it would be appropriate to list the range of expected warming for each of the two emissions scenarios instead. Also, the description of evidence base is too general in citing broad assessments when it would be more appropriate to cite specific literature. Indication of what data set the projections are based on is needed, and how model weighting is applied (if it is). Finally, quantitative statements linked to “extremely likely” should include the appropriate ranges computed using multiple GCMs. The numbers used here do not match Table 6.4. There may be an undocumented mismatch in the area indicated, definition of “late century”, and which RCPs are considered, and this should be noted.

> **Key Finding 4:** Extreme temperatures are projected to increase even more than average temperatures. The temperatures of extremely cold days and extremely warm days are both projected to increase. Cold waves are projected to become less intense while heat waves will become more intense. (*Extremely likely, Very high confidence*)

Similar to Key Finding 3, the description of evidence base should indicate what data set the projections are based on, and how model weighting is applied (if it is).

Are graphics clear, and do they appropriately reflect the major points in the text?

Figure 6.2 is not cited in the text and requires additional detail to provide support for chapter messages. Specific considerations are provided in Appendix A.

Figure 6.6 is not cited in the text and should either be removed or moved to Appendix B of the draft CSSR, where model weighting is discussed. The figure is also challenging to interpret and requires more explanation. The metric "distance from observations" would likely be confusing to the intended audience, and most scientists would require some knowledge of how that distance was calculated.

Figure 6.9 adds little to the chapter besides illustrating large geographic themes. It could be noted here that the empirical statistical downscaling improves on the coarse climate model output, by establishing a more geographically accurate baseline for number of days per year. Some of the changes are strongly tied to that baseline, which in turn is strongly tied to topography. That is, locations where minimum temperature is rarely <32°F (southern Arizona, gulf coast) see only very small changes.

Table 6.2, specifically the fact that nearly half of the extremes presented here have gotten cooler, not warmer, does not support the assertions in Key Finding 2. Context should be provided to explain this discrepancy.

Tables 6.4 and 6.5 should include uncertainty ranges.

Are likelihood / confidence statements appropriate, and justified?

All key findings contain both a likelihood and confidence statement. Only one should be listed—probably the likelihood statement.

Are statistical methods applied appropriately?

No statistical significance of historical trends is provided. The Committee strongly recommends reporting past trends and future projected changes with a range of values using commonly accepted methods. See Section II.2 of this report for more detailed recommendations about the treatment of trends and statistics. Figures and tables should show statistical significance of changes in temperature. Text describing projected temperature changes, including captions, should indicate the number of models or simulations used to calculate the average change.

Is the chapter balanced? Are there areas that should be expanded, or removed?

For the most part, the chapter is balanced in the topics covered, with noted exceptions. The Committee suggests additional discussion of changes in daytime high temperature vs. nighttime low temperatures, that are consistent with the recommendations in Chapter II about extreme events.

Recommended changes to structure

As part of the restructuring recommended for Chapter 3, some of the attribution information could be moved to Section 6.2.

III.7 CHAPTER 7: PRECIPITATION CHANGE IN THE UNITED STATES

Summary

This chapter is structured with a series of subsections that address historic changes (annual and seasonal, then snow, extremes, extratropical cyclones, and detection and attribution) and a second series of subsections that address projections (seasonal means, snow, extremes, and hurricanes). This structure is easy to follow, and addresses the main topics. Given the importance of precipitation to water resources and hazardous extremes and the reality that these will be among the costliest manifestations of climate change, it is appropriate that the CSSR authors have broken out snow, as well as extratropical cyclones and hurricanes, as separate sections. Noting that Chapter 8 discusses drought, since drought is mentioned numerous times in Chapter 7, would also be helpful.

The Committee identified multiple sections where the chapter would benefit from further clarification and discussion of the breadth of available literature. The use of different historical periods in Section 7.1 is confusing for the reader. Some of this may be unavoidable given that results are reported from many publications that have made their own decisions as to historic periods. Nonetheless, the Committee suggests trying to identify trends over the last century (more or less), and the period of greatest GHG emissions, roughly the last 40-50 years. In some cases, it may be possible to replot results of others for these periods, or at least provide an interpretation that maps to these periods (or others that are defensible). Regardless, the time period evaluated should be clearly stated for all analyses. Additionally, the text is inconsistent in use of “ramp” vs “step” trends (see also Section II.2). For instance, Figure 7.1 uses a step, which implies trend magnitudes that are half what they would be using a ramp. In most cases, ramp is preferable since manifestations of climate change occur gradually over time, with an exception being when some event may have caused an abrupt shift.

Chapter 7 seems to overstate the evidence for changes in precipitation extremes. For instance, the cited Westra et al. (2013) paper, reports that the number of statistically significant upward trends is larger (by a factor of 4 or so over the CONUS) than downward trends, but less than 9% of trends are statistically significant and upward and 5% would be expected due to chance. This suggests there may only be “weak evidence of increases in extremes” and the Committee recommends revising the text to better reflect the findings of relevant literature.

The snowpack discussion in this chapter focuses primarily on snow cover extent and lacks adequate discussion of snow water equivalent (SWE). Particularly over the West, where much of the annual runoff originates as snowpack, SWE in the springtime is critically important for hydrology and water resources, with snow cover extent being a much less important factor. The chapter should include some information about long-term SWE trends and increase discussion of this topic in the context of projections. There is recent work based both on observations and historical model reconstructions that could also be cited (e.g., Mote et al., 2016, Mao et al., 2015, and Margulis et al., 2016). An expanded section on snowpack, particularly SWE, could either be retained in this chapter or moved to Chapter 8, but should appear with a more comprehensive discussion in one.

SPECIFIC REVIEW COMMENTS RELATED TO THE STATEMENT OF TASK

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

The chapter reflects the scientific literature reasonably well. Addressing the gaps noted previously with respect to precipitation change that affect hydrology will improve the chapter balance. For precipitation extremes, the Committee suggests reviewing the report, “Climate Stabilization Targets: Emissions, Concentrations, and Impacts over Decades to Millenia” (NRC, 2011). Although not as recent as some available literature, this publication addresses the topic and may be appropriate to include. The Committee did not think that any critical content areas were missing from the report.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

Generally, the traceable accounts require the inclusion of more details about the science supporting the key findings and references to the literature. As written, there is not enough information to follow the line of evidence that underpins the findings. For example, Key Finding 3 points vaguely to "climate model projections and our understanding" which, combined with section 7.2.2, is insufficient to document how the calculations for Figure 7.7 were done in support of this key finding.

> **Key Finding 1:** There are sizeable regional and seasonal differences in precipitation changes since 1901. Annual precipitation has decreased in much of the West, Southwest and Southeast, and increased in most of the Northern and Southern Plains, Midwest and Northeast. A national average increase of 4% in annual precipitation since 1901 is mostly a result of large increases in the fall season. (*Medium confidence*)

The Committee suggests deleting the first sentence of this key finding. The core of the finding is stated in subsequent sentences, and the fact that precipitation has increased slightly over the last century is primarily attributable to large scale droughts in the 1930s and 1950s. There are, however, important regional differences. The finding should also state the nature of changes over the post-1970 period, as noted previously.

> **Key Finding 2:** Heavy precipitation events across the United States have increased in both intensity and frequency since 1901. There are important regional differences in trends, with the largest increases occurring in the northeastern United States. (*High confidence*)

This finding would be strengthened by focusing more specifically on the observation that, over the last century, heavy precipitation has increased in intensity and duration at a small, but statistically significant, number of stations. For stations where changes have been observed, a substantial fraction (about 80%) have been increases. The word 'across' implies ubiquity and therefore may not be the appropriate word choice. The finding should also include a statement about post-1970 trends.

> **Key Finding 4:** Northern Hemisphere spring snow cover extent, North America maximum snow depth, and extreme snowfall years in the southern and western United States. have all declined while extreme snowfall years in parts of the northern United States. have increased (*medium confidence*). Projections indicate large declines in snowpack in the western United States and shifts to more precipitation falling as rain than snow in the cold season in many parts of the central and eastern United States (*high confidence*).

This key finding would be much more impactful if it focused primarily on CONUS (and perhaps Alaska), and on SWE rather than snow depth and extent. As written, it is not supported by any figure or table, although Figure 8.3 could be relevant but is not mentioned here. See also the Summary for this chapter.

Are graphics clear, and do they appropriately reflect the major points in the text?

Generally, yes, however, additional detail is needed for some figures. For instance, Figure 7.7 (and others) would benefit from a more informative title and labeling of the y-axis. As currently displayed, it is very difficult to interpret. Also, how many of the CMIP5 models are represented in Figure 7.7? The across-model variations seem low.

In Figure 7.8, the spatial variability in projected changes also seems low and, if correct, bears explanation. It should also be noted for this figure whether it makes a difference if the return period is different. For the extreme value distribution EV1, the quantiles are just a fixed multiple of the mean, so changes in the mean are proportionately reflected in changes at any given return period. While the same does not apply for other distributions, it may well be approximately true, so perhaps something could be said about how other return periods change.

Chapter 7 would benefit from tables equivalent to those Chapter 6 (Tables 6.1, 6.2, 6.4, and 6.5). Chapter 6 also noted that there were differences in changes in extremes, depending on which extremes were considered. A table showing changes for 3-4 definitions of extreme precipitation would be helpful, or a strong justification for selecting only the 2-day 5-year event for the bar charts in Figure 7.7 and 1-day 20-year event for the map in Figure 7.8 .

A figure illustrating changes in snow cover extent could also be useful to this chapter and Figure 7.5 could be moved to Appendix B in the draft CSSR.

Are likelihood / confidence statements appropriate, and justified?

For the most part, yes, they seem appropriate.

Are statistical methods applied appropriately?

As detailed earlier, trends should include statistical significance statements whenever possible throughout the chapter.

Is the chapter balanced? Are there areas that should be expanded, or removed?

The chapter is reasonably well balanced.

III.8 CHAPTER 8: DROUGHTS, FLOODS, AND HYDROLOGY

Summary

This chapter is organized differently from Chapter 7, from which it logically follows. Chapter 7 includes first a historical context (basically trends) in the different subtopics then projections for each. The Committee recommends this structure also be used for Chapter 8 to provide a clear picture of what has been happening over about the last century, and what is projected to happen in the future. Also, wildfire (Section 8.3) does not fit naturally with the subject of the chapter as represented by the title, and probably belongs elsewhere in the report, perhaps Chapter 10. Finally, the title implies a rigorous consideration of hydrology (i.e. full hydrological cycle, including for instance groundwater). While there are well defined subsections for droughts and floods, there is not for hydrology, creating a structural mismatch with the title. Perhaps the chapter could include some brief narrative about what is meant by ‘hydrology’ in this context, point out what is not covered, or revise the title to better reflect the chapter content.

A number of substantial improvements are strongly recommended for Chapter 8 beyond these organizational suggestions. The Committee recommends that the chapter authors consider consulting with hydrologic experts to assist in revising this chapter. More extensive input from researchers with such expertise would help ensure that the final text is more authoritative and balanced.

Most of the primary recommendations for this chapter are framed through the content presented in the key findings. Revising the chapter text to reflect these recommendations given for key findings will help to strengthen the chapter.

> **Key Finding 1:** Recent droughts and associated heat waves have reached record intensity in some regions of the United States, but, by geographical scale and duration, the Dust Bowl era of the

> 1930s remains the benchmark drought and extreme heat event in the historical record. (*Very high confidence*)

Key Finding 1 does not fully reflect the science regarding trends in droughts. While some specific regions have experienced recent droughts of record intensity, analysis of global and continental-scale trends indicates that drought severity and other statistics have actually declined (e.g. Sheffield et al., 2012, Andreadis and Lettenmaier, 2006, and Mo and Lettenmaier, 2015). Recent research finds that over about the last 100 years, slight increases in precipitation (which are noted in Chapter 7) have overcome increased evapotranspiration (ET), resulting in generally increased soil moisture (Andreadis and Lettenmaier, 2006). Also, low flows (another indicator of drought) have become less common across much of the country, as documented in references such as Lins and Slack (1999 and 2005), as well as other U.S. Geological Survey publications which could be cited and discussed.

> **Key Finding 3:** Future decreases in surface soil moisture over most of the United States are *likely* as the climate warms. (*High confidence*)

Key Finding 3 does not accurately reflect the current state of understanding about the linkage between soil moisture and temperature. Changes in soil moisture depend entirely on the balance between precipitation change and ET changes (presumably increases). A common misconception, which is reflected in some of the work on drought, is that potential evapotranspiration is strongly related to temperature, and hence temperature increases result in strong increases in ET. However, ET over most parts of the United States is dominated by net radiation, which in turn is dominated by solar radiation, which is not temperature dependent. Other factors that influence ET could be affected by warming and other climate trends, in particular, vapor pressure deficit and longwave radiation, are temperature dependent and solar radiation depends on cloud cover. Terms related to vapor pressure deficit are also controlled by wind, and there are studies showing that near-surface wind speeds generally have been going down. The potential of changes in these factors to influence future ET are not well understood yet, making it difficult to make statements about future soil moisture with high or even medium confidence.

> **Key Finding 4:** Reductions in western U.S. winter and spring snowpack are projected as the climate warms. Under higher emissions scenarios, and assuming no change to current water-resources management, chronic, long-duration hydrological drought is increasingly possible by the end of this century. (*Very high confidence*)

The magnitude of projected snowpack decreases in Key Finding 4 may be understated. The draft CSSR could reasonably use words like “substantial”, as virtually all projections show large decreases in snowpack by mid-century. This key finding could also be strengthened by framing this topic as a change in an annual pattern rather than an episodic change (which is how droughts are typically framed). Further, including a sentence about how runoff timing and volumes are expected to change and how this change is linked to natural storage in the snowpack would improve this key finding and could be stated with high confidence.

> **Key Finding 5:** Detectable increases in seasonal flood frequency have occurred in parts of the central United States. This is to be expected in the presence of the increase in extreme downpours known with high confidence to be linked to a warming atmosphere, but formal attribution approaches have not certified the connection of increased flooding to human influences. (*Medium confidence*)

Findings concerning trends in flooding are highly complex and spatially variable and this key finding could be improved by revising the text to specifically articulate this. Within the existing literature, few locations show statistically significant changes in flooding nor have they been clearly linked to precipitation or temperature. Generally, a mixture of downward trends and upward trends are observed (e.g. Lins and Slack, 1999 and 2005) and when upward trends are observed, it has been shown for a relatively small proportion of measurement stations and other factors, including land cover, have been found to contribute to observed patterns (Vogel et al., 2011). There is some evidence of upward trends in precipitation extremes, but essentially none in floods, and this remains an outstanding research issue.

Additional Chapter-Level Summary Recommendations

This chapter could include a finding focused on snowpack and associated seasonal runoff timing changes, especially across the West. While this is not new, it is well understood and has clear hydrologic consequences. As shown in Mote et al. (2016), the exceptionally low spring 2015 snowpacks were pervasive across the West. Such conditions may become the norm in future decades. This important contributor to water scarcity has not only been detected, but also attributed to human-caused climate change. Mention of this observation-based finding before discussion of related future projections would improve this chapter.

The discussion of the California drought and attribution would be more appropriately balanced by including of additional literature and stronger recognition of the known complexities and outstanding research questions. Collectively, existing studies do not use a sufficiently consistent formulation to lay out a clear case for attribution and this should be stated (e.g., see Swain et al., 2014, Wang and Schubert, 2014, and Funk et al., 2014). The California drought is also unusual, as observed in the exceptional warmth in the winters of 2013-4 and 2014-5, especially the latter. This raises the question, as yet unanswered, of whether droughts in the western United States are shifting from precipitation control (as shown by Mao et al., 2015) to temperature control. There is some evidence to support a relationship between mild winter and/or warm spring temperatures and drought occurrence (Mote et al., 2016). This is a topic that could be addressed more strongly, with a view to changes in the full hydrologic cycle, which receives little coverage in this chapter otherwise.

Specific Review Comments Related to the Statement of Task

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

The Committee thinks that this chapter needs to provide a more comprehensive overview of the state of understanding of hydrologic change as documented in the literature. Addressing the gaps detailed throughout this chapter review will considerably improve the impact of this chapter.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

Some of the key findings should be revised, as described in earlier comments on this chapter.

Are graphics clear, and do they appropriately reflect the major points in the text?

Concerns noted above and in the Chapter 7 review for figures also pertain here. More specifically, for Figure 8.1, the Committee recommends using a more accepted method of showing variations in soil moisture in multi-model settings. One such approach is to use soil moisture percentiles rather than the raw model output. This approach better recognizes that inter-model differences are large, which is difficult to capture in the current figure, where the range in change is small and generally within the range of variability among models.

For both Figures 8.1 and 8.2, why distinguish between “small compared to natural variations” and “inconclusive”? Recommend simplifying and using stippling only.

The Committee recommends replacing Figure 8.3 with an off-line land surface model run with bias corrected inputs, which will represent elevation effects much better and remove the considerable GCM biases. Or, include other simulations, perhaps with hydrologic models, if available.

As part of the revisions recommended for this chapter, the Committee suggests identifying new figures that reflect the revised text.

Are likelihood / confidence statements appropriate, and justified?

See previous comments for recommendations to improve likelihood/confidence statements associated with the key findings.

Are statistical methods applied appropriately?

Throughout this chapter, greater statistical context, particularly that on historical trends and attribution, would strengthen the chapter.

Is the chapter balanced? Are there areas that should be expanded, or removed?

The chapter requires a more robust discussion of the hydrologic context in order to accurately represent the hydrology component named in the title.

Recommended changes to structure

As stated in the Summary comments, the Committee thinks that it would be more effective to use the Chapter 7 structure with historical trends first, then projections.

III.9 CHAPTER 9: SEVERE STORMS

Summary

The Committee commends the authors for producing a very strong draft chapter. Minor revisions are described in this section, but no major concerns about the chapter were raised.

Specific Review Comments Related to the Statement of Task

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

The Committee thinks that for the most part the chapter accurately reflects the scientific literature, with one important exception. References to “challenging the IPCC AR5 consensus” with regard to findings in changes in tropical cyclone (TC) intensity and frequency might be overly broad. It appears that only the findings on frequency are subject to a qualitative challenge, since the first such “challenge” (page 311, lines 1-3) seems to question only the magnitude but not the sign of the hypothesized relationship between warming and intensification of TCs.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** Human activities have contributed substantially to observed ocean-atmosphere variability in the Atlantic Ocean (*medium confidence*), and these changes have contributed to the observed increasing trend in North Atlantic hurricane activity since the 1970s (*medium confidence*).

The Committee recommends preceding this with an appropriate statement describing observed trends in TC properties in the North Atlantic. Without this, the relatively low confidence in attribution might be confused as low confidence in detection. It is important to be clear about the difference. For example, IPCC AR5 2013 was very confident in the existence of a trend in TC activity on the North Atlantic.

> **Key Finding 2:** For Atlantic and eastern North Pacific hurricanes and western North Pacific typhoons, increases are projected in precipitation rates (*high confidence*) and intensity *(medium confidence)*. The frequency of the most intense of these storms is projected to increase in the Atlantic and western North Pacific (*low confidence*) and in the eastern North Pacific (*medium confidence*).

The Committee suggests adding an appropriate statement about expected trends in overall number (frequency) of TCs. The chapter language on page 309, lines 20-22 is particularly effective, and could be included as part of this key finding: "Both theory and numerical modeling simulations (in general) indicate an increase in TC intensity in a warmer world, and the models generally show an increase in the number of very intense TCs."

> **Key Finding 3:** Tornado activity in the United States has become more variable, particularly over the 2000s, with a decrease in the number of days per year experiencing tornadoes, and an increase in the number of tornadoes on these days (*high confidence*). Confidence in past trends for hail and severe thunderstorm winds, however, is *low*. Climate models consistently project environmental changes that would putatively support an increase in the frequency and intensity of severe thunderstorms (a category that combines tornadoes, hail, and winds), especially over regions that are currently prone to these hazards, but confidence in the details of this increase is *low*.

The Committee is concerned that confidence in observed tornado trends may be less than "high", owing to, e.g., issues of shifting completeness of observational network. If high confidence is in fact warranted, the Committee suggests adding some supporting information in the traceable account. As written, it is unclear how the traceable account supports the key finding, as it appears to be internally inconsistent. Compare, for example, page 321, lines 24-25 ("virtually all studies") with page 310, line 28 "medium confidence that [human factors] contributed"…

Are graphics clear, and do they appropriately reflect the major points in the text?

The figures are a weak point in an otherwise strong chapter and the Committee recommends significant revisions.

Figure 9.1 has limited relevance, as it pertains to the western north Pacific region. If an effective figure pertaining to the North Atlantic can be found, that might be more useful.

In Figure 9.2, the only results of any apparent statistical significance pertain to the western Pacific region and thus are of relatively limited interest for this United States-focused draft CSSR. The results for locations near the continental United States appear to show very small differences having no statistical significance. If this is wrong, the Committee recommends providing supporting information, for example 95% confidence limits, on the differences. It appears that those limits are very broad, meaning that the range of possible trends is very large—probably so large as to not constrain things enough to be interesting.

In Figure 9.3, it is unclear whether the apparent trend seen in the red curve is statistically significant. If it is possible to provide information supporting its statistical significance, the Committee recommends doing so.

Figure 9.4 could be improved by removing some panels and enlarging others. The upper right panel is too small to read easily. This could be remedied to some extent by zooming in on the United States. The lower left panel could be deleted, as it appears to be simply a map of measured extreme precipitation events with an editorial comment about atmospheric rivers (ARs) and the same point is made more effectively in the bottom right panel.

Are likelihood / confidence statements appropriate, and justified?

Yes, the statements appear appropriate and justified.

Are statistical methods applied appropriately?

Yes, statistical methods appear to be applied appropriately.

Is the chapter balanced? Are there areas that should be expanded, or removed?

Yes, the chapter is balanced.

Recommended changes to structure

The authors should consider how and where different types of extreme precipitation and flooding are covered in the draft CSSR and ensure linkages across chapters. Chapter 9 mentions flood risk associated with ARs, but the chapter on flooding (Chapter 8) does not discuss risk associated with ARs. Chapter 9 also covers convective storms and ARs and it would be good to point out that Chapter 7 provides a complete discussion of variability in precipitation, irrespective of specific physical mechanism(s) driving that variability. Mechanisms of variability are important when it comes to improving understanding, but given the draft CSSR is intended to inform NCA4 description of impacts, it is important to quantify as well as possible variability on all time scales, irrespective of physical cause. Finally, it would also be beneficial to indicate that floods due to storm surge are covered in Chapter 12, Sea Level Rise.

Projections of ARs indicate greater frequency and intensity. Does this translate to increased precipitation in California? The text blurs some of the important differences between ARs in California, where they can increase snowpack, and ARs in the Northwest, where they almost invariably remove snowpack.

The box about the "hurricane drought" is good. Is there a corresponding discussion elsewhere about how this might affect preparedness?

III.10 CHAPTER 10: CHANGES IN LAND COVER AND TERRESTRIAL BIOGEOCHEMISTRY

Summary

This chapter covers a great deal of ground, and generally does a good job describing the state of science in many of its topics. Many of these areas have seen advances in science and conceptual understanding since NCA3, and certainly since the IPCC AR5. However, the Committee found many parts of this chapter problematic, and provides a number of suggestions for improving it here. These overarching comments are ranked here in roughly descending order of importance.

- The Key Findings are often not supported by the description of evidence base provided. They also do not match well with the chapter text, and are even inconsistent with it at times.
- The chapter puts too much emphasis on growing season length and albedo, and consistently plays down the direct effects of temperature and precipitation in driving ecosystem responses to climate change. The Committee recommends significantly condensing Sections 10.2.4 and 10.3.1, while more prominently acknowledging temperature and precipitation effects throughout.
- Following the last point: drought and tree mortality should be given a more in-depth discussion, given the extensive recent research and findings in this area, and the fact that this is one of the chapter's key findings.
- Throughout, the text is prone to vague and weak statements, sometimes with no clear connection to the information that the authors intend to convey: for example, page 337, lines 9-11, page 339, lines 13-14, page 344, lines 2-5, page 345 lines 1-3, page 346, lines 16-18. Text should be precise and clear. Structurally, paragraphs in this chapter frequently lack strong topic sentences and combine multiple topics, often in a confusing way.
- The chapter title does not match the chapter's content, as land use/land cover change is really only mentioned in the introduction and on page 342.
- None of the chapter's key findings appear in the ES.

Specific Review Comments Related to the Statement of Task

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

The Committee thinks that the chapter generally accurately reflects the scientific literature in specific areas, and that no critical content areas are missing from the draft report. However, the discussion of some topics in the report should be expanded, while the emphasis on others should be reduced (see Summary), and better linkages to the key findings are needed.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** Changes in land use and land cover due to human activities produce changes in surface albedo and in atmospheric aerosol and greenhouse gas concentrations. These combined effects have recently been estimated to account for 40% ± 16% of the human-caused global radiative forcing from 1850 to 2010 (*high confidence*). As a whole, the terrestrial biosphere (soil, plants) is a net "sink" for carbon (drawing down carbon from the atmosphere) and this sink has steadily increased since 1980, in part due to CO_2 fertilization (*very high confidence*). The future strength of the land sink is uncertain and dependent on ecosystem feedbacks; the possibility of the land becoming a net carbon source cannot be excluded (*very high confidence*).

The description of evidence base provided for Key Finding 1 seems to be referring to albedo effects only. Since the finding is about both albedo and GHG effects, the evidence should also address both. Note that this type of concern is a recurring one throughout this chapter.

Also, Key Finding 1 and Figure 10.2 seem somewhat inconsistent with the information in Figures 2.3, 2.6, and 2.7. In particular: no reason is given for starting in 1850 instead of 1750; the reader's attention is not directed to Chapter 2; and the method of partitioning each contribution into LULCC and non-LULCC is not stated. The partition shown for CO_2 is plausible given Figure 2.7, but only if the enhanced land carbon sink is ignored. Agriculture is a major source of N_2O but this is true even if land use were not changing to increased arable land, so it seems a stretch to ascribe all N_2O emissions to LULCC. The discussion of nitrogen on page 341 does not address N_2O emissions or their relationship to LULCC. In summary, basing Key Finding 1 on one study (page 342, line 19) should constitute low confidence.

> **Key Finding 2:** The increased occurrence and severity of drought has led to large changes in plant community structure with subsequent effects on carbon distribution and cycling within ecosystems (for example, forests, grasslands). Uncertainties about future land use changes (for example, policy or mitigation measures) and about how climate change will affect land cover change make it difficult to project the magnitude and sign of future climate feedbacks from land cover changes. (*High confidence*)

There is a major mismatch between this key finding, which is about the past, and the description of the evidence base, which is about the future. For this reason, the description of the evidence base is incomplete and a more thorough description of the data, evidence, and relevant studies should be included. In addition, there is strong evidence for impacts of drought on plant community structure, but the evidence for “increased occurrence and severity of drought” is not presented and not clearly supportable. Note also that, as described in Section III.8, it is far from clear that there is really an “increased occurrence of drought”. Additionally, the tone of Key Finding 2 is essentially opposite that of Key Finding 1. Key Finding 1 says the land is a net carbon sink and Key Finding 2 says drought is having an impact. Both can be right, but the juxtaposition requires explanation. Finally, this key finding could be better linked to the more extensive treatment of drought in Chapter 8.

> **Key Finding 3:** Since 1901, the consecutive number of both frost-free days and the length of the corresponding growing season has increased for all regions of the United States. However, there is important variability at smaller scales, with some locations showing decreases of as much as one to two weeks. Plant productivity has not increased linearly with the increased number of frost-free days or with the longer growing season due to temperature thresholds and requirements for growth as well as seasonal limitations in water and nutrient availability (*very high confidence*). Future consequences of changes to the growing season for plant productivity are uncertain.

This key finding is mostly about climate variables (length of the frost-free season) while the evidence is about ecosystem responses. One cannot conclude that the evidence supports the finding.

> **Key Finding 4:** Surface temperatures are often higher in urban areas than in surrounding rural areas, for a number of reasons including the concentrated release of heat from buildings, vehicles, and industry. In the United States, this urban heat island (UHI) effect results in daytime temperatures 0.9°–7.2°F (0.5°–4.0°C) higher and nighttime temperatures 1.8°– 4.5°F (1.0°–2.5°C) higher in urban areas, with larger temperature differences in humid regions (primarily the eastern United States) and in cities with larger populations. The UHI effect will strengthen in the future as the spatial extent and population of urban areas grow. (*High confidence*)

This key finding includes a very thin description of the evidence base that does not really support the assertions made in the finding. It should be expanded and clarified, or the key finding should be deleted.

Are graphics clear, and do they appropriately reflect the major points in the text?

The graphics are generally clear, but Figures 10.1 and 10.2 are probably not both necessary, and one might be replaced by a table. Discussion of figures in the text; particularly Figure 10.2, is too brief and should be expanded on for the figure to provide value to the chapter. Regardless, figures require better explanation in their captions. For Figure 10.1, "LULCC" should be defined and captions in general need to be clearer and more informative. The Figure 10.2 caption should refer to Figure 2.3 since the Myhre et al. (2013) forcings are shown, and include more detail.

Are likelihood / confidence statements appropriate, and justified?

Yes, the statements are appropriate and justified.

Are statistical methods applied appropriately?

The Committee is concerned about ad hoc time period choices and unqualified assertions of trends. This is addressed in general comments about the entire draft report (see Section II.2).

Is the chapter balanced? Are there areas that should be expanded, or removed?

The chapter could be better balanced, as detailed earlier in Section III.10.

III.11 CHAPTER 11: ARCTIC CHANGES AND THEIR EFFECTS ON ALASKA AND THE REST OF THE UNITED STATES

Summary

Some of the global consequences of climate change in the Arctic are potentially catastrophic and irreversible. There may also be physical thresholds beyond which these consequences become inevitable (even if they might unfold over centuries). For these reasons, this topic has both importance and policy urgency, and a thorough treatment in the draft CSSR is important. The third-order draft of this chapter is a sound foundation, and the Committee encourages the authors to consider the following points as they revise the chapter.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

> **Key Finding 1:** For both the State of Alaska and for the Arctic as a whole, near-surface air temperature is increasing at a rate more than twice as fast as the global-average temperature. (*Very high confidence*)

This key finding needs to be supported by stronger evidence than is currently provided on page 371, lines 26-35. As written, it contains illogical and confusing reasoning and contradictory conclusions. Are the satellite observations of the middle troposphere or the surface temperature? If longer records indicate that decadal variability dominates, why base Key Finding 1 on a study of temperature change since 1981? A strong topic sentence that summarizes the main message would help, instead of starting with "Satellite observations". Additional detail should also be provided in the traceable account.

> **Key Finding 3:** Arctic sea ice and Greenland Ice Sheet mass loss are accelerating and Alaskan mountain glaciers continue to melt (*very high confidence*). Alaskan coastal sea ice loss rates exceed the Arctic average (*very high confidence*). Observed sea and land ice loss across the Arctic is occurring faster than climate models predict (*very high confidence*). Melting trends are expected to continue resulting in late summers becoming nearly ice-free for the Arctic ocean by mid-century (*very high confidence*).

This key finding discusses sea ice projections, but the description of evidence base mentions only observations. Presumably the projections are based in some way on CMIP5 simulations, but specific literature should be cited.

> **Key Finding 4:** Human activities have contributed to rising surface temperature, sea ice loss since 1979, and glacier mass loss observed across the Arctic. (*High confidence*)

The confidence level associated with Key Finding 4 seems low and the Committee recommends evaluating whether a higher confidence level might be appropriate. In either case, a more transparent reasoning process should be laid out in the traceable account. Recent work by Kirchmeier-Young et al. (2016) may also be relevant to cite here.

> **Key Finding 5:** Atmospheric circulation patterns connect the climates of the Arctic and the United States. The mid-latitude circulation influences Arctic climate change (*medium to high confidence)*. In turn, current evidence suggests that Arctic warming is influencing mid-latitude circulation over the continental United States and affecting weather patterns, but the mechanisms are not well understood (*low to medium confidence*).

There is universal recognition that Arctic influence on mid-latitude weather is an area of active research (as pointed out in the draft CSSR) and the Committee supports including some discussion of this topic in the chapter, with citation of the full breadth of current research perspectives on this linkage. However, because no scientific consensus on this topic has been reached, the Committee strongly recommends removing Key Finding 5, so as to not place disproportionally high emphasis on a topic where there is currently little confidence.

Introduction: The second to last paragraph of the introduction mentions unique challenges associated with improving understanding of the Arctic. This is appropriate, but the Committee is concerned that this might leave the reader with the impression that we do not know enough to usefully inform policy, which is not the case. The final paragraph in the Introduction (page 371, lines 14-15) would be strengthened by stating not only that our understanding is improving, but also that it is advanced enough at present to effectively inform policy. It may also be worthwhile to explicitly state that Alaska is in the Arctic, making the United States an Arctic nation (the first sentence implies that it is not).

Permafrost: GHG emissions from thawing permafrost are an important mechanism by which the Arctic affects the rest of the planet. With this in mind, the key finding on GHG emissions from thawing permafrost should be stronger. Saying only that "The overall magnitude of the permafrost-carbon feedback is uncertain" is, while strictly true, not helpful. While the Committee recognizes that these emissions are quite uncertain, it is clear that these emissions have the potential to complicate our ability to meet policy goals like limiting warming to 2°C, as is a target in the Paris Agreement. This should be stated. Further, the discussion of permafrost should be separated from discussions of snow cover and methane hydrates, with the entire discussion of permafrost provided in one contiguous section. The report emphasizes methane release from permafrost, which may not be appropriate. While permafrost is a source of methane, the text should explicitly note that at present, research indicates that more carbon is released from permafrost as CO_2 than as methane. Finally, it is important to be sure that GHG emissions from thawing permafrost are considered consistently throughout the draft CSSR. In particular, the discussion of remaining allowable emissions consistent with meeting the 2°C goal (ES, page 27, lines 17-24) appears not to consider these emissions. The Committee considered this to be an important oversight. Similarly, the discussion of permafrost in Chapters 1 and 15 should be revisited in light of the above comments to ensure consistency.

Greenland Ice Sheet: Discussion of Greenland Mass Balance here overlaps substantially with Chapter 12, which provides a more thorough overview. The Committee recommend trimming this passage and referring to the equivalent in Chapter 12.The discussion that is provided in Chapter 11 is overly focused on recent observed trends in ice sheet mass loss. While this is an important topic, there should also be a short discussion of future trends. For example, is there a threshold beyond which eventual complete melting becomes inevitable? Do we know where this threshold is? (e.g. see Robinson et al., 2012). If so, how long would complete disintegration take? The implications of Greenland ice sheet mass loss for SLR and potential impacts on ocean circulation should be mentioned and linked to more detailed discussion elsewhere in the report, including that of model sensitivity in Chapter 15 and as previously noted, Chapter 12.

Sea ice extent: The projection is made that the Arctic will become 'ice free (in summer) by mid-century.' It is further stated that "natural variability…future emissions, and model uncertainties … all influence sea ice projections." This last statement is indisputable, but it would be helpful if something more specific could be said about the importance of future emissions on the fate of summer sea ice. In other words, how much control do we have (in principle) over whether and when summer Arctic sea ice disappears?

Arctic connections to mid-latitude weather: This is characterized in Chapter 9 as low confidence and low to medium confidence in Chapter 11. Regardless of which of these is most appropriate, the draft CSSR should be internally consistent. See the more detailed recommendation on this topic provided with Key Finding 5 earlier in this section.

III.12 CHAPTER 12: SEA LEVEL RISE

Summary

This is a strong chapter. It is well written, uses graphics effectively, and provides an excellent, comprehensive overview of the individual factors contributing to SLR, with particular emphasis on its spatial heterogeneity. The chapter represents a substantial departure from previous assessments of SLR (including the NCA3), and represents a substantial advance relative to previous U.S. sea level assessments. Another particular strength of the chapter is the outlook beyond the year 2100. The Committee thinks that the potential rates of global mean sea level (GMSL) rise in the next century should also be discussed, because they are in the >cm/yr range, which poses particular challenges for coastal infrastructure, etc.

Notable changes relative to previous work include the revision of future GMSL scenarios, in line with the recent findings of the U.S. Interagency Sea Level Task Force (Sweet et al., 2017). The new scenarios now consider six discrete GMSL trajectories, in comparison with four used previously. In a further departure from previous assessments, the new, individual sea level scenarios are placed in context with published probabilistic projections of future sea level following standard RCP emissions scenarios (e.g., Kopp et al., 2014, 2016). The chapter considers and contextualizes the latest results from ice-sheet modeling that includes physical processes not previously considered at the ice-sheet scale. The chapter also breaks new ground relative to previous reports by providing some regional guidance on the expected departure of future relative SLR around the North American coastline, relative to GMSL estimates. This regionalized analysis also includes guidance on evolving recurrence probabilities of high water (flood events), which is particularly useful.

No fundamental deficiencies were found, however the Committee did raise several issues that should be addressed to improve the presentation of the material and overall clarity of the chapter, or highlighted and given even more emphasis.

- The Committee recommends considering the advantages of reducing the dates/time intervals in use (perhaps focusing on 1900 to 2000, and 1993 to present), if possible, for greater consistency. This would serve to simplify comparisons of past SLR with the tabulated future sea-level estimates expressed relative to the year 2000. The need for consistent, simple time intervals applies to the entire chapter, including Section 12.4.2, which mixes discussions of post-1970s and post-1900 eras.
- The elevated sea level (6-9.3m) during the Last Interglacial provides a powerful message that the polar ice sheets are sensitive to modest warming. Adding some discussion that sea level was likely even higher during previous interglacials, including MIS-11 (~400ka), when GMSL might have been 6-13m higher than today (Raymo and Mitrovica, 2012), and likely even higher still during the Pliocene (~3 million years ago; Rovere et al., 2014) could be considered. The Committee strongly recommends moving Figure 4.3 to Chapter 12, where it would be more effective and illustrative of GMSL sensitivity to past warming. Removing the CO_2 values from Figure 4.3, to avoid complications associated with the influence of orbital versus GHG radiative forcing during these past time periods is also recommended.
- While the accelerating rate of GMSL rise since the late 20th century is described in the chapter, it is an important statement that could be emphasized further. This also applies to the notion that loss of land ice is overtaking thermosteric effects as the primary contributor.
- The chapter does a nice job of illustrating the radically different regional responses (fingerprints) to Greenland vs. Antarctic ice-sheet retreat (Figure 12.1). However, the simple notion that North America faces greater risk from ice loss in Antarctica than from ice loss in Greenland is not as simply and clearly stated as it could be. This point should be emphasized, because it relates directly to the subsequent discussion on the potential for drastic Antarctic ice loss.
- The Committee noted that the impacts of changes in land-water storage (past and projected) are not sufficiently covered, although the Committee acknowledges that the land-water storage component is relatively modest and is considered in the likely ranges of SLR based on Kopp et al. (2014). Some additional discussion on this topic could be helpful.
- Some of the “emerging science” described in the chapter (DeConto and Pollard, 2016, Golledge et al., 2015) shows that the loss of marine-based ice (in West Antarctic for example) is a long term (millennial timescale) commitment, due to the slow thermal response (cooling of the ocean). The effective “permanent” loss of marine-based ice would obviously have lasting/irreversible impacts on U.S. coastlines and should be mentioned.
- The spatial pattern of recent and ongoing thermosteric SLR (indirectly illustrated in Figure 12.2) is somewhat marginalized. While the potential for future impacts caused by thermal expansion is smaller than from ice sheet loss, the thermosteric effects are already impacting locations in the western Pacific with U.S. economic, strategic, and humanitarian interests; and will continue to do so regardless of ice sheet loss. A similar point can be made for the ocean dynamical effects on regional sea level, which seems underemphasized, relative to the potential impacts they could have along the U.S. East Coast.
- The Committee appreciates the cautious treatment of new ice sheet modeling that implies the potential for much higher SLR in coming decades and centuries than previously reported (e.g., DeConto and Pollard, 2016). While it is important for the draft CSSR to consider the full range of physically plausible SLR, this discussion could be balanced by also mentioning alternative modeling (e.g., Ritz et al., 2015) that implies more modest future SLR. While Ritz et al. (2015) do not directly account for the glaciological mechanisms considered by DeConto and Pollard (Marine Ice Sheet and Marine Ice Cliff Instabilities), their work does provide an alternative view

of Antarctica's potential contribution to future SLR that should also be mentioned for completeness.

Specific Review Comments Related to the Statement of Task

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

The Committee thinks that the chapter accurately reflects the current scientific literature on this topic, although the discussion of drastic Antarctic ice-sheet retreat could be broadened by comparing the recent results of DeConto and Pollard (2016) with Ritz et al., (2015) as already noted. Discussion of ocean heat content and influence on SLR should also be provided and appropriate, up-to-date references added.

In addition to the comments made previously, the Committee recommends an expanded discussion regarding the onset of anthropogenic influences on SLR, and further recommends that the authors consider enhancing their graphics to illustrate the anthropogenic contributions to past (and future) GMSL rise, and perhaps a breakdown of the relative contributions to GMSL from the individual processes and sources described in the report. This would provide an important update to Figure 13.1 in IPCC AR5 (Church et al., 2013).

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

The Committee compliments the overall clarity of the chapter, and the well-written background content on what causes global and relative SLR. The chapter could be improved by consistent treatment of time scales wherever possible.

> **Key Finding 1:** Global mean sea level (GMSL) has risen by about 8–9 inches (about 20-23 cm) since 1880, with about 3 of those inches (about 7 cm) occurring since 1990 *(very high confidence)*. Human-caused climate change has made a substantial contribution to GMSL rise since 1900 *(high confidence)*, contributing to a rate of rise faster than during any comparable period since at least 800 BCE *(medium confidence)*.

The Committee recommends the use of consistent time intervals in their discussion of past SLR, particularly avoiding mixing discussion of post 1880 and post 1900 GMSL in the same paragraph, if possible. In that case, the first sentence might read something like "Global mean sea level (GMSL) has risen by about 7.5 inches (about 19 cm) since 1900…". Some further discussion/clarification would be helpful, as to when in the 20th century the anthropogenic influence on GMSL began. The traceable accounts reflect the current state-of-the science, and confidence levels are appropriate.

> **Key Finding 2:** Relative to the year 2000, GMSL is *very likely* to rise by 0.3–0.6 feet (9-18 cm) by 2030, 0.5–1.2 feet (15-38 cm) by 2050, and 1 to 4 feet (30-130 cm) by 2100 *(very high confidence in lower bounds; medium confidence in upper bounds for 2030 and 2050; low confidence in upper bounds for 2100)*. Emissions pathways have little effect on projected GMSL rise in the first half of the century, but significantly affect projections for the second half of the century *(high confidence)*. Emerging science regarding ice sheet stability suggests that, for high emissions, a GMSL rise exceeding 8 feet (2.4 m) by 2100 cannot be ruled out.

The Committee thinks it is important to state that very high (>2.4m) SLR by 2100 is physically possible, but the language "cannot be ruled out" is vague, open to interpretation, and does not provide useful guidance. The traceable accounts reflect the current state-of-the art, and confidence levels are appropriate.

> **Key Finding 3:** Relative sea level (RSL) rise in this century will vary along U.S. coastlines due, in part, to: changes in Earth's gravitational field and rotation from melting of land ice, changes in

> ocean circulation, and vertical land motion (*very high confidence*). For almost all future GMSL rise scenarios, RSL rise is *likely* to be greater than the global average in the U.S. Northeast and the western Gulf of Mexico. In intermediate and low GMSL rise scenarios, it is *likely* to be less than the global average in much of the Pacific Northwest and Alaska. For high GMSL rise scenarios, it is *likely* to be higher than the global average along all U.S. coastlines outside Alaska *(high confidence)*.

This key finding lists several locally important processes in a way that diverts attention from the fact that if future ice loss is dominated by Antarctica (vs. Greenland), much of the U.S. coastline will experience considerably more relative SLR than the global average. The traceable accounts reflect the current state-of-the art, and confidence levels are appropriate.

> **Key Finding 5:** The projected increase in the intensity of hurricanes in the North Atlantic could increase the probability of extreme coastal flooding along the U.S. Atlantic and Gulf Coasts beyond what would be projected based solely on RSL rise. However, there is *low confidence* in the magnitude of the increase in intensity and the associated flood risk amplification, and it could be offset or amplified by other factors, such as changes in hurricane frequency or tracks.

Given the importance of long-duration winter storms on East Coast flooding in particular, the Committee recommends considering whether this key finding should be extended to include a comment on extratropical cyclones, in addition to Hurricanes. The traceable accounts reflect the current state of science, and confidence levels are appropriate.

Are graphics clear, and do they appropriately reflect the major points in the text?

The figures are generally clear and appropriately reflect the key points, although some specific recommendations are noted here.

As noted previously, the Committee recommends that Figure 4.3 (with the CO_2 values removed) be moved to Chapter 12, where it would be more effective at illustrating the potential sensitivity of the polar ice sheets to warming.

Figure ES.8 does not appear in Chapter 12 even though it shows SLR data. The Committee suggests that the figure be moved to Chapter 12, perhaps with a single representative city left as a figure in the ES, and discussed appropriately. Removal of the U.S. basemap would allow the individual time series to be expanded. At present, the axes on the individual panels are so small they are almost illegible. Furthermore, the y-axes should stop at 365, to reinforce that they are ‘days per year’ which is why the annual occurrences of daily flooding saturate near the end of the time-series, and a note added to the caption that this limit results in many of the curves having an inflection point. The choice of colors (blue and teal) could also be reconsidered for added clarity.

For Figure 12.2, panel labels ‘a’, ‘b’, and ‘c’ are missing, although they are mentioned in the caption. The Discussion of Figure 12.2c in the text could also better match the time period shown in the figure.

Figure 12.3 mixes meters and feet and should be edited to be consistent in the use of units.

Are likelihood / confidence statements appropriate, and justified?

Likelihood and confidence statements are generally appropriate and justified. The Committee also notes the importance of considering new science hinting at the potential for much higher future sea level than previously reported, but agrees that the confidence in this finding is still low and requires ongoing research. That said, the Committee questions the wording that GMSL >2.4m by 2100 “cannot be ruled out" as this is too open to interpretation and could be misconstrued as 'barely' possible, for example. Given the importance of this issue, this wording should be reconsidered.

Are statistical methods applied appropriately?

The Committee commends the blending of discrete sea level scenarios with a probabilistic approach and has no recommendations regarding the statistical methods used.

Recommended changes to structure

The chapter is well balanced, although the introduction is thinner than other chapters and some modest rewrite might be considered. No specific edits are recommended.

III.13 CHAPTER 13: OCEAN CHANGES: WARMING, STRATIFICATION, CIRCULATION, ACIDIFICATION, AND DEOXYGENATION

Summary

The ocean has received increasing attention in climate assessment reports. Following the lead of IPCC AR5WG1, the draft CSSR treats SLR and other ocean changes in separate chapters. As the title of this chapter suggests, there are many aspects of ocean changes that are important, both for their impacts on the ocean and its ecosystems, and also for impacts beyond the ocean. The Committee thinks that more effort could be devoted to linking this chapter to broader climate system changes. In particular, the role of the oceans in storing heat, and the link between changes in ocean heat content and changes in sea surface temperature could be discussed. In addition, the importance of ocean/atmosphere coupling in ENSO, mid-latitude storm tracks, and the thermohaline circulation, could also be better reflected in the text, and in turn the consequences of changes in ENSO for the United States and its territories (augmenting Chapter 5) could be emphasized, as is done in the Chapter 11 for the Arctic.

The chapter as a whole, including the key findings, was awkward to read even for those with knowledge of oceanography. Too many discipline-specific words or phrases are used with insufficient explanation. For example, the expression "ocean acidity" is used on a number of occasions, without any explanation of what it means. A number of words or phrases are explicitly noted in the Line Comments (Appendix A). Furthermore, it is rarely made explicit that any numerical value ascribed to a change in this parameter almost always refers to the surface ocean. The Committee recommends that this chapter be revised so as to improve consistency across the draft CSSR.

Specific Review Comments Related to the Statement of Task

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

The Committee felt that the chapter generally accurately reflects the scientific literature with two exceptions. First, Key Finding 1 represents an incomplete view of the evidence about changes in the Atlantic Meridional Overturning Circulation (AMOC). Second, the Committee suggests also noting the importance of changes in ocean properties (such as warming) for Antarctic ice sheet instability and SLR.

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

The evidence for changes in the AMOC mentioned briefly in the traceable accounts, appears to rely on a single study, and contains no quantitative statements to put the changes into context. Other studies reach different conclusions (e.g. Rhein et al., 2013), assessing the then-available literature, stated that "there is no evidence for a long-term trend." A fuller treatment of the issue is warranted, especially since it appears in the ES. This should include reference to more of the literature on this topic, including studies that emphasize the variability and challenges in assigning causes of AMOC trends. Moreover, if the 2 Sverdrup number is to be mentioned, it should be put into context with the total AMOC (e.g.,"may have slowed on the order of 10%").

> **Key Finding 1:** The world's oceans have absorbed more than 90% of the excess heat caused by greenhouse warming since the mid 20th Century, making them warmer and altering global and regional circulation patterns and climate feedbacks (*very high confidence*). Surface oceans have warmed by about 0.45°F (0.25°C) globally since the 1970s (*very high confidence*). The Atlantic meridional overturning circulation (AMOC) has slowed since preindustrial times (*high confidence*). Regionally, eastern boundary upwelling, such as along the U.S. West Coast, that sustains fisheries and controls local climate has intensified (*high confidence*).

Key Finding 1 contains many topics and should be split into multiple findings. The last statement that upwelling along the U.S. West Coast has intensified is difficult to reconcile with other statements on page 454 (lines 2 and 10), indicating a more mixed picture both in the past and for the future, especially given the apparent attribution statement. Hence, the level of confidence assigned to this finding seems too high.

> **Key Finding 5:** Under a high future scenario (RCP8.5), the AMOC is projected to decline by 6 Sverdrups (1 x 10^6 m^3/sec), global average ocean acidity is projected to increase by 100% to 150%) (*very high confidence*), and ocean oxygen levels are projected to decrease by 4% (*high confidence*) by 2100 relative to preindustrial values. Under a low future scenario (RCP2.6), global average ocean acidity is projected to increase by 35% and oxygen projected to decrease by 2% by 2100. Larger acidity increases and oxygen declines are projected in some regions and in intermediate and mode waters (*medium confidence*).

This key finding is not well grounded in the text. No specific details of the projected AMOC decline could be found in the chapter. The key finding also generalizes the projected average ocean acidity while the associated text focuses largely on the regional variability and the percent changes in acidity are not clearly traceable to the text. Little discussion is provided for the projections of the AMOC decline, with the text referring in places to the uncertainty of projections using earth system models. There appears to be an error in estimating the change in global ocean acidity between preindustrial time and 2100 that would result from scenario RCP8.5. Also, the unit "Sverdrup" appears nowhere in the document outside of this key finding and may not be appropriate for the intended reader and should be omitted. Finally, the traceable accounts should provide a more detailed summary of the information contained in the references provided.

Are graphics clear, and do they appropriately reflect the major points in the text?

The graphics are clear, but all three figures relate to changes in ocean chemistry, which constitute only two of the five topics named in the title. The legends generally lack some information needed to interpret the figures.

In Figure 13.1, there seems an excess of detail, although the legend still does not unambiguously describe the plots (i.e. It is unclear whether the green values refer to carbonate ion concentrations, or whether the x(CO_2) values are "wet" or "dry"). A citation for CO2SYS v2.1 should also be included.

The Figure 13.2 caption should be clear in stating that this is a change in surface ocean pH that has been estimated.

In the Figure 13.3 caption, the modeled density surface depicted should be included, as it is a key piece of information. This caption should also state that the data is on a particular density surface (26.5), as it was presented in the Long et al. (2016) source.

Are likelihood / confidence statements appropriate, and justified?

Yes, statements are appropriate and justified.

Are statistical methods applied appropriately?

Yes, statistical methods are appropriate.

Is the chapter balanced? Are there areas that should be expanded, or removed?

Mostly, however as noted above, the discussion of the AMOC is limited in scope and ocean heat content should be discussed. Also, the discussion of ocean acidification is somewhat confusing to those not in the field and would be improved by clarifying the various terms used. In particular, clarification that "acidity" is being used (apparently) as a synonym for hydrogen ion concentration, and "acidification" as an increase in that concentration is needed. Presumably the use of the term "corrosive" is not (as most might think) referring to a chemical damage to a metal, but rather implies the potential for dissolution of aragonite (the more soluble form of biogenic calcium carbonate)? Reference is also made to concepts such as "sensitivity to ocean acidification" or "buffering capacity" without explicitly stating what these terms mean. Additionally, as the various chemical mechanisms for such changes are not clearly described, the distinction between open ocean and coastal acidification is hard to follow. Perhaps a box describing these mechanisms could be added to help with this.

III.14 CHAPTER 14: PERSPECTIVES ON CLIMATE CHANGE MITIGATION

Summary

This chapter provides a concise overview of the key concepts that frame the challenge of limiting damage from climate change through a combination of mitigation and adaptation, and is a readable account of the implications of the Paris Agreement. The framing is mostly based on a "reluctant participant" model, where progress with mitigation stops when pre-determined commitments are reached. In contrast, the presentations from the United Nations Framework Convention on Climate Change (UNFCCC) more typically present decarbonization as a process with emissions reductions that become increasingly ambitious through time, as technologies improve and nations work through the experience of institutionalizing low-carbon societies. For the purposes of understanding mitigation pathways, a notable omission (e.g., in Figure 14.1) is a Paris-compliant scenario, i.e., one that has a >50% chance of stabilizing warming at less than 2°C.

The chapter's key findings largely miss the opportunity to make what could be the chapter's central point: a consequence of the essentially permanent nature of warming from CO_2 is that stabilization of CO_2 at any given concentration can only be achieved if CO_2 emissions fall to zero or become negative, to compensate for the remaining emissions of other GHGs and land-use change. Stabilizing warming at 1.5°C or 2°C requires emissions to fall to zero within a few decades, and even stabilizing warming at 3°C or 4°C requires zero emissions a few decades after that.

The absence of a focus on the need to drive CO_2 emissions to zero means that the chapter is not as clear as it might be on the range of emissions trajectories consistent with any temperature goal, illustrated in Figure 14.3. Specifically, it is important to emphasize the point that, for any mitigation goal, slower action in the near term requires more aggressive reductions or larger negative emissions later in the century. The linear relationship between cumulative emissions and warming creates the clearest entry point for understanding possible futures and especially for appreciating the motivation for reducing emissions to zero.

The chapter also misses an opportunity to add value by framing the mitigation challenge as one of managing risk, which has two dimensions. One is the risk of impacts at any level of warming. Here, links to Chapters 6-9 and 15 would be helpful. The other is the probability that a given emissions trajectory holds warming below a given goal. For the first dimension, the opportunity is largely in laying out the issues. This chapter, indeed this draft report, is appropriately focused on setting the stage for a thoughtful presentation of impacts. Still, the discussion can be more informative with a deeply grounded discussion of risk. The second dimension is central to the theme of Chapter 14. Without a clear presentation of the probabilities of reaching climate goals, the presentation of the emissions numbers has limited value. While it is not the responsibility of this draft report to define a “right” probability of meeting a goal, it is important to frame the discussion in a balanced way.

One of the biggest challenges in framing discussions of mitigation is striking a useful balance between discussion of CO_2 and other climate altering substances. The overall sense of the Committee is that the chapter puts less emphasis on short-lived climate pollutants and other long and short-lived GHGs than the topic deserves. The discussion of climate intervention is valuable, though it would be more useful with a careful discussion of the limited knowledge base concerning climate intervention, especially solar radiation management.

Another challenge is that natural scientists tend to use carbon “C” but economists and policy experts tend to use “CO_2”. From a natural science perspective, “C” is the more natural quantity to discuss, for several reasons. But the solutions, from discussions of carbon pricing to allowable budgets, are almost universally discussed in units of “CO_2”. The Committee thinks that this chapter (and the whole report) would be clearer and more useful with all of the quantities presented in units of “CO_2” where it is appropriate to do so.

Specific Review Comments Related to the Statement of Task

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

Most of the specific recommendations for this chapter are framed through discussion of the key findings. The key findings of Chapter 14 are all fundamentally consistent with the scientific literature, but they could be structured to more accurately capture the relative importance of several key concepts. In particular, none of the key findings emphasizes the point that stabilizing warming, independent of the target, requires that emissions of CO_2 and other long-lived GHGs fall eventually to zero. Further, none makes the point that the difference in the emissions trajectories that lead to stabilization at levels ranging from 1.5°C to 4°C turns out to be only several decades in the future for reaching zero CO_2 emissions.

> **Key Finding 1:** There will be a delay of decades or longer between significant actions that reduce CO_2 emissions and reductions in atmospheric CO_2 concentrations that contribute to surface warming. This delay—the result of the long lifetime of CO_2 in the atmosphere and the time lag in the response of atmospheric CO_2 concentrations following a reduction in emissions—means that near-term changes in climate will be largely determined by past and present greenhouse gas emissions, modified by natural variability. (*Very high confidence*)

Key Finding 1 presents the relationship between CO_2 and warming in a confusing way. A casual reading of the finding would be that decreases in CO_2 concentration resulting from natural partitioning into land and ocean sinks might lead to cooling and that there are important time lags between emissions and impacts on warming (or emissions reductions and impacts on cooling). Both parts of this are misleading. Many papers (see especially Matthews and Caldeira, 2008 and Solomon et al., 2009) show that warming from CO_2 is essentially permanent due in part to the long lifetime of CO_2 in the atmosphere and in part to the decreasing heat transfer to the oceans as they gradually warm. Matthews and Solomon (2013) make the important point that, if emissions stop, additional warming stops shortly thereafter. It is not really useful to discuss the lag between emissions reductions and concentration reductions because the CO_2 problem is essentially one of cumulative emissions, such that delaying action in the near term makes it more difficult to solve the problem in the longer term.

> **Key Finding 2:** Limiting the global-mean temperature increase to 3.6°F (2°C) above pre-industrial levels requires significant reductions in global CO_2 emissions relative to present-day emission rates. Given the near-linear relationship between cumulative CO_2 emissions and global temperature response, cumulative emissions would likely have to stay below 1,000 GtC for a 2°C objective, leaving about 400 GtC still to be emitted globally. Assuming future global emissions follow the RCP4.5 scenario, the total, cumulative emissions commensurate with the 2°C objective would likely be reached between 2051 and 2065, while under the RCP8.5 scenario, the timing would likely fall between 2043 and 2050. (*High confidence*)

This finding is, in some sense, based on a logical inconsistency. RCPs 4.5 and 8.5 are constructed around the idea that there is not a goal of limiting warming to 2°C, which makes them intrinsically incompatible and challenging to discuss in a single context. Also, the stated “cumulative emissions would likely have to stay below 1,000 gigatons carbon (GtC)” is given without a citation and is inconsistent with the 790Gt C cited in IPCC AR5 2013. According to IPCC, cumulative CO_2 emissions through 2016 are about 555 GtC, leaving a remaining allowance of 235 (not 400) GtC.

Additionally, it is important to include the probability of reaching the target and to be clear on the assumptions about other GHGs and aerosols, and on the implications of those assumptions.

> **Key Finding 3:** Successful implementation of the first round of National Determined Commitments under the Paris agreement is a large step towards the objective of limiting global warming to 3.6°F (2°C). Even greater greenhouse gas emission reductions are required beyond 2030 in order to increase the likelihood of achieving the 2°C goal; indeed, substantial (although smaller) reductions after 2030 would be required to achieve even the lesser goal of significantly reducing the likelihood of a global mean temperature increase greater than 7.2°F (4°C). (*High confidence*)

This finding would be clearer with an explicit acknowledgement of the link between climate stabilization and zero CO_2 emissions. Presenting the concepts in terms of emissions reductions after 2030 misses that key point. Key Finding 3 (and Figure 14.1) are both grounded in a specific conceptual model of what it means to comply with the Paris Agreement. In particular, the idea that “Continued ambition” should be read as emissions staying at 2030 levels is only one of many different possibilities. It is also possible (and more consistent with the way the Agreement has been framed by leaders in the UNFCCC) to interpret “Continued ambition” as sustaining rates of decarbonization, rather than emissions levels. With this framing, “Continued ambition” leads to decreasing global emissions, and “Increased ambition” leads to more rapid emissions decreases. Additionally, this finding misses a central element in the UNFCCC narrative about the Paris Agreement, notably its role in building a “culture” of emissions reductions. Almost all of the analysis makes strongly value-laden assumptions about the way that initial emissions reductions influence prospects for future emissions. Without weighing in on which assumptions might be correct, it is important to note their influence on the assessment of the challenges associated with reaching any goal.

> **Key Finding 4:** If projected atmospheric CO_2 concentrations are not sufficiently low to prevent warming of 2°C or more, climate-intervention strategies such as technological CO_2 removal or solar radiation management may gain attention as additional means to limit or reduce temperature increases. Assessing the technical feasibility, costs, risks, co-benefits and governance challenges of these additional measures, which are as-yet unproven at scale, would be of value to decision makers. (*Medium confidence*)

This key finding is currently written as a prediction about future policy emphases and the statement about "may gain attention" feels like a commentary on potential political dynamics. It would be clearer and more useful if presented as saying something about the state of knowledge about climate intervention. In particular, the statement could make it clear that at present, there is not sufficient knowledge to support a mature judgment about benefits and risks of possible use of intervention approaches, and some of these approaches could have unintended consequences and would not address all negative impacts of climate change (e.g. solar radiation management does not lessen ocean acidification).With this in mind, the key finding could be reshaped to state that geoengineering solutions require additional research and there are preliminary indications that geoengineering could limit some, but not all, aspects of climate change. Finally, a National Academies committee tasked with evaluating climate intervention techniques developed separate reports on CO_2 removal/sequestration and albedo modification (NRC 2015a and 2015b), noting that the large differences in research needs and social risks warranted independent treatment. A similar distinction between climate intervention approaches could also be considered here.

Are graphics clear, and do they appropriately reflect the major points in the text?

Figure 14.1 presents several possible trajectories for future emissions, but it does not present any with a greater than 50% chance of stabilizing warming at no more than 2°C. Given the chapter's emphasis on ambitious mitigation, there would be real value showing at least one trajectory with a greater than 50% chance of stabilizing below 2°C and one with a greater than 50% chance of stabilizing below 1.5°C. Relevant scenarios are shown in Figure 14.3.

Are likelihood / confidence statements appropriate, and justified?

The confidence statement on Key Finding 4 is difficult to interpret, based on the wording of the finding. As written, the draft report appears to assess confidence in the prediction that climate intervention will get increased attention and on the value for policy makers of increased attention. Presumably, the confidence should be associated with an assessment of the potential for climate intervention to contribute solutions or to the maturity of current knowledge.

Are statistical methods applied appropriately?

Yes, statistical methods applied are appropriate.

Is the chapter balanced? Are there areas that should be expanded, or removed?

There is no simple way to provide a comprehensive overview of the prospects for and challenges of mitigation in a few brief pages. Still, this chapter could set the stage more effectively with a clearer focus on the full range of possible future trajectories and on the critical issue of the probability of meeting any climate goal.

The treatment of aerosols and GHGs other than CO_2 could be stronger. The treatment of climate intervention would be clearer with an increased emphasis on the fact that climate intervention strategies are much less well known than climate change and that a reasonable foundation for decisions will require a big expansion of technology development as well as knowledge, especially in the area of governance and political dimensions.

III.15 CHAPTER 15: POTENTIAL SURPRISES: COMPOUND EXTREMES AND TIPPING ELEMENTS

Summary

The Committee found this chapter to be a welcome addition to the discussion of climate science and recommends it be expanded. It is the first time in a synthesis document of climate science that this topic has been addressed in a stand-alone chapter. The importance of recognizing compound extremes and tipping points (or thresholds) is fundamentally based in the inherent properties of complex systems and in the science of extremes in risk characterization. The chapter covers the limits of risk quantification and two broad categories of low probability-high impact events (compound extremes and tipping points). The Committee has some suggestions for improvement of the chapter.

A more thorough introduction for this topic is warranted. One suggestion is to better frame the chapter in the context of climate change as a complex system of interacting components. Prediction is difficult based on knowledge of the components of the system alone, the history of the system matters, emergent features appear that are not necessarily observed in the individual pieces, and feedbacks make simple cause and effect rare.

- The chapter could be strengthened if revised to move in the direction of more emphasis on lower probability but high consequence outcomes emphasizing compound extremes and tipping points, e.g. methane hydrates influenced by ocean warming and pressure.
- Because surprises are unknown unknowns, it is suggested that “Potential Surprises” be removed from the title, or changed to “Potential for Larger Changes”.
- There is no mention of negative feedbacks that could potentially offset positive feedbacks. The Committee recommends including this for balance.
- It would be valuable to mention a few examples of some past surprises (e.g., ozone hole, rate of Arctic sea ice loss) and discuss when scientists have been surprised and the factors that contributed to that surprise.
- The chapter could be strengthened by illustrating how gradual climate change can lead to tipping points in built as well as natural ecosystems (see NRC, 2013).
- The chapter could include a more thorough discussion of characterizing risk (see NASEM, 2016b)
- Chapter 15 would benefit from the inclusion of known unknowns in the science, such as changing natural variability in a warming world, ocean-ice dynamics including potential impact of ice-sheet melt on ocean circulation, changing ocean ecosystems, and their interaction with the physical ocean environment, stratosphere-troposphere exchange.

Specific Review Comments Related to the Statement of Task

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

The Committee thinks that the chapter could be updated with some more recent references, e.g. Clark et al., 2016, Liu et al., 2017, Drijfhout et al., 2012, and Koenig et al., 2014.

There is also the soil C "bomb" hypothesis, whereby metabolic/microbial activity adds heat to thawing soils resulting in a runaway carbon release. (e.g., Hollesen, et al. 2015).

Are the key findings presented clearly, and documented in a consistent, transparent and credible way?

Key findings are generally presented clearly and appropriately.

> **Key Finding 1:** Positive feedbacks (self-reinforcing cycles) within the climate system have the potential to accelerate human-induced climate change and even shift the Earth's climate system, in part or in whole, into new states that are very different from those experienced in the recent past (for example, ones with greatly diminished ice sheets or different large-scale patterns of atmosphere or ocean circulation). Some feedbacks and potential state shifts can be modeled and quantified; others can be modeled or identified but not quantified; and some are probably still unknown. *(Very high confidence)*

Without including negative feedbacks, the confidence of Key Finding 1 may be overstated. The Committee recommends acknowledging this and considering whether the confidence level is appropriate.

> **Key Finding 3:** While climate models incorporate important climate processes that can [be] well quantified, they do not include all of the processes that can contribute to positive feedbacks, correlation of extremes, and abrupt and/or irreversible changes. For this reason, future changes outside the range projected by climate models cannot be ruled out (*very high confidence*), and climate models are more likely to underestimate than to overestimate the amount of future change (*medium confidence*).

Key Finding 3 also includes only positive feedbacks and certainly the models do not incorporate all processes that contribute to both positive and negative feedbacks. There is no obvious summary of "what is and is not included in the latest generation of CMIP5 models" in Chapter 9, as the description of evidence base suggests. Moreover, it might be helpful to mention the failure of climate models to simulate importantly different past climates like the Paleocene-Eocene Thermal Maximum.

Are graphics clear, and do they appropriately reflect the major points in the text?

Table 15.1 lists some potential tipping elements. Some of the terminology in the table is vague and could be made more explicit. For example, there is frequent use of the term "collapse" to describe a state shift (AMOC, ice-sheet retreat, sea ice retreat) when it would be more valuable to define the state shifts more explicitly. North Atlantic Convection could refer to the ocean, atmosphere, or both and should be clarified. Also, consider adding "freshwater forcing on ocean circulation" as a main impact pathway for Greenland and Antarctic ice-sheet retreat. Finally, ecosystem services are listed as a main impact pathway: the subject might better be left for NCA4, but if retained, 'ecosystem services' is a rather broad term that could be made more explicit.

Figure 15.1 (left panel) should include potential climate tipping points for the entire globe, not just the Americas. In particular, the instability of marine-based ice in deep East Antarctic basins represents a large and scary unknown (Pollard et al., 2015, DeConto and Pollard, 2016, Aitken et al., 2016, Mengel and Levermann, 2014, etc.). The bubble in the figure should be re-labeled to read "Instability of marine-based Antarctic ice", rather than implying that just West Antarctica is vulnerable. Figure 15.1 (right panel) seems to be rather obvious (high-impact wildfire and drought events occur under hot, dry conditions) and could be deleted. This figure is also not referenced in the text and should be.

Are likelihood / confidence statements appropriate, and justified?

See comments for key findings.

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Appendix A
Line Comments

Line comments are provided for the Executive Summary and all chapters contained in the draft CSSR. For each comment, committee members indicated how important they thought addressing the comment was by providing one of three letter designations, ranked in order of highest to lowest priority: V indicates strongly (or vigorously) recommend, R is recommend, and S is suggest.

EXECUTIVE SUMMARY

#	page/line	V/R/S	
1	General	R	Climate models or Earth System models? In the early days of USGCRP the models were primarily atmosphere models with radiative forcing and some feedback loops. Today's models have become more fully coupled system models and hence the term "Earth System models" is more appropriate.
2	P11/L10-18	S	Narrowly defined, climate may be the statistics of weather, but this discussion could be improved by considering the context of the climate system—notably the role of the oceans, which make the climate change problem so different from weather prediction.
3	P11/L18	R	This statement implies monotonic change which is certainly not true of all weather patterns. This should be rewritten to be scientifically accurate.
4	P11/L19	S	Augment the statement about 150 years with one about more recent changes, e.g. since the big increase in slope of radiative forcing (Figure 2.6) around 1960.
5	P11/L20	R	This sentence is unclear. The text implies that the non-uniformity caused the changes, when it should state that the warming caused the changes, with modulation by the non-uniformity.
6	P11/L29-33	R	Chapters 2 and 10 should better reflect how ecosystem responses are feeding back to climate (especially for ocean CO_2 uptake).
7	P11/L29	R	A statement that the number of extremes in recent years exceeds that expected by chance is needed here.
8	P13-31	S	Throughout this chapter, and probably the entire document, temperature refers to surface temperature, yet it is rarely stated this way. Somewhere, it would be helpful to state explicitly that 'temperature' refers to surface temperature and also to remind readers that temperature does not just change at the surface.
9	P13/L32-P14/L7	R	A key point here: for most of the United States, the observed warming is consistent with anthropogenic forcing (Figure 6.5).

#	page/line	V/R/S	
10	P13/L6	R	The sentence "Fifteen of the last 16 years…" is unclear. Could rephrase as "All 15 of the 15 warmest years in the instrumental record have occurred in the last 16 years."
11	P13/L6	S	Entire box: it is possible that the next couple of years will be cooler than 2015 and 2016 due to the large El Niño event of 2015-2016. It is worth pointing this out somewhere, though not necessarily here. Page 13, line 28 might be a good place to put a statement about the 2015-2016 El Niño event.
12	P13/L12	V	Why state "more than 1.6° F"? It may be more appropriate to put confidence intervals on the change.
13	P13/L16-27	S	Bullets beginning on lines 16 and 21 could be combined.
14	P13/L17	R	Human activities are described as "primarily responsible"—does this mean that > 50% of the change is being ascribed to human activity? Does it mean something else? Should be specific.
15	P13/L28-31	S	Variability might also be changing, but is hard to measure and quantify, and also complicates the detection and attribution (see previous paragraph).
16	P13/L26	R	Need to provide some quantification for "small". Possible wording, "…over that period is not more than a small fraction of the total trend." It would be even clearer if authors could provide a real quantification, along the lines of "over that period is not more than a small 25% of the total global trend."
17	P13/L30	S	The comment about "limited" influence of El Niño needs some level of quantification. Even something like "its influence is limited to a small fraction of global and regional climate trends…" would be beneficial.
18	P13/L32-P14/L2	S	It might be appropriate to compare the speed of the warming to previous paleo-temperature changes.
19	P14/L2	R	Figure 6.2 (on which this statement is based) is not convincing, certainly not with high confidence.
20	P14/L4	R	The phrase "early 1900s" is too general and inaccurate shorthand for the 1901-60 average. See also main text remark about using slope-based statements.
21	P14/L5	R	For western United States temperatures, it would be wise to add a terse qualifier from the discussion in the chapter, e.g., that changes in circulation might be partly responsible for the enhanced warming in the West and suppressed warming in the Southeast, lest an unsuspecting reader surmise that one area is more susceptible to GHG increases and the other less so. See also the main text comment about treatment of observed trends using a slope-based approach.
22	P14/Fig. ES.1	S	Perhaps it would be worthwhile to point the reader back to the Front Matter for an explanation of reference time periods or other approach to describing observed trends. See review comments on Chapter 2

#	page/line	V/R/S	
			about computing trends and about using hatching.
23	P14/L11	V	This is the 4th temperature baseline in 3 pages. Reduce variations if at all possible.
24	P14/L16-22	R	The key point, largely missing from the ES, is that warming from CO_2 is essentially permanent. That is the point that should be made here. The modest warming from inertia is not irrelevant, but it is not a big deal.
25	P14/L16-19	R	The statement here about committed warming seems to be at odds with some papers, including Mathews and Weaver, 2010, (and others cited in main text for Section III.14), which show that there would be no additional warming if human GHG emissions were to immediately cease. There's an important difference between freezing concentrations and eliminating anthropogenic GHG emissions. Recommend revising to better reflect breadth of literature on this topic. • Matthews, H. D., and A. J. Weaver. 2010. Committed climate warming. *Nature Geoscience* 3(3):142-143. DOI: 10.1038/ngeo813.
26	P14/L16-22	R	Consider adding a statement regarding remaining uncertainty in estimates of climate sensitivity. This seems too important not be stressed in the ES. Need to define or describe what sensitivity is, and be careful in caption of Figure ES.2. As written, warming commitment and climate sensitivity, which are most relevant at the low and high end of future emissions, are a little bit tangled up in this bullet point.
27	P15/L1-6	S	Several edits will give Figure ES.2 more impact and better grounding in the chapters. Near-term and "few decades" (30-50 years?) need to be clearly specified. Since the "lower scenario" in many figures is RCP4.5 instead of (apparently, here) RCP2.6, the qualitative descriptions of the scenarios should be clarified by labeling them also with RCPs. Some reference to Chapter 14 and the steps needed to achieve RCP2.6 would be appropriate. The two panels would benefit from tick marks on the right hand side or horizontal lines accompanying the tick marks. It might be helpful to explain why there is a broader range for the temperature response in the higher scenario. Finally, note that with RCP4.5 and RCP2.6, temperature is nearly stabilized by 2100 while with RCP8.5 is still rapidly warming: the world beyond 2100 also matters.
28	P15/L9	R	This sentence should note the emissions are global. An uninformed reader might misinterpret this to mean U.S. emissions only.
29	P16/Fig. ES.3	V	Figure ES.3 (and especially the source figure, Figure 6.7) should explicitly state which RCPs are used. The caption for Figure ES.3 says "See Figure 6.7...for more details" but there are no more details in Figure 6.7. It is identical, except for the addition of "Figure source: NOAA/NCEI". Also "near-present" is ambiguous, as elsewhere

#	page/line	V/R/S	
			"near-present" and "present-day" are both used to represent 1986-2015. Use the date range throughout for clarity.
30	P17-19	R	The evidence is a lot stronger for increases in temperature-related extremes than for precipitation, where the changes are barely more than could be attributable to chance (see comments on Chapter 7 in particular). Language should be added that indicates this distinction.
31	P17-18	S	There is no home in the ES for seasonal precipitation changes, either observed or modeled. Page 13 begins a section "Global and U.S. temperatures will continue to rise" and page 17 pivots to extremes. Although the results may seem uninteresting, this might be considered a gap. Even just a short statement about the ambiguities of precipitation projections would suffice.
32	P17/L3	S	The terminology "extreme weather", "extreme climate", and "extreme event" or some combination appears many times in the report, but no definition is provided.
33	P17/L7-9	S	For balance, this sentence should also note that cold extremes are becoming less frequent, and perhaps also that flooding is (contrary to popular view) changing in complicated ways with no clear national trend. As written, this feeds the inaccurate blanket statement "all kinds of extremes are getting worse/increasing" even though lines 17ff clarify.
34	P17/L6-9	S	Could note that extremes also present challenges for businesses and national security.
35	P17/L10-12		There is, at best, scant evidence that tornadoes are exhibiting changes linked to climate change. What does seem to be missing in this list are heat waves, storm surges, intense precipitation events.
36	P17/L17-20	S	Is there a geographic pattern for the observed changes in cold/heat waves, as there is for heavy precipitation (next paragraph)?
37	P17/L26-P18/L4	R	The findings that (1) the frequency and severity of ARs, which account for 30-40% of western snowpack (a California-centric view that doesn't apply to the rest of the West), is projected to increase and (2) reductions are projected in western United States winter and spring snowpack are not consistent. Some explanation is needed, e.g., that the first refers to increased precipitation and the second refers to a reduction in the proportion of snowfall to total precipitation due to warming. As defined, ARs almost always leave behind less snow in Oregon and Washington because they raise the freezing level usually to the 5000-6000 foot level and the combination of high temperatures, high dew points, heavy rain, and strong winds melt a lot of snow (in other words, they do not end a drought, they set one up). Of more interest would be whether rain-on-snow events will increase, or heavy precipitation, regardless of whether they qualify as atmospheric rivers (and researchers do not agree on a definition of ARs)

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38	P17/L33	R	The term "benchmark" is unclear. The passage seems to be saying that the Dust Bowl era is the period of worst drought and highest temperatures in the historical record for the U.S., which may only be true for certain regions and certain ways of measuring drought and extreme temperatures. For many regions of the U.S., recent temperatures are warmer and/or drought is more severe than in the 1930s. Recommend rewording to better reflect the state of knowledge.
39	P18/L1-4	V	This bullet should be revised. There is strong evidence that western United States snowpacks have already been decreasing, so the sentence should say "continued reductions". The phrase "assuming no change in current water-resources management" strays into impacts, policy, and adaptation. It is sufficient to say that temperature changes will overwhelm any increases in precipitation in many places, leading to reductions in snowpack (and summer soil moisture) and to changes in unregulated streamflow in rivers where snowmelt is a significant component, or something similar. The West already experiences summer low flows as part of its natural hydroclimate. And it would read better to add an article: "end of the century."
40	P18/L5-9	S	This Bullet could mention expansion of area where tropical cyclones can occur (Figure 9.2).
41	P18/L11-16	S	The wording in this figure caption is awkward.
42	P19/Fig.ES.5	R	Would it be possible to include Alaska in this figure? Also, the final version of the CSSR should use a higher quality image, because the legend in the top panels is barely legible. The definitions used in the bottom panels also need to be explained somewhere in the report.
43	P19/L6	R	The 1901-1960 average is not the average for the first half of the 20th century.
44	P19/L16	S	The word "chaotic" has a specific mathematical meaning to atmospheric dynamicists, and a rather different meaning for the public. Suggest clarifying which is meant—if the former, a bit of explanation would be needed.
45	P19/L22-P20/L3	R	This statement slightly oversteps the evidence presented on the topic of NPO in Chapter 5, page 191, lines 11-13, where NPO is briefly mentioned and "effects on U.S. hydroclimate...have been reported." Absent strong evidence (e.g., reasonably impressive correlation coefficients), the use of the word "important" is a stretch. Similar concerns apply to NAM.
46	P20/L5-7	R	This last sentence is either a weak allusion to attribution studies of which the Committee is unaware, or a speculation. If the former, evidence should be presented (perhaps in Chapter 5) and a stronger statement written. If it is speculation, it should be removed.
47	P20/L10	S	Connected, yes; "strongly" is debatable.
48	P20/L20-25	S	This might be an appropriate place to note also the effects of a

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			poleward expansion of the Hadley circulation on tropical cyclone ranges (Chapter 9, page 309, lines 29-35). Some clarification is also needed because, as shown in Chapter 1, these purported shifts in subtropical dry zones have not been clearly observed over land.
49	P21/Fig. ES.6	R	Figure ES.6 does not illustrate "natural variability now being influenced by human activities", and the report does not present any evidence to support the claim. Chapter 5 says merely that "only low confidence is indicated for specific projected changes in ENSO variability" (Page 191, lines 32-33, emphasis added). Recommend deleting the figure, or if retained, revising the caption to accurately reflect the state of science. Also see ES comment on P19/L22.
50	P21/L8-11	S	The ocean is an integral part of a coupled system not just a planetary component that has feedback. In other words, the "climate" is not just the atmosphere. "Ocean" should be singular, not plural.
51	P21/14-15	S	Consider listing GMSL rise in both inches and SI units (cm?) to be consistent with use of both ºF and ºC, and usage of both units in Chapter 12.
52	P22/L4-7	S	The findings on SLR could start with a conclusion about risks of long-term commitment to several feet/meters and then address 21st century.
53	P22/L14	R	"In most projections…" Really? Are there any projections in which GMSL does not continue to rise after 2100? None are shown or discussed in Chapter 12. Recommend changing to "all" or explaining
54	P22/L19-26	R	It would be helpful if all of the conclusions on differences between local and global sea level rise were quantified (e.g. "0.2 m more or less than the global average"). It would also be very useful to indicate that the regional differences look a lot less important if SLR is at the high end of the range, especially after 2100.
55	P22/L33-35	S	Reference to impacts strays from draft CSSR intended focus; if retained, this statement could be revised to note that some of these impacts are already observed.
56	P23/L4-13	S	These two paragraphs should follow the same structure and need not both mention effects of changes in oxygen. Suggest starting the first paragraph with observations of change, then mention potential consequences (not well understood), and then removing potential consequences from second paragraph.
57	P24/L4	S	There is no need to use "include" if the list that follows is complete. "Examples are shown above for..."
58	P24/L9-14	S	This paragraph is stylistically inconsistent with the rest of the ES.
59	P24/L18-21	S	The potential consequences of thawing permafrost for the carbon cycle are more nuanced than presented here. Recommend adding "but the magnitude of carbon release is currently uncertain" to the end of this statement. See comments in Section III.11 of main text.

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60	P25/L3-4	S	"Human activities…" This is an awkward and unnecessary statement here. It almost hints that activities other than (or in addition to) emissions are to blame. Could reword this to more directly link the ice loss to human-induced warming, rather than ambiguous "activities".
61	P25/L5-6	R	It does not add information to conclude with high confidence that earlier models were wrong. The statement would be much more powerful if framed in terms of a rate of ice loss (with confidence) and a parenthetical statement that the new estimates are substantially higher than older ones.
62	P25/L7	V	The basis for this very important statement is a single sentence in Chapter 11 (page 373, lines 32-34) and it does not appear in the Chapter 11 Key Findings. For the prominence in the ES, it deserves more prominence in Chapter 11.
63	P25/L9-12	R	See comments in Section III.11 of main text regarding confidence levels. It would also be useful to add some indication of which weather patterns show some evidence of Arctic influence, if any; otherwise the last clause is too vague to include.
64	P26/L7-8	R	The section heading regarding a 2ºC temperature limit requires some explanation of why a 2ºC limit is important is needed. Consider using the Box on page 27 as the heading for this section instead
65	P26/L7-P28/L4	R	There is almost no mention in this section of non-CO_2 GHGs and some discussion of them is warranted.
66	P27/L10-16	R	This bullet is confusing. Consider revising the first portion to something like: "Significant actions taken today to reduce CO_2 emissions would take a decade or longer to influence atmospheric CO_2 concentrations. This delayed response—the result of the long lifetime…" The key conclusion is that warming from CO_2 is essentially permanent, unless the CO_2 is removed by carbon capture and storage. The draft report will be clearer with a stronger focus on the climate issue as one related to cumulative emissions of CO_2. The statement "reductions in atmospheric CO_2 concentrations" is not absolute reductions, but reductions relative to a high-emissions scenario. This should be phrased clearly so that readers do not infer that aggressive climate policies would cause atmospheric CO_2 concentrations to drop any time soon.
67	P27/L15	S	The phrase "modified by natural variability" muddles the message. Perhaps the clause is meant as a concession to the point made in the cited Hawkins and Sutton papers (2009, 2011) that in the near-term, internal (natural) variability dominates over greenhouse warming?
68	P27/L17-24		If the figures cited here are meant to somehow include the effects of human non-CO_2 forcers (e.g. methane) then that should be clearly stated and the text should say how one establishes equivalence between cumulative emissions of CO2 and other forcers which have much shorter lifetimes.

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69	P27/L29	S	If stating “unproven at scale,” what would the authors consider to be the correct scale for proof?
70	P27/L26-27	R	Without adequate background and discussion here of what solar radiation management is, this sends a dangerous message. It is also important to say more about drawbacks (e.g. solar radiation management does not address other concerns like ocean acidification, and could lead to other problems). Recommend deleting the first clause. Also, the National Academies reports on climate intervention stress the differences between solar radiation management and CO_2 removal and to reflect this, discussion of the two topics should be in separate bullets (see also main text).
71	P27/L31-P28/L4	R	These two bullet points need revision. Both cover similar ground and should either be separated cleanly into one on past CO_2 analogs and one on sea level, or combined artfully. Remove the word ‘precise’ (page 28, line 3)—there is no precise paleo analog at any point in Earth’s history, and ascribing the differences to CO_2 (by mentioning only CO_2) neglects important other factors in driving the differences. As written, a reader could infer that if atmospheric CO_2 concentrations are not reduced, then Earth will eventually experience the conditions mentioned here (+3.6 C global mean temperature, and +66 feet GMSL). If that is not the case, then those figures may not be appropriate. If it is the case, then say so. If this is uncertain, than the text could say something like “if CO_2 concentrations are sustained at Pliocene levels long enough, the risk of reaching Pliocene sea levels is unknown.” May also be worth mentioning the Paleocene-Eocene Thermal Maximum.
72	P28	R	The green box at top of the page is misplaced.
73	P28/L5-P29/L14	V	This section could be better framed by invoking the concern about low probability, high impact events. Replace the fuzzy “cannot be ruled out” by something like “important enough to merit serious consideration”. It would also be worth mentioning explicitly the worrisome fact that the processes and/or feedbacks that contributed to vastly different states in the past seem to be missing from climate models, and therefore they are not suited to predict at what CO_2 levels those processes and/or feedbacks may kick in and push the planet to a different state. Finally, the issues could be linked to the concept of Paris Agreement temperature limits (previous section) and avoiding unknown but potentially catastrophic risks.
74	P29/L20-P30/L33	R	See Section II.1. Since this list appears in almost the same form in Chapter 1, it could be trimmed here to focus on the newest and/or most important developments.
75	P29/L26	S	Perhaps change “changing extreme-climate” to “changing regional texture of extreme-climate”.
76	P29/L31-	V	This statement should be stronger. It should start with a clear

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	P30/L2		statement that warming has continued and that there was not a pause or hiatus. If the hiatus is still mentioned in the revised version, replace 2000 with 1998 because the red herring of the hiatus only worked if the trend analysis started in 2000.
77	P30/L12	S	Might be good here to say "seasonal regrowth." Otherwise referring to regrowth of sea ice might be puzzling.
78	P30/1	S	This statement should be "as predicted by basic atmospheric and ocean physics..." since ocean heat uptake is a very important part of story.

1: OUR GLOBALLY CHANGING CLIMATE

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79	P32/L13-15	S	This sentence is vague and does not add useful information to the Key Finding.
80	P32/L16-20	V	Key Finding 3 is appropriate, but steals some thunder from Chapter 3 as it is currently written. See Chapter 3 comments for recommended suggestions to address this.
81	P33/L10-19	R	This paragraph could be a lot clearer with a bit more explanation. Now, it reads like a series of ungrounded assertions.
82	P33/L26	S	"quite unpredictably" should be replaced with a more appropriate word
83	P34/L9-13	R	For most readers, the contrast between increasing Antarctic sea ice and a shrinking Antarctic ice sheet will be unclear. This is important enough to explain clearly.
84	P34/L9-11	S	Text should indicate over what period the small increase in Antarctic sea ice occurred. Recent reports indicate that Antarctic sea ice declined unexpectedly in 2016. See http://nsidc.org/
85	P34/L26	S	The use of "compelling" in this sentence adds little value. Recommend deleting it.
86	P34/L28	V	Use of a different length of the averaging period for the start and the end of the interval is confusing. See Section II.2 of main text regarding better statistical approaches for reporting observed changes.
87	P35/L4	R	"the previous"—the previous very strong El Niño but not the previous El Niño as the text implies
88	P35/L5	R	Quantify how much lower the global temperature was during the last El Niño (1998) relative to 2015.
89	P35/L9	S	It is generally preferable to work on the basis of evidence rather than assumptions. The phrase "we must assume" could be replaced by "it

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			is possible" or something similarly circumspect.
90	P36/L24-26	R	The wording gives the impression that RCP2.6 is likely to be less than 1.5, which is not correct.
91	P37/L8-10	R	It's not clear what is meant here about 13-year and 18-year intervals. Are these running means?
92	P37/L9	R	It is important to note that satellite data and surface data are not measuring the same things. (This point can, with effort, be deduced from the next sentence or with less effort from Figure 1.5).
93	P37/L28-29	R	Emphasize that the hiatus was revealed as a slow-down in *surface* warming. As described on page 38, excess heat may have been transferred to the deep oceans. Benestad et al. 2016 also shows that other measures of climate change indicate continued warming of the planet during the hiatus. • Benestad, R. E. 2016. A mental picture of the greenhouse effect: A pedagogic explanation. *Theoretical and Applied Climatology*:1-10. DOI: 10.1007/s00704-016-1732-y.
94	P37/L34-35	R	A citation needed for this statement.
95	P38/L18-19	R	This statement might appear to contradict Key Finding 5
96	P38/L28-31	R	Discussion of the comparison between CMIP5 models and observations seems to let the models off the hook. Acknowledge that the capability of models to capture the internal variability of the oceans is probably flawed.
97	P38-39	S	The emphasis on PDO in this section could be lessened (see Chapter 5)
98	P38/L34	S	A word appears to be missing after "new"
99	P39/L2-3	S	Reader may think that looking only at 17-year intervals obscures the true signals of climate change. Why 17?
100	P39/L6-P40/L13	S	The use of "attributed" would benefit from referring to the explanation of detection and attribution in Chapter 3.
101	P39/L15-23	R	An explanation of why wet areas are getting wetter, and dry areas drier, would improve this paragraph.
102	P39/L15-23	R	The changes described in this paragraph should be better quantified, with uncertainty or statistical significance noted. See Section II.2 of main text.
103	P39/L21-23	R	Sentence on changes in Arctic precipitation needs to be clarified. As written, it is unclear whether increases or decreases have been detected, and with what magnitude.
104	P39/L29-30	S	The placement of the reference to Figure 1.7 implies that it also shows moisture levels instead of just precipitation. Recommend moving the reference to follow "century" page 39, line 29.

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105	P39/L29-30	R	Is the slight increase in precipitation statistically significant? Even if it is, is it appropriate to discuss global changes in precipitation when the responses to climate change are so regionally diverse?
106	P39/L32-34	R	Citations are needed for the ENSO statement; the references at the end of the sentence seem to refer to the operational updating by NCEI, not the ENSO attribution.
107	P40/L1-14	R	Quantify the changes described in this section. How much and over what interval?
108	P40/L25-29	R	This sentence gives the incorrect impression that there needs to be a change in the shape of the probability distribution for a small shift in the mean to lead to a large change in extremes.
109	P41/L33	R	As written, this sentence conveys a very limited amount of information. Are the low confidence trends up or down? Are they low confidence because there are trends in opposite directions across regions or because regional signals are weak?
110	P42/L9-10	S	Quantify the shift in storm tracks.
111	P42/L15-22	S	This sentence overstates the position of Barnes and Polvani (2015). They emphasize that Arctic amplification *may modulate* certain aspects of mid-latitude circulation response to climate change (emphasis is theirs, page 5526 in citation).
112	P42/L20-22	S	This sentence requires clarification. Is this mainly about the strengths of ETCs or about the locations? Is the key point that weakening of meridional gradients will lead to less intense ETCs overall?
113	P42/L24-26	R	This statement conflates the statistical problems of detection and of attribution—they are not the same. Recommend clarifying the language.
114	P43/L5-7	S	The sentence beginning, “However, the same study demonstrated…”is unclear.
115	P43/L9-10	R	Clarify that the carbon emissions from deforestation come mainly from biomass burning.
116	P43/L26-27	R	Quantify the change in snow cover extent and change in albedo.
117	P44/L8	R	It is important to make the point that, early in the anthropogenic era, deforestation was mainly temperate. The dominance of tropical deforestation is a post-1950 phenomenon.
118	P44/L18	R	Parmesan and Yohe 2003 is not the right reference for this statement.
119	P44/L19	R	The Reyes-Fox paper talks about CO_2 extending the growing season length in places where the length is water limited. As written, the text seems misleading in suggesting that a longer frost-free season and a possible growing season extension due to water conservation are additive, or even potentially additive.
120	P44/L20-26	R	Some of this material is revisited in Chapter 10; there should be a

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			tighter linkage.
121	P45/L24-25	R	Over what period of record? The previous two sentences suggest it could either be 1979-2014 or “since 1988”, or something else since the statement refers to IPCC 2013.
122	P46/L9-10	R	Stating that IPO controls tropical SSTs is not an accurate reflection of the current understanding of this topic.
123	P46/L19-P47/L20	R	Much of this material parallels Chapter 12 and the two chapters should be better linked.
124	P47/L14	S	Clarify that these are mountain glaciers.
125	P47/L21-38	S	Much of this parallels material in Chapter 11 and should be better coordinated.
126	P48/L1-29	S	Much of this parallels material in Chapter 12 and should be better coordinated
127	P50/L1-4	S	Statistical downscaling is hardly new; could add a sentence or two explaining how the LOCA method differs from earlier methods.
128	P52/L23-28	R	This paragraph places too much emphasis on the importance of improving climate models.
129	P55/L5-6	R	Consider providing a range or upper limit to projected changes in climate over the next 100 years.
130	P56/L1-4	R	Defining the role of ENSO and other natural cycles as “limited” is too imprecise. It would be much more useful to give a quantitative range or to say something like “no more than a small fraction of anthropogenic changes”
131	P56/L2-4	R	As written, a reader may wonder about natural variability in the past, for example, paleoclimates. Make clear that the “limited influence” of natural variability refers to this influence in the recent past and present-day.
132	P58/L3-5	R	Figure 1.1 The different curves should be identified as well as the time resolution of the data.
133	P62/L2-14	S	Figure 1.5 Describe what the different curves represent.
134	P65/L2-7	S	Figure 1.8: Mann et al. 2008 was unwilling to say much about the southern hemisphere temperature trends due to paucity of proxies in that hemisphere. Recommend revising the caption to reflect this uncertainty in the southern hemisphere, and therefore global temperatures over the past 1700 years. Or just change “global” to “northern hemispheric.”

2: PHYSICAL DRIVERS OF CLIMATE CHANGE

#	page/line	V/R/S	
135	General	R	Many acronyms are unnecessary. For example, SSI, TSI, RFari, RFaci, SWCRE, and LWCRE are used only in 1-2 paragraphs.
136	P86/L6	R	Text needs a citation for the 33°C calculation.
137	P86/23-25	R	Figure 2.1 includes more factors than those listed here in the text, some of which have larger fluxes than solar radiation reflected by the surface. Clarify the caption by stating that many of the fluxes pictured are feedbacks.
138	P87/L26-27	R	The text should make clear that the equilibrium surface temperature response for the equation given here is global.
139	P87/L23	R	Text should define the “top” of the atmosphere.
140	P87/L27-P100	V	Discussion of radiative forcing could begin with definition of instantaneous radiative forcing.
141	P88/L17-27	V	Text on aerosol forcings should be saved for later in the chapter, as the reader has not yet been introduced to the different aerosol effects.
142	P88/L3	R	Text states: “A change that results in a net increase in the downward flux at the tropopause constitutes a positive RF…” Depending on the definition of RF, the increase could be at the surface or top of atmosphere.
143	P88/L35	R	Text should emphasize evidence for the relatively small effects of cosmic rays on climate. See: • Krissansen-Totton, J., and R. Davies. 2013. Investigation of cosmic ray-cloud connections using MISR. Geophysical Research Letters 40(19):5240-5245. DOI: 10.1002/grl.50996.
144	P88/L38-P89/L1	R	Text should mention changes in snow and ocean-ice as examples of changing albedo.
145	P89/L11-12	R	This paragraph is overly complex even for a scientifically literate audience and should be simplified, even for a scientifically literate audience. For example, the reader may know little about stratospheric vs. tropospheric ozone, and cannot be expected to follow the line of reasoning here. Section could be shortened considerably.
146	P90/L6-7	R	Text should clarify that only the most explosive volcanic eruptions lead to aerosol reaching the stratosphere, where they can have global climate effects. Most volcanoes affect only regional climate due to short lifetime of aerosols in the troposphere.
147	P90/L2-4	V	“On millennial timescales, changes in solar output are expected to have influenced climate.” Text should be made more specific or deleted.
148	P91/L5-7	R	Text should explain that the long lifetimes of these gases account for their relatively homogeneous distributions.

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149	P91/L6-7	V	The text remarks that seasonal variations in CO_2 occur in response to changing "transpiration." While carbon uptake is to some degree controlled by stomatal opening, the main reason for the seasonal variation in CO_2 is photosynthesis.
150	P92/L12-13	V	"Over the last 50 years or more, CO_2 has shown the largest annual concentration and RF increases among all GHGs (Figures 2.4 and 2.5)." Methane has the largest relative increase in concentration. Recommend just stating CO_2 RF increase is largest.
151	P93/L9-11	V	Information on methane trends should be updated. Global methane has increased by 5.7 ppb per year over 2007-2013, with extreme increase in 2014. See Nisbet et al., 2016, and references therein. • Nisbet, E. G., E. J. Dlugokencky, M. R. Manning, D. Lowry, R. E. Fisher, J. L. France, S. E. Michel, J. B. Miller, J. W. C. White, B. Vaughn, P. Bousquet, J. A. Pyle, N. J. Warwick, M. Cain, R. Brownlow, G. Zazzeri, M. Lanoisellé, A. C. Manning, E. Gloor, D. E. J. Worthy, E. G. Brunke, C. Labuschagne, E. W. Wolff, and A. L. Ganesan. 2016. Rising atmospheric methane: 2007–2014 growth and isotopic shift. Global Biogeochemical Cycles 30(9):1356-1370. DOI: 10.1002/2016GB005406.
152	P93/L2-11	R	Paragraph about methane should provide information on relative magnitudes (with uncertainty ranges) of sources and sinks.
153	P94/L7	V	CO_2-eq needs to be defined.
154	P95/L28	V	Sentence on "improving" aerosol uncertainties needs clarification.
155	P96/L35-36	V	Sentence should state in *at least* the past 800,000 years…
156	P97/L20-22	V	Sentence confuses emissions with secondary aerosol formation, which is not considered an "emission."
157	P97/L3-8	R	Text should define synthetic GHG emissions.
158	P97/L20	V	Text should clarify that aerosols have short lifetimes and are relatively quickly rained out or deposited on timescales of days to weeks. It is the short lifetimes that leads to the inhomogeneous distributions. Both meteorological factors (such as temperature and clouds) and chemical transformations influence the production and lifetime of aerosols.
159	P97/L28	R	Sentence should be clear that responses are *climate* responses.
160	P98/L7-9	V	ERFs drive cloud and surface temperature changes, not the other way around. See Myhre et al. 2013, cited in main text.
161	P98/L4-11	V	List of feedbacks should include the ocean response e.g., changes to ocean circulation
162	P99/L13-15	R	The sentence would benefit from explanation of why the climate effect of clouds varies with altitude.
163	P100/L17-18	S	"However, there is evidence that the presence of a polar surface-

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			albedo feedback influences the tropical climate as well…" Mention the climate effect of soot deposition on glaciers at low latitudes e.g., see: • Wang, M., B. Xu, J. Cao, X. Tie, H. Wang, R. Zhang, Y. Qian, P. J. Rasch, S. Zhao, G. Wu, H. Zhao, D. R. Joswiak, J. Li, and Y. Xie. 2015. Carbonaceous aerosols recorded in a southeastern Tibetan glacier: analysis of temporal variations and model estimates of sources and radiative forcing. Atmos. Chem. Phys. 15(3):1191-1204. DOI: 10.5194/acp-15-1191-2015. • Yang, S., B. Xu, J. Cao, C. S. Zender, and M. Wang. 2015. Climate effect of black carbon aerosol in a Tibetan Plateau glacier. Atmospheric Environment 111:71-78. DOI: 10.1016/j.atmosenv.2015.03.016.
164	P100/L1-5	S	Suggest mentioning the interaction of warming oceans with sea ice and the subsequent acceleration of ice sheet loss.
165	P100/L16	V	Text neglects to mention that snow is present in mid-latitudes where it makes a big difference in absorbed solar in springtime.
166	P100/L2-5	V	Text should cite new paper on AMOC: • Liu, W., S.-P. Xie, Z. Liu, and J. Zhu. 2017. Overlooked possibility of a collapsed Atlantic Meridional Overturning Circulation in warming climate. Science Advances 3(1). DOI: 10.1126/sciadv.1601666.
167	P100/L29-32	R	There are more recent papers examining climate feedbacks of land cover change on ozone that could be cited. For example, Tai et al. 2013, and papers examining the effects of climate on wildfires: Yue et al., 2013; 2014; 2015. • Tai, A. P. K., L. J. Mickley, C. L. Heald, and S. Wu. 2013. Effect of CO2 inhibition on biogenic isoprene emission: Implications for air quality under 2000 to 2050 changes in climate, vegetation, and land use. Geophysical Research Letters 40(13):3479-3483. DOI: 10.1002/grl.50650. • Yue, X., L. J. Mickley, and J. A. Logan. 2014. Projection of wildfire activity in southern California in the mid-twenty-first century. Climate Dynamics 43(7):1973-1991. DOI: 10.1007/s00382-013-2022-3. • Yue, X., L. J. Mickley, J. A. Logan, R. C. Hudman, M. V. Martin, and R. M. Yantosca. 2015. Impact of 2050 climate change on North American wildfire: Consequences for ozone air quality. Atmospheric Chemistry and Physics 15(17):10033-10055. DOI: 10.5194/acp-15-10033-2015. • Yue, X., L. J. Mickley, J. A. Logan, and J. O. Kaplan. 2013. Ensemble projections of wildfire activity and carbonaceous aerosol concentrations over the western United States in the mid-21st century. Atmospheric Environment 77:767-780. DOI:

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			10.1016/j.atmosenv.2013.06.003.
168	P102/L31-36	V	Text should describe lifetimes and subsequent distribution of heat in the ocean.
169	P103/L8-21	V	Paragraph on trends in phytoplankton NPP is confusing. Why would climate change affect phytoplankton? Can the observed trends in phytoplankton be reconciled? If not, then the text should at least acknowledge that.
170	P103/L26-31	R	Text requires clarification as to why intensification of hydrological cycle leads to changes in salinity.
171	P104/L23	S	The flat trend in atmospheric methane shown in Figure. 2.5 suggests that thawing permafrost has not lead to increases in methane.
172	P104/L14-17	V	"...the strength of MOC will significantly decrease…" The word "will" should be "may."
173	P104/L20-23	V	"Permafrost and methane hydrates contain large stores of carbon in the form of organic materials, mostly at northern high latitudes..." Permafrost contains organic materials, and methane hydrates do not. Text should more clearly distinguish between these two potential sources of greenhouse gases.
174	P106/L24-25	R	Only large, very explosive volcanoes can lead to climate impacts of years to decades. See: • Raible, C. C., S. Bronnimann, R. Auchmann, P. Brohan, T. L. Frolicher, H. F. Graf, P. Jones, J. Luterbacher, S. Muthers, R. Neukom, A. Robock, S. Self, A. Sudrajat, C. Timmreck, and M. Wegmann. 2016. Tambora 1815 as a test case for high impact volcanic eruptions: Earth system effects. Wiley Interdisciplinary Reviews-Climate Change 7(4):569-589. DOI: 10.1002/wcc.407. • Robock, A. 2000. Volcanic eruptions and climate. Reviews of Geophysics 38(2):191-219. DOI: 10.1029/1998RG000054.
175	P107/L32-33	V	The text should acknowledge that aerosols are increasing over Asia and possibly Arabian peninsula. See: Hsu et al., 2012; Chin et al., 2014; Lynch et al. 2016. Given that the climate impacts from aerosol are regional, such regional increases could be very important. Reader is also curious why the trends in aerosol are inhomogeneous, and the text should mention that aerosol sources are being reduced in the developed world due to air quality concerns. • Hsu, N. C., R. Gautam, A. M. Sayer, C. Bettenhausen, C. Li, M. J. Jeong, S. C. Tsay, and B. N. Holben. 2012. Global and regional trends of aerosol optical depth over land and ocean using SeaWiFS measurements from 1997 to 2010. Atmos. Chem. Phys. 12(17):8037-8053. DOI: 10.5194/acp-12-8037-2012. • Chin, M., T. Diehl, Q. Tan, J. M. Prospero, R. A. Kahn, L. A. Remer, H. Yu, A. M. Sayer, H. Bian, I. V. Geogdzhayev, B. N. Holben, S. G. Howell, B. J. Huebert, N. C. Hsu, D. Kim, T. L.

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			Kucsera, R. C. Levy, M. I. Mishchenko, X. Pan, P. K. Quinn, G. L. Schuster, D. G. Streets, S. A. Strode, O. Torres, and X. P. Zhao. 2014. Multi-decadal aerosol variations from 1980 to 2009: a perspective from observations and a global model. Atmos. Chem. Phys. 14(7):3657-3690. DOI: 10.5194/acp-14-3657-2014. • Lynch, P., J. S. Reid, D. L. Westphal, J. L. Zhang, T. F. Hogan, E. J. Hyer, C. A. Curtis, D. A. Hegg, Y. X. Shi, J. R. Campbell, J. I. Rubin, W. R. Sessions, F. J. Turk, and A. L. Walker. 2016. An 11-year global gridded aerosol optical thickness reanalysis (v1.0) for atmospheric and climate sciences. Geoscientific Model Development 9(4):1489-1522. DOI: 10.5194/gmd-9-1489-2016.
176	P108/L4-6	V	Text should make clear that aerosols both scatter and absorb incoming sunlight.
177	P108/L16-17	V	"… only a few very specific types of aerosols (for example, from diesel engines) are sufficiently dark that they have a positive radiative forcing." This sentence should be deleted as it appears to minimize the impact of absorbing aerosols. Black carbon and brown carbon aerosols from many different sources absorb sunlight..
178	P109/L24-39	V	Much of this section repeats what should be in the "Description of evidence base" section. Text should stick with the terms aerosol-radiation interactions and aerosol-cloud interactions throughout. The terms "indirect" and "semi-direct" should be retired.
179	P110/L17-18	V	Regional effects of aerosols can be quite large, which is not surprising given that the regional forcing of aerosols can be equal to or greater than the magnitude of global forcing from GHGs. Recommend taking this under consideration in this description of evidence. See for example: • Philipona, R., K. Behrens, and C. Ruckstuhl. 2009. How declining aerosols and rising greenhouse gases forced rapid warming in Europe since the 1980s. Geophysical Research Letters 36:5. DOI: 10.1029/2008gl036350. • Ruckstuhl, C., R. Philipona, K. Behrens, M. C. Coen, B. Dürr, A. Heimo, C. Mätzler, S. Nyeki, A. Ohmura, L. Vuilleumier, M. Weller, C. Wehrli, and A. Zelenka. 2008. Aerosol and cloud effects on solar brightening and the recent rapid warming. Geophysical Research Letters 35(12). DOI: 10.1029/2008GL034228. • Wild, M. 2016. Decadal changes in radiative fluxes at land and ocean surfaces and their relevance for global warming. Wiley Interdisciplinary Reviews: Climate Change 7(1):91-107. DOI: 10.1002/wcc.372. • Leibensperger, E. M., L. J. Mickley, D. J. Jacob, W. T. Chen, J. H. Seinfeld, A. Nenes, P. J. Adams, D. G. Streets, N. Kumar, and D. Rind. 2012. Climatic effects of 1950-2050 changes in US anthropogenic aerosols-Part 2: Climate response. Atmospheric Chemistry and Physics 12(7):3349-3362. DOI: 10.5194/acp-12-3349-2012.

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180	P110/L28-32	R	Other major uncertainties to note include ocean uptake of CO_2 and the biological and physical response of the ocean to climate change. Another large unknown is the response the Brewer Dobson circulation and the subsequent impact for stratosphere-troposphere coupling.
181	P110/L3	R	Text should say that the largest *positive* feedback is water vapor.
182	P113	V	Figure 2.1: Caption should state what year, or range of years, is/are represented and whether these values are annual averages. Caption should also make clear that some of these fluxes represent feedbacks.

3: DETECTION AND ATTRIBUTION OF CLIMATE CHANGE

#	page/line	V/R/S	
183	P139-143	S	The efforts of the International Detection and Attribution Group (IDAG) should be mentioned (http://www.clivar.org/clivar-panels/etccdi/idag/international-detection-attribution-group-idag; http://www.image.ucar.edu/idag/).
184	P141/L18-21	R	The “relevant chapters” are mentioned but not referred to. Chapters where attribution statements are made should be specified.
185	P141/L34-P142/L4	R	“this topic cannot be comprehensively reviewed here”—while the highlights from NAS (2016) are helpful, they do not convey the full impact of that report. Please elaborate a bit, e.g., how much less confidence is there in attributing drought than heat waves?

4: CLIMATE MODELS, SCENARIOS, AND PROJECTIONS

#	page/line	V/R/S	
186	P152/L7	R	The phrase “depends primarily on future emissions” could be misleading. If the intent of the sentence is to say that uncertainty in future warming beyond mid-century is due to uncertainty in future emissions, it should be noted that the amount of warming will depend on emissions up until that point. Perhaps “depends primarily on prior emissions and ...”
187	P152/L9	S	It is worth mentioning that it is very unlikely that the atmospheric concentration of CO_2 will be below 400 ppm in this century.
188	P152/L30	S	Chapter 2 is referred to as Scientific Basis, but the title of Chapter 2 is Physical Drivers of Climate Change. The same discrepancy is on page 153, lines 6 and 12, page 159, line 36-37, and page 317, line 22.
189	P153/L20	S	“led by China and the United States” might be an overstatement, as certain European countries might validly claim to have taken

#	page/line	V/R/S	
			aggressive measures sooner
190	P154/L4	S	"earth system" should be "Earth system"
191	P154/L26-33	R	This paragraph should also mention that RCP8.5 is a scenario in which the concentration of atmospheric aerosols is anticipated to be greatly reduced, making the combined radiative effect of increased CO_2 and reduced aerosols even greater than expected for CO2 increase alone.
192	P155/L16	S	Sanderson et al. 2016 should be listed as Sanderson et al. 2016a., since there are two Sanderson et al. 2016 papers cited.
193	P156/L24-31	R	This paragraph is not clear and the purpose of the calculation is not described. The paragraph should be rewritten for clarity and motivation, and the purpose of the calculation should be described. Swain and Hayhoe, 2015 should also be referenced.
194	P156/L10	R	Section 4.2.3: Is this pattern scaling / GMT scenario used in the rest of the report? If not, it should be deleted from this chapter.
195	P156/L15-19	S	If Section 4.2.3 is retained, terms should be better defined or explained, including time-slice, scenario uncertainty, and climate sensitivity, to make more understandable for the intended audience.
196	P157/L22-P158/L4	R	The paragraphs illustrate a lack of organization found throughout this chapter. The first two lines restate, without reference, the point made (unclearly) on P156/L16-21, then abruptly introduce Key Finding 1, with no elaboration, and with the confusing clause about an unlikely scenario in which sequestration suddenly increases, all with no references. The next paragraph introduces the Paris Agreement, and links RCP scenarios to cumulative emissions to temperature targets, again with no references. The paragraph should be rewritten for clarity.
197	P157/L30	R	The statement, that only 150 Gt more carbon can be emitted globally in order to meet the 1.5C target in the Paris Agreement, should have a reference.
198	P158/L14-P159/L30	R	This brief foray into paleoclimate is more appropriate for the paleoclimate discussion in Chapter 6 and should be integrated there.
199	P159/32-P161/L7	R	This section should discuss why models differ in their calculation of climate sensitivity.
200	P160/L25	S	CMIP6 is unlikely to be much farther along by the time this report is issued. Suggest omitting reference to CMIP6.
201	P160/L32	S	Also refer to Sanderson et al. 2016b.
202	P162/L30	R	The phrase "bias correction will remove the physical interdependence between variables" is imprecise, because the latter is not necessarily a consequence of bias correction. Recommend rewording to "statistical downscaling can alter some of the physical interdependence between variables."

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203	P163/L15-P164/L18	R	A statement should be added here that the intent of weighting models is to increase confidence in a particular response but that in doing so, there is a danger of underestimating the range of uncertainty, and hence missing possible climatic outcomes.
204	P164/L37	S	The IPCC AR5 uses 1.5-4.5C as a range. Why the difference?
205	P165/L7-10	S	The two sentences: "For precipitation … entire century." states that precipitation is necessarily a sub-grid-scale process. But, precipitation is constrained by large-scale moisture convergence, so there are large-scale constraints. Recommend focusing the statement on reference to extreme precipitation, or individual precipitation events.
206	P165/L12-13	R	Insert a reference for the statement that natural variability is mostly related to uncertainty in ocean initial conditions.
207	P166/L37-P167/L5	S	Unclear what "the second" refers to. The second statement in the key finding would appear to be "projections...differ modestly" but the traceable account statement invokes "radiative properties..." which are not obviously related to available candidates for the second statement. The summary is also inaccurate, because the notion of "committed warming" was not introduced until about the IPCC Third Assessment Report.
208	P167/L4-5	R	This response sounds reactionary and as such, dismissive. Recommend rewording to focus on "basic physical principles of radiative transfer" or something more specific.
209	P168/L15-18	S	These statements should include references.
210	P169	V	The table of emissions rates for RCP8.5 and actual values include some values with 10 significant digits. At most, the values are known to 2 or 3 significant digits. The table should be reformatted to include no more than 3 significant digits for all values shown.
211	P174/Fig. 4.4	R	The history portrayed here is not entirely consistent with the IPCC equivalent (Figure 1.13 of Cubasch et al. 2013)—aerosols are included in SAR (1996), carbon cycle in TAR (2001). Also, the main story is not just increasing amounts of *physical science* as some of these could fall into other kinds of natural science (as line 29 of page 160 notes). The Committee has recommended deleting this figure, as noted in the main text. If the CSSR authors choose to retain it, consider the suggestions provided in this comment.

5: LARGE-SCALE CIRCULATION AND CLIMATE VARIABILITY

#	page/line	V/R/S	
212	P186/L33	R	"teleconnections" should be defined, or a different phrase should be used.

#	page/line	V/R/S	
213	P186/L35	S	Change "Principle" to "Principal"
214	P187/L5-19	V	Removal of Figure 5.1 and instead a brief explanation in the text is recommended, as stated in main text. The text could convey these key points: the general circulation transports heat poleward in complex, time-varying circulations. In the tropics, the overturning direct Hadley cell is made up of several more zonally confined circulations and large east-west overturning cells (e.g. the Walker circulation). The mid-latitudes are characterized by zonal jets that become dynamically or baroclinically unstable, and by extratropical cyclones and large planetary scale waves, the latter two responsible for the bulk of the poleward atmospheric heat and moisture transport. The polar latitudes are similarly asymmetric with the principal activity in the form of cyclones and anticyclones.
215	P187/L19	R	NWS 2016 is cited as the Figure 5.1 source, but there is no listing in the references section.
216	P187/L20-28	R	Add references for statements linking U.S. climate to NAO, PDO, ENSO etc.
217	P187/L31-34	R	Recommend referencing • Palmer, T. N., F. J. Doblas-Reyes, A. Weisheimer, and M. J. Rodwell. 2008. Toward seamless prediction: Calibration of climate change projections using seasonal forecasts. Bulletin of the American Meteorological Society 89(4):459-+. DOI: 10.1175/bams-89-4-459.
218	P187/L32-34	R	The second part of this sentence could be better articulated. Suggest something like: "The climatic response to external forcing may be altered by the forced response of these existing, recurring modes of variability. Further, the structure and strength of regional temperature and precipitation impacts of these recurring modes of variability may be modified due to a change in the background climate."
219	P188/L5	S	This sounds more like attribution than detection in the usual formulation
220	P188/L3-37	S	Can any of these changes be quantified, even in a relative sense?
221	P189/L17-30	R	The relatively small sample of ENSO events that have been observed in either the EP or CP categories should be mentioned. The differences between these "flavors of ENSO" are described in the peer-reviewed literature, but care is usually taken to note that the number of events in each category is < 10, so statistical significance is marginal.
222	P189/L31-38	R	The first part of this paragraph indicates that models don't agree on the projected changes in El Niño intensity or on changes in the zonal SST gradient, and then the paragraph ends by saying that studies suggest a near doubling in frequency of extreme ENSO events. It sounds contradictory. Also, the studies cited use a very strange metric

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			for extreme ENSO variability, which may not be appropriate for comparing 20th century and 21st century ENSO events. Removing the last sentence wouldn't change the overall intent of the paragraph.
223	P190/L1	V	"Robust evidence" is mentioned twice (also page 288, line 34). Reference to specific kinds of evidence should be provided... For example, "Model studies (cite) and observational analyses (cite) show a …"
224	P191/L11	R	The NPO is not the dominant pattern of variability, but usually is the second most dominant pattern of variability (this is true of Linkin and Nigam as well, the study cited here). Recommend rewording to "a recurring mode".
225	P191/L34-P193/L2	R	Readers without a strong background in atmospheric sciences/dynamic meteorology will have trouble following this subsection, and its contribution to the messages of the chapter is unclear. Recommend either rewriting or removing the subsection.
226	P193/L7	V	Delete "with a characteristic time scale of 40-60 years". See the cited Newman et al. 2016, Section 5, for a discussion of the lack of a characteristic time scale for the PDO. Christensen et al. (2013) says 20-30 years, Gedalof et al. (2002) says it behaved quite differently in the 19th century (as indeed is also the case in the past ~15 years). • Christensen, J. H., K. Krishna Kumar, E. Aldrian, S.-I. An, I. F. A. Cavalcanti, M. de Castro, W. Dong, P. Goswami, A. Hall, J. K. Kanyanga, A. Kitoh, J. Kossin, N.-C. Lau, J. Renwick, D. B. Stephenson, S.-P. Xie, and T. Zhou. 2013. Climate Phenomena and their Relevance for Future Regional Climate Change. In Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change. T. F. Stocker, D. Qin, G.-K. Plattner, M. Tignor, S. K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P. M. Midgley, eds. Cambridge, United Kingdom and New York, NY, USA: Cambridge University Press. • Gedalof, Z. e., N. J. Mantua, and D. L. Peterson. 2002. A multi-century perspective of variability in the Pacific Decadal Oscillation: new insights from tree rings and coral. Geophysical Research Letters 29(24):57-51-57-54. DOI: 10.1029/2002GL015824.
227	P193/L11-13	V	Suggest rewording the sentence to: "Consequently, PDO-related variations in temperature and precipitation in the United States are very similar to (and indeed may be caused by) variations associated with ENSO and the Aleutian Low strength (North Pacific Index, NPI), as shown in Figure 5.3. A PDO-related temperature variation in Alaska is also apparent ..."
228	P193/L21-24	V	Similar to previous comment, suggest rewording to: "United States temperature and precipitation variations related to the Pacific Decadal Oscillation (PDO) are very similar to (and indeed may be caused by) variations associated with ENSO and the Aleutian Low strength

#	page/line	V/R/S	
			(North Pacific Index, NPI).
229	P193/L35-P194/L10	R	Additional comments should be added about the AMO: 1) Some authors refer to AMO as AMV, i.e., Atlantic Multidecadal Variability to acknowledge the fact that the instrumental record is insufficient to detect an oscillation with 50-70 year period. 2) The oscillatory nature of AMO is further called into question by the possibility that it is arises in response to inter-decadal fluctuations in atmospheric aerosols, so there is nothing intrinsically oscillatory about it. 3) The fact that an AMO signal only emerges from SST time series after detrending should be mentioned, i.e., the "warm" and "cold" phases described in the text are w.r.t. a background upward trend.
230	P195/L6	S	This might be a natural point to include a short digression on Hawkins and Sutton 2009 (move from Section 4.4). It would make more sense complementing Figure 5.4.
231	P199/L12	S	Suggest: … lack of climate models' ability to properly simulate …

6: TEMPERATURE CHANGES IN THE UNITED STATES

#	page/line	V/R/S	
232	P218/L12-13	S	"between 1901 and 2015" is an ambiguous way of describing how the trends are calculated. Recommend using phrasing more similar to Table 6.1.
233	P218/L21	R	"Each NCA region" (et seq.)—according to the first figure in the report, there are 10 regions. There is no mention in this chapter of changes in the Caribbean.
234	P218/L24-25	S	"driven by a combination of natural variations and human influence" needs a reference.
235	P219/L9-10	R	This statement would be strengthened considerably with a time series plot to back it up. Such a figure could then be revisited in a subsequent figure with the GCM-simulated past and future temperatures.
236	P219/L34	R	Should be "Figure 6.2"
237	P219/L30-38	V	This conclusion requires a few logical steps: (a) the pollen-based reconstruction indicates temperatures about 0.2°C lower for the last data point compared to the warmest data point around AD 850; (b) during the period of overlap, the instrumental curve is exactly accurate with respect to the pollen-based graph; and (c) the last data point on the instrumental curve is higher than the high data point around AD 850. The problems with this set of logical steps include (1) it is not clear exactly how close the relationship between the instrumental and pollen curves really is and (2) the uncertainties for the instrumental curve are not computed. None of this is covered in

#	page/line	V/R/S	
			the traceable account, and the key finding is therefore unsupported by the figure and the text.
238	P220	R	There is no mention of changes in extremes for Hawai'i, Alaska, and the Caribbean in Tables and only mention of Alaska in the text (across subsections covering extremes). Extremes should be included, or omission should be explained.
239	P221/L13	R	Please clarify whether the 90th percentile is over the entire record or defined for 1901-1960 or 1986-2015.
240	P221/L17-20	R	A reference is needed for this statement.
241	P221/L20-21	S	Extremely, extremely slight.
242	P221/L22-24	V	The metrics, "brief period" and "intense cold waves" need to be explained more fully.
243	P221/L26	V	The definition of "extreme cold waves" is clearer, but still needs explanation. Is "extreme cold wave" the 10th percentile for the coldest 5-day stretch of each year?
244	P221/L34-P222/L9	R	Similar to previous comments, metrics in this paragraph should be better defined, including heat waves.
245	P221/L35	R	"somewhat less common" seems to be an understatement?
246	P222/L7-9	R	This statement may be true but it is not supported by Figure 6.4 or by any citations.
247	P222/L8-9	S	"as evidenced by"—a single event is not evidence, but could be an example. Suggest rewording: "… than those in the 1930s; one example is the multi-month heat waves …"
248	P222/L16-17	R	Presumably this is a different definition of 1901-2015 temperature trends from that used up to now in this chapter? Clarification is needed.
249	P222/L22-26	R	See Abatzoglou et al. 2007 that suggested that the lower warming in the southeast and higher warming in the west were both connected to atmospheric circulation. • Abatzoglou, J. T., and K. T. Redmond. 2007. Asymmetry between trends in spring and autumn temperature and circulation regimes over western North America. Geophysical Research Letters 34(18). DOI: 10.1029/2007GL030891.
250	P224/L6-P225/L6	S	Provide a date range for "near term".
251	P225/L27-37	R	It is difficult to interpret these results without further understanding of what the "cold spells, extreme cold waves, etc." metrics mean. As noted previously, recommend providing definitions.
252	P225/L37-P226/L3	R	While this statement seems obvious, a reference is needed. Also, it seems like it does not fit here. The entire chapter has been listing

#	page/line	V/R/S	
			statistics of how temperature is changing, and then it ends with two sentences describing the physical relationship between heat waves and land surface conditions. This might fit better earlier in the section.
253	P228/L20-22	V	This statement does not help trace anything, since some of the specific indices used here are not defined sufficiently to match them to indices in Zhang et al. 20141. Recommend deleting this sentence, and providing details of all calculations used to support this key finding.
254	P231-235	R	Tables that list the regions in this chapter should indicate they are NCA4 regions.
255	P237/Fig. 6.2	R	Y-axis and caption say that the anomalies are calculated with respect to 1904-1980 average. The average for the blue curve over that period looks to be about -0.25°C, not zero. Is this correct? The caption also says that the instrumental data shown are only for the period 2000-2006. Is this a typo?
256	P238-239	R	The methods for generating the time series in the lower panels of Figure 6.3 and all of Figure 6.4 should be described.
257	P240/Fig. 6.5	V	Gray boxes in Figure 6.5 presumably are where insufficient observations exist, and the CMIP5 data have been masked in the same places. This should be explained.
258	P240/Fig. 6.5	R	It is difficult to understand the green boxes with white hatching, notably the one near Oklahoma (?): the observed trend is 0.5-1ˆF/100 yrs and the modeled trend is 1.5-2°F per 100 yrs, and somehow that's not a detectable trend but is consistent with models? Clarify.
259	P243/Fig. 6.8	R	The patchy texture of Figure 6.8 likely arises from statistical oddities in the downscaling technique rather than from physical processes. Does the ESD add any information or would it be just as defensible to plot the CMIP5 output directly?

7: PRECIPITATION CHANGE IN THE UNITED STATES

#	page/line	V/R/S	
260	P253/L3-6	R	In Chapter 6, a similar reduction was attributed to the lengthening of the period averaged for "recent" times—note previous comments regarding averaged time periods. The southwest drought since 2011 does not pop out in Figure 7.1 as claimed here.
261	P253/L8-15	S	This discussion of interannual variability and individual regional droughts is slightly out of place in a paragraph that references a map (Figure 7.1). Recommend starting with a description of the spatial patterns (and conceding that the splotchiness may be an artifact of the gridding process). Are any of the trends statistically significant?

#	page/line	V/R/S	
262	P253/L34-36	S	These seasonal changes are not equivalent. Isn't it the case that the changes in fall are small and not significant, whereas the changes in spring are very large (especially June)?
263	P253/L33-P254/L6	S	Recommend providing additional, more authoritative citations for this information, perhaps IPCC.
264	P254/L6-17	V	"increase"..."decrease"..."trend"..."decreased"... for a lot of these comparisons, the period of record and possibly method are important in determining the sign of the result and should be specified.
265	P254/L22-P267/Fig. 7.2	V	Page 254 says 5-day but page 267 (Figure 7.2) says daily. Page 254 discusses individual stations (implying that they are visible in the figure) but page 267 shows regional averages. These discrepancies should be reconciled or explained more clearly.
266	P254/L33-34	R	Methods for calculating 5-year return value should be detailed here or in an appendix. Are any of the changes statistically significant?
267	P255/L9-10	V	How were station data combined into a spatial average? Was it CONUS? Greater detail is needed.
268	P255/L27-29	R	This passage seems to be a vehicle to discuss a single study. ETCs are surely more important in winter, and ETCs are surely a less important factor in summer in many NCA regions than other factors (e.g. tropical cyclones, southwest monsoon, other summertime convection). The link to the cited Pfahl et al. 2015 study is not clear-what season? Were they so idealized as to be irrelevant? What do GCMs say?
269	P257/L14-26	R	Since there's only one example of a U.S. storm, perhaps in addition to the lessons drawn from the two studies on the Colorado event, some more general lessons about detection and attribution of individual storms can be drawn from other parts of the world—the UK folks have done several studies of heavy precipitation events there.
270	P257/L15	S	"fewer extreme storms"—fewer than what? Fewer than observed?
271	P257/L21	R	Given this result, why show projections of snowpack change from a GCM (Figure 8.3)?
272	P259/L31-33	R	"large compared to natural variations"—as computed from observations or from the models' respective 20th century or NAT simulations? Larger, meaning what exactly (>1 sigma?) and why distinguish between "small compared to natural variations" and "inconclusive"—why not just reduce the load on the reader and use stippling only?
273	P260/L2	S	Recommend referring back to Chapter 5 where changes in Hadley circulation are discussed.
274	P260/L7-17	S	This is also covered in Chapter 8 and better coordination across chapters would improve this section.

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275	P260/L34	V	How were the standard deviations calculated? Across (how many) participating GCMs?
276	P261/L10-16	S	Recommend reorganizing this section to discuss the landfalling portion first, since that is more relevant to the U.S.
277	P261/L18-21	R	Should "CM3" be "CMIP3"? If not, what does it mean?
278	P262/L16	S	Recommend also noting that encroachment or removal of vegetation can contribute to uncertainties in observed precipitation trends.

8: DROUGHTS, FLOODS, AND HYDROLOGY

#	page/line	V/R/S	
279	P281/L7	R	Opening this key finding with "…is complicated" weakens the impact of what follows by suggesting that our understanding must be poor or limited. Recommend deleting this sentence here and where repeated in traceable accounts.
280	P281/L3-6	R	Important to note here that the "dust bowl" was not a purely natural phenomenon—it was exacerbated by human land management practices.
281	P281/L29	R	This section should note that the three characterizations of drought also have a varying range of timescales and are implicitly defined as deficits relative to some notion of what constitutes sufficient water (precipitation, soil moisture, stream flow).
282	P281/L29	R	"scarcity" has economic connotations. "deficit" may be more appropriate.
283	P281/L35-P282/L2	S	Stating "no region" seems perhaps oversimplified and inconsistent with Figure8.1, where it looks as if parts of northern Canada may see increased moisture during almost all seasons.
284	P281-285	S	Key Findings 1 and 2 have no figures associated with them. Inclusion of a time series for Key Finding 1 could nicely illustrate the message.
285	P282/L3-4	S	*both increase and decrease* should be *either increase or decrease.*
286	P282/L10	V	It is important to mention that precipitation deficits occur on a range of timescales, not just seasonal and annual. Some researchers maintain that "flash droughts" can result from just a few weeks of dry weather, and it is also clear from the paleo record that the long end of the timescale for droughts may be measured in decades, as indeed is mentioned later in this section.
287	P282/L11	S	It is unclear what is meant by "effect of these natural variations". Consider reframing this to ask how rising temperatures change the hydrologic balance, and how human-induced changes in atmospheric circulation might change the magnitude or frequency of precipitation deficits.

#	page/line	V/R/S	
288	P283/L5-6	R	Reference is needed.
289	P283/L10-12	S	"Attribution statements…are without associated detection." This is a very indirect way of noting that Swain et al. (2014) found positive attribution of the ridiculously resilient ridge to human-caused climate change. This should be stated directly (while also noting the lack of associated detection).
290	P283/L22-23	R	"The Great Plains/Midwest drought of 2012 was the most severe summer meteorological drought in the observational record for that region (see cited Hoerling et al. 2014 paper)." Is this consistent with earlier statements about the '30s Dust Bowl being the worst drought ever? If so, please explain how.
291	P283/30-34	R	Clarify whether this is intended to indicate that human influences intensified the drought by increasing temperatures and reducing soil moisture.
292	P284/L1-15	S	This paragraph seems to imply positive attribution of "the blob" and associated precipitation deficit to human influences. Is that correct? If so, it should be stated more clearly.
293	P284/L5-8	R	Bond et al. actually say the opposite—the ridge *caused* the blob. Also see Mote et al. (2016), who suggested that the blob influenced the likelihood of drought in 2015, mainly in the Northwest. • Mote, P. W., D. E. Rupp, S. Li, D. J. Sharp, F. Otto, P. F. Uhe, M. Xiao, D. P. Lettenmaier, H. Cullen, and M. R. Allen. 2016. Perspectives on the causes of exceptionally low 2015 snowpack in the western United States. Geophysical Research Letters 43(20):10,980-910,988. DOI: 10.1002/2016GL069965.
294	P284	R	In discussion of "the blob" and "ridiculously resilient ridge," recommend mention that it has been hypothesized that persistent phenomena like these are associated with arctic amplification, and link to Chapter 11, where this is already stated.
295	P284/L33-34	V	In the statement, "less sensitivity to temperature increases than to precipitation variations, which have increased over the 20th century", the juxtaposition of a directional temperature change and an increase in magnitude of precipitation variations is confusing. The quantity of soil moisture should be sensitive to total moisture input, not to the interannual variability.
296	P284/L38-P285/L2	S	This sentence, although correct, is potentially confusing/misleading. The reader could conclude simply that there has been no human influence on meteorological drought in the United States, when the authors may be intending to convey is that such an influence may exist, but studies based on precipitation trends do not show one. If this is intended to be an "absence of evidence" statement, rather than an "evidence of absence" statement, it should be rephrased for clarity and moved to follow the next sentence, which states a positive finding.

#	page/line	V/R/S	
297	P284	S	The 2012 Central United States drought has been classified as a "heat wave flash drought" (see Mo and Lettenmaier, 2015). But the frequency of such droughts has been going down over about the last 100 years, and 2012 represented what appears to be an isolated uptick in a type of event that is becoming increasingly rare. This trend should be noted in this section. • Mo, K. C., and D. P. Lettenmaier. 2015. Heat wave flash droughts in decline. Geophysical Research Letters 42(8):2823-2829. DOI: 10.1002/2015GL064018.
298	P284	S	Recommend incorporating discussion of the challenges in interpreting of P-E from climate models over the western U.S. because of their inability to resolve topography properly at a coarse resolution.
299	P285/L22-26	R	If the PET formulation in the cited Walsh et al., 2014 is the standard Thornthwaite temperature index method, it will likely lead to an over-estimation of droughts, as the cited Sheffield et al. (2012) paper shows.
300	P285/L14-P286/L2	S	Recommend splitting this long paragraph into two for clarity, with one focused on precipitation deficits and one on soil moisture.
301	P285/L7-12	R	Are other basins that have received attention in the literature that could also be included here?
302	P286/L3-6	S	This statement is very similar to one made on the previous page.
303	P286/L11-15	R	If available, consider addition citations for more robust simulations with offline hydrologic or other land surface models.
304	P286/L6-9	R	The statement "a direct CMIP5 multimodel projection.... total depth of the soil" is incorrect. Soil moisture percentiles based on total column soil moisture (from multiple land surface models) are already used in NOAA's input to the U.S. Drought Monitor. Generally, the estimated soil moisture percentiles are more, rather than less, consistent than the models' surface soil moisture.
305	P287	V	As shown in Lins and Slack 1999 and 2005, runoff has been increasing across most of the United States. at percentiles up to about the median, therefore model projections that indicate decreases seem questionable. What do the models show over the historic period? Aside from the western U.S., where snowpack-related changes clearly are related to warming, conclusions regarding runoff should be given low confidence. • Lins, H. F., and J. R. Slack. 1999. Streamflow trends in the United States. Geophysical Research Letters 26(2):227-230. • Lins, H. F., and J. R. Slack. 2005. Seasonal and regional characteristics of U.S. streamflow trends in the United States from 1940-1999. Physical Geography 26:489-501.

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306	P287	R	It may be inaccurate to refer to runoff changes associated with shifts in runoff timing in the western United States (related to reduced snowpack) as increased drought. Instead, this is a permanent shift in runoff timing.
307	P287/L29-P289/L23	R	This section would be strengthened with inclusion of some discussion of changes in the risk of floods associated with ARs. The discussion of ARs in Chapter 9 suggests that such changes might be expected. Even if no studies have been done and there is little one can say, it would be good to mention the issue.
308	P288/L4-16	V	Recommend specifying the time period. Using “trends” without reporting the time period (and perhaps method, if not the default least-squares linear fit) is vague and comparisons across studies can lead to contradictions if the time periods do not match.
309	P288/L21-26	S	This paragraph shifts abruptly from observed to projected changes. Was this inserted to reinforce the point in the previous three sentences that precipitation and runoff extremes happen in different seasons? The study mentioned points toward increases in fall (as well as winter), which do not support the point very cleanly. Recommend revising this text.
310	P288/L31-P289/L2	V	The discussion of attribution of flooding should recognize—and the text should state—that changes in non-climatic factors like channel structure, basin land use, etc., can be significant factors complicating such attribution.
311	P289/L24-P290/L5	R	This section is very out of place in Chapter 8. Recommend moving to Chapter 10.
312	P290/L3-4	R	The information presented here seems to suggest medium confidence, based on the definition provided in the draft CSSR. Recommend citing Westerling et al. 2007, who argue that there is a strong anthropogenic signal, and reviewing the NASA fuel fire index (see e.g. doi:10.5194/nhess-15-1407-2015). Beyond the studies cited, authors might also consider mentioning the well-established indirect effects of human activities on wildfire activity in the western United States: warmer temperatures, earlier snowmelt and runoff, and in many areas and times of year, lower soil moisture. These effects would suggest that there are not “competing schools of thought” on this issue, but instead a question of the relative importance of anthropogenic climate change versus other factors.
313	P291/L20-21	R	This statement appears to be based on comparison between recent droughts and the dust bowl, but the latter probably has a human-induced component, therefore is not an example of “Earth’s hydrologic natural variation.” This should be revised.
314	P291	S	Recommend stating that while soil moisture is not well observed over long periods, land surface models do a pretty good job of reproducing it, and have allowed reconstructions for ~100 years.

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315	P292/L31	S	Uncertainty is "not low" is awkward. Recommend stating high or moderate as appropriate.
316	P293/L17-18	R	It is important to note the long-standing nature of our understanding of effects on climate change on western United States hydrology here. While these changes are described in the cited Barnett et al. 2008 paper, they were also described well before then and this should be noted. Also Barnett et al. and others attributed changes to human-induced climate change, which should be noted here.
317	P294/L11-17	R	The summary statement should address water scarcity, since the key finding does.

9: EXTREME STORMS

#	page/line	V/R/S	
318	P308/L18	S	"this increase" should be "this projected increase"
319	P308/L28-32	R	This statement pertains to projected changes in ARs. Can anything be said about observed historical trends in ARs?
320	P308/L34	S	Recommends opening this section with a brief reminder about why we care about this subject: severe storms cause disruption, financial losses, and loss of life.
321	P308/L34-P309/L8	R	Is it really the uncertainty in sign or trends that makes detection and attribution relatively difficult for severe storms? Is the relative rarity of these events, which reduces the statistics significance of observed trends, not a more important factor?
322	P308/L34-P309/L8	S	This introductory paragraph suggests that the scope of the chapter will be limited to analysis of past trends, when in fact future projections are also discussed. Some commentary about the difficulty of projecting changes in severe storms would therefore also seem appropriate to include here.
323	P309/L10-19	R	Is it worth noting here that Hartmann et al., 2013 found increases in tropical cyclone activity to be "*Virtually certain* in North Atlantic since 1970?"
324	P309/L32-35	R	The statement "particularly robust" does not seem well supported by the trend shown in Figure 9.1. In what sense is 0.2°/decade or about 1.5° latitude in total robust? Is the trend statistically significant? Does the evidence really support the statement that the observed rate of movement can "substantially change patterns of tropical cyclone hazard exposure"?
325	P310/L15-21	R	Were these formal attribution studies? Some of the citations listed here predate the application of detection and attribution methodology to questions other than global mean surface temperature. Possibly a word other than "attribution" (with accompanying reference to

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			Chapter 3) would be appropriate, perhaps “ascribed”?
326	P310/L26	S	Could eliminate “in the literature.”
327	P310/L33-P311/L14	S	In this passage, IPCC statements are referred to as “consensus”. This does not occur elsewhere in the report and “assessment” would be a more accurate and common term for IPCC statements.
328	P310/L33-P311/L13	R	Do these modeling studies reproduce any of the observed variations in response to the mechanisms described on page 310, lines 15-20?
329	P311/L11	S	Recommend stating “the increase [decrease] in tropical cyclone frequency” instead of referring to “the sign of the change in tropical cyclone frequency.”
330	P311/L19-20	S	What is the difference between consistency and consensus?
331	P312/L9-11	S	This sentence should be posed as a statement and not a question.
332	P313/L5	S	Is it helpful to describe the hurricane drought as “anomalous” (meaning deviating from what is standard, normal, or expected)? It seems that that is the premise here. The question is, what is the explanation for this anomaly? The evidence presented suggests a large random element, with a possible contribution from climate change of uncertain magnitude.
333	P313/L23	S	Are post-storm damage assessments also used to determine the occurrence of a tornado? If so, indicate it here.
334	P315/L15-22	S	This passage is confusing and would be improved with clarification of the “climate conditions” and their relationship to CAPE and supercell strength.
335	P315/L30	S	It is unclear which “part of the United States.” is being referred to.
336	P315/L33	S	Arctic should be arctic, since it is an adjective here.
337	P315/L37-P316/L2	R	The explanation of “anomalously strong Pacific trade winds,” even if correct, is not very informative without identifying the cause of the anomaly. Can anything more fundamental be said about alternative possible causes of the “weather patterns of recent years?”
338	P316/L3-7	R	There seems to be broad agreement here between observed and projected (increasing) trends, suggesting that we should have some confidence in those increases. Yet, the corresponding section in Key Finding 4 (page 308 lines 20-24) seems to convey much less certainty and confidence. This should be reconciled.
339	P316/L4	S	Recommend this poleward shift should also be mentioned in Chapter 5.
340	P316/L13-P317/L3	R	The story is quite different for the Northwest states than for California—AR events are so warm that they lead to net removal of snow and therefore do not “end” droughts there. Some of the literature cited has a California bias in that it does not acknowledge ways in which ARs, and their effects, differ in other parts of the

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			West. Some discussion of their role in rain-on-snow floods (like those in February 1996 in Oregon and December 2007 in NW Oregon-SW Washington) would be an appropriate balance to the overly California-centric flavor of this paragraph.
341	P317/L3-7	R	Framing the question about how total distribution of precipitation (means and extremes) will change by discussing ARs is a popular approach, but it is unclear how this framing adds to the question. At minimum, this discussion should be put into the context of extreme precipitation discussed in Chapter 7 (and to some extent Chapter 8). The IVT approach discussed briefly at the bottom of page 317 may be a more useful way to cover this topic.
342	P317/L30	S	Are the "studies that show qualitatively similar increases" noted here observational studies? Please clarify.
343	P321/L35-37	V	This 'summary' discusses methodologies, not conclusions, and should be revised to reflect the key messages of this finding.

10: CHANGES IN LAND COVER AND TERRESTRIAL BIOGEOCHEMISTRY

#	page/line	V/R/S	
344	P337/L9-11	R	This is a non-informative use of the confidence language: with a literal interpretation, this is just saying you are pretty sure the probability is not zero. It would be much more useful to be explicit that the land could become a net source, with a probability that is not known but might be on the order of something between 10% and 50%, especially with continued high emissions.
345	P337/L26-27	S	It is probably better not to try to provide a mechanistic explanation of the urban heat island effect in this brief statement.
346	P338/L2-3	V	This is a misleading opening, implying that all LUC effects are via albedo.
347	P338/L33	R	"Earth browning" and "global greening" need definitions or to be replaced with self-explanatory terms
348	P338/L34-36	S	Update to include Girardin et al. (2016), who found no overall growth stimulation for continental boreal forest. • Girardin, M. P., O. Bouriaud, E. H. Hogg, W. Kurz, N. E. Zimmermann, J. M. Metsaranta, R. De Jong, D. C. Frank, J. Esper, U. Büntgen, X. J. Guo, and J. Bhatti. 2016. No growth stimulation of Canada's boreal forest under half-century of combined warming and CO2 fertilization. Proceedings of the National Academy of Sciences of the United States of America 113(52):E8406-E8414. DOI: 10.1073/pnas.1610156113.
349	P339/L31-33	S	Possible citations to add here include: and Bond-Lamberty, B. and

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			Thomson, A. 2010. Temperature-associated increases in the global soil respiration record. • Bond-Lamberty, B., and A. Thomson. 2010. Temperature-associated increases in the global soil respiration record. Nature 464(7288):579-582. DOI: 10.1038/nature08930. • Crowther, T. W., K. E. O. Todd-Brown, C. W. Rowe, W. R. Wieder, J. C. Carey, M. B. MacHmuller, B. L. Snoek, S. Fang, G. Zhou, S. D. Allison, J. M. Blair, S. D. Bridgham, A. J. Burton, Y. Carrillo, P. B. Reich, J. S. Clark, A. T. Classen, F. A. Dijkstra, B. Elberling, B. A. Emmett, M. Estiarte, S. D. Frey, J. Guo, J. Harte, L. Jiang, B. R. Johnson, G. Kroël-Dulay, K. S. Larsen, H. Laudon, J. M. Lavallee, Y. Luo, M. Lupascu, L. N. Ma, S. Marhan, A. Michelsen, J. Mohan, S. Niu, E. Pendall, J. Peñuelas, L. Pfeifer-Meister, C. Poll, S. Reinsch, L. L. Reynolds, I. K. Schmidt, S. Sistla, N. W. Sokol, P. H. Templer, K. K. Treseder, J. M. Welker, and M. A. Bradford. 2016. Quantifying global soil carbon losses in response to warming. Nature 540(7631):104-108. DOI: 10.1038/nature20150.
350	P340/L20-27	S	Similarly compelling statistics have been calculated for California's drought, and could be included here.
351	P340/L27-30	S	This sentence requires clarification, it's confusing as written.
352	P341/L4-17	S	This paragraph needs a sense of scale. Are these generally small or large effects, especially relative to other impacts of climate change and human activity?
353	P341/L4-38	V	Since SOCCR-2 is a draft report that will not be finalized until after CSSR, it should not be cited. Instead, the primary literature underlying the statements should be referenced.
354	P341/L18-29	R	This paragraph would benefit from a little more detail on the relationship between N availability and plant growth. For instance, line 24 should state that N mineralization *transforms the N into a form that can then be taken up by plants*, which results in the shift in N from the soil to vegetation.
355	P341/L27-29	S	Sentence on CMIP5 models seems out of place; remove?
356	P341/L33-35	R	This sentence requires the mechanistic context to explain why CO_2 losses from soils would decrease with N deposition.
357	P342/L1-15	S	Paragraph is too long, relative to importance of VOCs for climate change and vis-à-vis main chapter points.
358	P342/L1-15		Nearly all the references regarding VOCs are outdated. The chemical mechanisms involved in the oxidation of VOCs in the atmosphere have been much revised in recent years, and current understanding of the effects of VOCs on regional climate has changed. See for example Tai et al. (2013), Achakulwisut et al. (2015), and Heald and Ridley (2016).

#	page/line	V/R/S	
			• Tai, A. P. K., L. J. Mickley, C. L. Heald, and S. Wu. 2013. Effect of CO2 inhibition on biogenic isoprene emission: Implications for air quality under 2000 to 2050 changes in climate, vegetation, and land use. Geophysical Research Letters 40(13):3479-3483. DOI: 10.1002/grl.50650. • Achakulwisut, P., L. J. Mickley, L. T. Murray, A. P. K. Tai, J. O. Kaplan, and B. Alexander. 2015. Uncertainties in isoprene photochemistry and emissions: implications for the oxidative capacity of past and present atmospheres and for climate forcing agents. Atmos. Chem. Phys. 15(14):7977-7998. DOI: 10.5194/acp-15-7977-2015. • Heald, C. L., and J. A. Geddes. 2016. The impact of historical land use change from 1850 to 2000 on secondary particulate matter and ozone. Atmos. Chem. Phys. 16(23):14997-15010. DOI: 10.5194/acp-16-14997-2016.
359	P342/L22-26	S	These sentences should ideally address issues beyond just fire.
360	P342/L28-P343/L17	S	This paragraph is not well defined and is a mixture of too many topics. Recommend breaking it apart.
361	P342/31-32	V	Chapter 5 did not present compelling evidence that such changes are underway or even expected.
362	P343/L18-30	R	This is a confusing paragraph that ranges from storms to fires, with many puzzling comments. It is poorly structured and out of place; remove and/or break up to put elsewhere, or significantly rewrite to improve logical flow and emphasize important points.
363	P343/L25-29	S	This sentence is unclear. Does this mean flows will be lower than the historic extreme lows?
364	P343/L31-38	V	This would be a natural place for the Wildfires Section 8.3.
365	P344/L2-5	V	Almost every indicator of human activity has increased since about 1950, making statements about the correlation between CO_2 uptake and emissions unhelpful without additional context. Also, emissions could mean either industrial or ecosystem.
366	P345/L1-3	V	The description of the trend seems over-precise. If this interpretation is not supported by a robust statistical analysis, it should not be presented, and it should certainly not be presented as clearly understood.
367	P345/L1	R	Stating the growing season changes are "more variable" using referenced figures is not an apt comparison, since Figure 10.3 is a map and Figure 10.4 is a time series.
368	P345/L16-17	S	Are not plant hardiness zones based on temperate and growing season length? So, does this sentence add anything?
369	P345/L28	S	The cited EPA (2016) report is a peer reviewed document, but it seems to not be the most appropriate reference to adequately support this statement.

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370	P346/L6	S	“exacerbated” has the wrong implication. Recommend “amplified”.
371	P346/L6-7	S	The Reyes-Fox et al. (2014) paper cited here makes it clear that their conclusion is intended for settings where the season end is set by drought and not by cold. Zhu et al.(2016) provide an example where this does not appear to be the case. • Zhu, K., N. R. Chiariello, T. Tobeck, T. Fukami, and C. B. Field. 2016. Nonlinear, interacting responses to climate limit grassland production under global change. Proceedings of the National Academy of Sciences of the United States of America 113(38):10589-10594. DOI: 10.1073/pnas.1606734113.
372	P346/L16-18	S	This sentence is difficult to understand.
373	P346/L27-32	S	This sudden shift to CMIP5 model projections is unexpected and out of place.
374	P347/L13-17	S	Groundwater depletion is one of the major themes in recent years. It deserves more than this cursory treatment.
375	P347/L25	R	The chapter starts with lots of statements about the role of climate change in increasing sinks and then states that the general effect is to decrease forest sinks. These two elements of the interpretation need to be reconciled.
376	P348/L31-32	S	Were any of those cities in the United States?
377	P349/L1-11	S	This discussion is not useful without some information on direction and magnitude of the effects.

11: ARCTIC CHANGES AND THEIR EFFECTS ON ALASKA AND THE REST OF THE UNITED STATES

#	page/line	V/R/S	
378	General	R	There could be more discussion on the relative importance of the main drivers of sea-ice change (air vs. ocean temperature, prevailing wind-driven export, etc.) in this chapter.
379	P370/L25-27	S	Sea level rise should also be mentioned.
380	P370/L25-33	S	Lines 31-33 seem to serve the same purpose as 25-27, but state things in less obscure terms. Consider consolidating.
381	P370/L23-P371/L15	S	A number of statements in the introduction are very obvious and are not necessary, given the “scientifically literate” target audience.
382	P370/L27	R	Statement on ‘high sensitivity’ should include source or evidence.
383	P370/L24-P371/2	S	This paragraph contains a lot of useful information but does not flow well. Recommend revising to make the sentence order more logical.
384	P371/L1	R	Much of Alaska is within the Arctic, so the statement that Alaska’s

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			climate is "connected to the Arctic" does not seem necessary.
385	P371/L3-13	V	As written, this section could leave a read with the impression that not enough is known about Arctic climate change to inform policy, which is not the case. Recommend adding some statement to the effect that despite these uncertainties, we certainly know enough to formulate effective policies.
386	P371/L11-15	R	The concepts of "stunted scientific progress" and "significant scientific progress" are at odds. This section of the introduction needs editing.
387	P371/L20	R	"Vertical profiles of temperature". Where? In the boundary layer? Free atmosphere, upper ocean? Wording needs to be more direct and explicit.
388	P371/L26-35	S	The post-1979 temperature changes are impressive! It would be nice to see a map of them.
389	P372/L4	S	Should "however" be "moreover?"
390	P372/L9	S	"will continue"
391	P372/L22	R	Where does the "New Arctic" era come from? A reference is needed.
392	P373/L5-7	R	It is unclear how the statistics cited support the statement about "The age distribution...". If the decrease in multi-year ice were greater than the decrease in first-year ice, that would support the statement, but instead, the decreases are the same, within uncertainties. Furthermore, looking at Figure 11.1, the decrease in extent of multi-year ice appears to be much greater than 13%. Even considering the different baseline years (1988 vs 1984), this seems to be an inconsistency.
393	P373/L17-19	R	What is the definition of melt season (also in the caption for Figure 11.2)? Also, from Figure 11.2, it looks like part of Alaska's west coast has seen an increase in melt season.
394	P373/L29-32	V	"*very likely*" a human contribution to sea ice loss? Is this implying that there is up to a 10% chance that there is no human contribution at all to loss of Arctic sea ice? This seems surprising and inconsistent with the subsequent statement that future sea ice loss is *virtually certai*. If future human forcing is so certain, how can past human forcing be less certain?
395	P373/L30-31	R	"Internal climate variability alone could not have caused recently observed record low Arctic sea ice extents (Zhang and Knutson 2013)." A probability associated with this statement should be provided, if possible.
396	P373/L39-P374/L2	R	The section on sea ice is long on observational trends but short on projections of the future. In particular there is no quantitative discussion of how future sea ice extents depend on emissions scenarios. Recommend adding material on future projections.

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397	P373/L3	R	"The thickness…" Presumably you mean the *mean* thickness?
398	P374/L17-18	R	"AW" is an abbreviation used only here and only twice. Recommend just writing out.
399	P374/L18	R	Is the "observed AW warming unprecedented in the last 1150 years" referring to rates of warming, total warming since 1970, or maximum temperatures? This is too vague as written.
400	P374/L28-31	R	Projections of SLR should be left to Chapter 12, which is very different from Church et al. 2013 cited here.
401	P375/L18-P376/L11	R	It would be appropriate to tie this passage to the equivalent in Chapter 10. But, it is not clear that it fits within the purview of this report, viz, physical aspects of climate change.
402	P375/L33	S	It is unclear what "Thresholds in temperature and precipitation shape Arctic fire regimes…" means. Please clarify.
403	P375/L14	S	Modeling studies (projections) and observations are being awkwardly blended in this statement.
404	P375/L18-P376/L5	S	This paragraph is OK but completely Alaska-focused. It could be improved by inclusion of at least a few sentences of context with with respect to other parts of the North American and global Arctic.
405	P375/L22	S	"Shortened snow cover and higher temperatures…" compared to what?
406	P375/L27-30	S	This sentence could be broken up and re-written for improved clarity.
407	P375/L37	R	The basis for the stated projections is not given. Is it based on fire-weather analysis calculated from GCM climate projections? Some basic information should be provided, rather than just citations.
408	P376/L6-7	S	This sentence is confusing. Recommend restructuring to something like, "Approximately 50% of the total global soil carbon is found in boreal forest and tundra ecosystems". Also, please clarify whether this value contains carbon contained in permafrost.
409	P376/L16-17	R	The math here is unclear here—50% decline between 1967 and 2012 (45 years) = 11% per decade not 19.8% per decade? The citation is a broken URL.
410	P376/L20-22	R	Please explain why May is chosen for comparison instead of another month.
411	P376/L32	R	Why "since 2000"? Every time series in Figure 11.3 goes back to the 1980s.
412	P377/L12	R	"Mass loss from ice sheets and glaciers influences sea level rise" is too much of an understatement. It is important to explain that the relative contribution of mass loss to SLR continues to increase, now exceeds thermal expansion, and has the potential to eventually alter the landscape.
413	P377/L36	R	The sentence: "Ice mass loss… has steadily declined" is confusing, as

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			it seems to indicate that the rate of mass loss is decreasing around the Gulf of Alaska. That would be surprising given that the Arctic is warming rapidly, where mass loss from the biggest single ice sheet (Greenland) is accelerating, and where the Pan Arctic rate of mass loss seems roughly constant since around 2000 (Figure 11.4). Is this intended to state that ice mass [not mass loss] has steadily declined? If not, an explanation of why mass loss is decelerating, i.e. why glaciers near Gulf of Alaska are behaving differently from those in the rest of the Arctic, is needed.
414	P378/L14	S	The meaning of this sentence is unclear. What is meant by “factor”?
415	P378/L32-33	S	It would be useful to mention the recent California drought as an example of “persistent circulation phenomena like blocking and planetary wave amplitude.”
416	P379/L12-24	R	The statement that “these simulations do not support” a dominant role for loss of sea ice is followed by the argument that the models do not adequately represent the processes relevant to this question. If that is the case, then the “these simulations do not support” statement seems misleading. Clarify what is meant and why the results of these models are worth reporting.
417	P379/L36-P380/L2	S	Since AMOC has been covered in other chapters, cross-reference should be included.
418	P380/L2	S	Refer to Chapter 15.
419	P380/L5-6	S	Might want to weaken this statement; Alaska’s “carbon rich” permafrost is in a narrow band on North Slope and doesn’t compare to e.g. Hudson Bay Lowlands (see Figure 1a in the cited Schuur et al., 2015 paper).
420	P380/L7-8	V	The statement that “warming Alaska permafrost …is a concern…for the global carbon cycle” is too tepid and obscurely worded. Warming is a concern for the global climate, and the possibility of significant and uncontrollable releases of carbon threaten to undermine global efforts to control climate change.
421	P380/L12-29		For balance, consider citing Oh et al., (2016) who suggest that much of the Arctic can act as a sink for methane, even when permafrost thaws. • Oh, Y., B. Stackhouse, M. C. Y. Lau, X. Xu, A. T. Trugman, J. Moch, T. C. Onstott, C. J. Jørgensen, L. D’Imperio, B. Elberling, C. A. Emmerton, V. L. St. Louis, and D. Medvigy. 2016. A scalable model for methane consumption in arctic mineral soils. Geophysical Research Letters 43(10):5143-5150. DOI: 10.1002/2016GL069049.
422	P380/L15	S	Consider citing Treat et al. synthesis papers. For example: • Treat, C. C., S. M. Natali, J. Ernakovich, C. M. Iversen, M. Lupascu, A. D. McGuire, R. J. Norby, T. Roy Chowdhury, A.

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			Richter, H. Šantrůčková, C. Schädel, E. A. G. Schuur, V. L. Sloan, M. R. Turetsky, and M. P. Waldrop. 2015. A pan-Arctic synthesis of CH_4 and CO_2 production from anoxic soil incubations. Global Change Biology 21(7):2787-2803. DOI: 10.1111/gcb.12875.
423	P380/L15-18	R	This significantly misstates the central finding of the cited Schädel et al. 2016 paper, which was that emissions from thawed permafrost soils are likely to be overwhelmingly dominated by CO_2, not CH_4.
424	P380/L17	R	"Schädel"
425	P380/L17	R	How much permafrost-sourced CH_4 production oxidizes to CO_2? The statement that CH_4 is 20 times stronger a greenhouse gas than CO_2 is misused here.
426	P380/L18-22	R	How does the estimate of this feedback square with the fact of little change in CH_4 (Figure 2.5) during a period of rapid Arctic warming?
427	P380/L19-20	R	Explain why are there negative signs in front of 14 and 19?
428	P380/L21	V	The global temperature rise quoted here is from the permafrost-carbon feedback alone. Clarify.
429	P380/L30-P381/L9	R	There is some overlap here with Chapter 15. Note also that much of the CH_4 released is likely to oxidize to longer-lived CO_2.
430	P382/L10-11	S	"Climate models have been predicting… for more than 40 years." To make the meaning completely clear, it would be better to say "For more than 40 years, climate models have been predicting…"
431	P383/L12	S	Summary sentence simply reiterates previous text and does not integrate the key finding, evidence base, and key uncertainties in a concise way. The key finding and summary sentences almost seem reversed.
432	P383/L16	S	Typo. Check grammar.
433	P384/L21	R	Why is the likelihood of impacts only 2/3 when finding states that "crumbling buildings, roads, and bridges are being observed."? It seems like it should be 100%, since impacts are already occurring.
434	P383/L28-32	R	Recommend citing Schädel et al. 2016, and mentioning dominance of CO_2.
435	P383/L32	S	Perhaps add in-situ gas flux measurements to the list? Schuur et al. 2009, among many others. Schuur, E.A.G., Vogel, J.G., Crummer, K.G., Lee, H., Sickman, J.O., Osterkamp, T.E. 2009. The effect of permafrost thaw on old carbon release and net carbon exchange from tundra. Nature, 459(7246): 556-559. doi:10.1038/nature08031
436	P384/L1	S	Recommend adding microbial activity (warming) to the list. For example, see: • Hollesen, J., H. Matthiesen, A. B. Møller, and B. Elberling. 2015. Permafrost thawing in organic Arctic soils accelerated by ground heat production. Nature Climate Change 5(6):574-578. DOI:

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			10.1038/nclimate2590.
437	P384/L30	R	Are estimates of permafrost soil carbon content generally based on just the upper 1m of the soil column (e.g., see Tarnocai, 2009)? Is there still high uncertainty and possibly much greater potential losses than current estimates? • Tarnocai, C., J. G. Canadell, E. A. G. Schuur, P. Kuhry, G. Mazhitova, and S. Zimov. 2009. Soil organic carbon pools in the northern circumpolar permafrost region. Global Biogeochemical Cycles 23(2):n/a-n/a. DOI: 10.1029/2008GB003327.
438	P385/L38	S	Recommend revising "…is affecting coastal erosion" to "is increasing coastal erosion."
439	P386/L4	S	Consider replacing thermohaline circulation with MOC or AMOC?
440	P386/L11	S	Mention uncertainty of impact on fresh water forcing on ocean circulation. See: • Liu, W., S.-P. Xie, Z. Liu, and J. Zhu. 2017. Overlooked possibility of a collapsed Atlantic Meridional Overturning Circulation in warming climate. Science Advances 3(1). DOI: 10.1126/sciadv.1601666.
441	P386/L17-35	R	Line 17 states *high confidence*, lines 31 and 35 seem to contradict this by stating *very high confidence*. Please reconcile
442	P386/L28	S	Recommend "fine spatial scale" rather and "fine regional scale".
443	P389/Fig. 11.1	R	What are the thin green bars in the lower right inset? More significantly, comparing two individual years carries significant risks of cherry-picking. Recommend showing the classic September time series since 1979. This figure and Figure 11.2 are perhaps not the best choices for illustrating the key findings and main points of this section.
444	P389	V	As noted in Section II.3 for Figure ES.9/Figure 11.1, using a single year to compare with 2016 could be perceived as "cherry picking" to maximize the difference. Perhaps better to use a 1980's average.
445	P390/Fig. 11.2	R	Color scheme is strange, with no apparent logical progression. The positive scale, e.g. starts with increasingly dark shades of blue and then abruptly changes to greens. There does not seem to be any green on the map so perhaps this could be revised by simply eliminating the greens from the color bar.
446	P391/Fig. 11.3	S	This figure clearly shows that the coldest coastal soils are warming fastest, but it seems that what really matters is the increased area of permafrost at (or close to) 0ºC. If retained, recommend putting Centigrade scale on the right vertical axis.
447	P392/Fig. 11.4	S	The right y-axis should be explained (presumably it is for GRACE).

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448	P392	S	Perhaps add an additional figure showing increased area of Greenland with net negative net mass?

12: SEA LEVEL RISE

#	page/line	V/R/S	
449	P411/L26	R	Key Finding 4: “elevation thresholds” is ambiguous and should be defined.
450	P412/L7	S	Assessment of “change” is ambiguous. Consider replacing with “Assessment of vulnerability to rising sea levels…”. Then, the last sentence could simply begin: “A risk-based perspective on sea-level rise points to the need for an emphasis on how changing… “
451	P412/L12	S	Consider rewording to read: “1) increased volume of seawater from thermal expansion of the ocean as it warms, and 2), increased mass of water in the ocean from melting ice in mountain glaciers and ice sheets…”
452	P412/L15	R	This is mildly esoteric and could be made more explicit by defining GRACE, and/or write “(altimeter and gravity measurements) and in situ water column measurements (Argo floats)…”
453	P412/L11-22	R	An important point to consider emphasizing is that in the last century, the largest contributor to SLR was thermal expansion, but now, “since 2005” loss of land ice has begun to take over.
454	P412/L27	S	When did this “weakening of the Gulf Stream” occur? Is this referring to the 2010 spike in sea level along the US. Northeast (NYC, Boston, etc.)? This could be articulated more clearly.
455	P412/L34	S	Perhaps this should read “...and the reduced gravitational attraction of the ocean toward the ice sheet”
456	P413/L1	S	“cores” may not be the best word choice here. Consider rewording to say “In areas once covered by the thickest parts of the great ice sheets of the Last Glacial Maximum…” and then, on line 3, replace “Slightly further away from the cores with “Along the flanks of the ice sheets, such as… “
457	P415/L5	R	As in the introduction, it may be simpler/clearer to keep everything focused on the 20th century (1900) rather than post 1880. The implication that the rate of SLR was ~1.2 to 1.5 mm/yr during most of the last century, but is now twice that (~3mm/yr) is a critical point. The rate of SLR is accelerating and this should be emphasized strongly.
458	P415/L33	R	“heat storage” implies a total quantity of energy (Joules), not an energy flux (Wm^{-2}). Is this intended here (as in the rest of this section) to refer to, “rate of heat uptake by the ocean”, rather than “heat storage”? Clarify.

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459	P416/L11-14	S	As worded, the sentence beginning, "On interannual scales, ENSO... " may appear to contradict itself to some readers. Consider rewording.
460	P416/L16	S	Consider avoiding the use of "stronger evidence". "mounting evidence", or "accumulating evidence" may be better choices.
461	P416/L19	S	Consider replacing "Input-output calculations" with "mass balance calculations".
462	P416/L27	S	Add Wouters et al. (2015) to the list of references for ice mass loss in the Bellingshausen Sea region. • Wouters, B., A. Martin-Español, V. Helm, T. Flament, J. M. Van Wessem, S. R. M. Ligtenberg, M. R. Van Den Broeke, and J. L. Bamber. 2015. Dynamic thinning of glaciers on the Southern Antarctic Peninsula. Science 348(6237):899-903. DOI: 10.1126/science.aaa5727.
463	P416/L23	S	Helm et al. (2014) adds support to mass gain in Dronning Maud Land. • Helm, V., A. Humbert, and H. Miller. 2014. Elevation and elevation change of Greenland and Antarctica derived from CryoSat-2. Cryosphere 8(4):1539-1559. DOI: 10.5194/tc-8-1539-2014.
464	P417/L2	S	"Accelerating mass loss over the record…". What record? Consider clarifying that this is referring to Tedesco et al., 2013.
465	P417/15-16		Suggest that the authors clarify and expand on this important statement, because at least one reference cited here suggests an estimate lower than 250 cm.
466	P420/L24	S	Another paper worth citing to support the concept of a long-term sea-level "commitment" would be Golledge et al. (2015). • Golledge, N. R., D. E. Kowalewski, T. R. Naish, R. H. Levy, C. J. Fogwill, and E. G. W. Gasson. 2015. The multi-millennial Antarctic commitment to future sea-level rise. Nature 526(7573):421-425. DOI: 10.1038/nature15706.
467	P420/L24	R/S	Importantly, some of the "emerging science" discussed here (e.g., DeConto and Pollard, 2016), shows that the loss of marine-based ice is permanent on the timescales being considered here, because of the slow thermal recovery of the ocean. In other words, if lost, marine-based ice will not regrow until the oceans cool enough to allow the regrowth of buttressing ice shelves… which will take centuries to millennia. Consider including some discussion on this point.
468	P421-422	R/S	Regional Projections. The list (#1-6) is accurate, clear, and concise. However, it mostly emphasizes the gravitational fingerprint of ice sheet and glacier loss. It might be worth considering the addition of a bullet, regarding the expected distribution of near-term, stearic-driven

#	page/line	V/R/S	
			sea level rise (which could especially impact U.S. interests in the western Pacific). See figure 12.2.c. The impact of ocean dynamic effects on the U.S. Northeast Coast might be worth a bullet too, as it could be in the ~1 to >10 cm range by 2100. For example see: • Yin, J. 2012. Century to multi-century sea level rise projections from CMIP5 models. Geophysical Research Letters 39(17). DOI: 10.1029/2012GL052947. • Yin, J., and P. B. Goddard. 2013. Oceanic control of sea level rise patterns along the East Coast of the United States. Geophysical Research Letters 40(20):5514-5520. DOI: 10.1002/2013GL057992.
469	P427/L17	V	Key Finding 2: The list of RCP's appears to be backwards.
470	P430/L9	V/R	This should read: "… regarding the stability of marine-based ice in Antarctica". Ice in both West and East Antarctic outlets and deep basins are vulnerable.
471	P430/L11-19	R	This would be a good place to reiterate the important point that most of North America will experience substantially more relative SLR from an equivalent loss of ice on Antarctica than from Greenland.
472	P430/L35	S	This statement could also be listed under Key Finding 2.
473	P432/L23	S	Check grammar. "to do so.."
474	P433/L19	S	Check grammar. "at specific locations"
475	P434/Fig. 12.1	S	Consider citing the original source of the GIA solution in panel e? This may be from Hay et al., 2015? There are two Kopp et al., 2015 references. Label 2015a and 2015b?

13: OCEAN CHANGES: WARMING, STRATIFICATION, CIRCULATION, ACIDIFICATION, AND DEOXYGENATION

#	page/line	V/R/S	
476	General	R	A few examples of words that should be defined are: autotrophic, saturation with respect to aragonite, bathyal
477	P452/L30	V	. . . global average <u>surface</u> ocean acidity . . .
478	P452/L30	V	A definition of "global ocean acidity" is needed.
479	P453/L18-21	R	Recommend using "increases" instead of "changes"? All of the effects outlined in this sentence are in a single direction, implying that only one direction of changes in stratification can be responsible. Otherwise an equally valid reading of the sentence is that decreases in stratification would also have the same effects.
480	P454/L14	V	The full citation to "Rykaczewski et al. 2015" is not included in the References section

#	page/line	V/R/S	
481	P454/L16-18	S	Converting these changes to degrees/decade would allow more direct comparisons over the different time periods, although trends over such a short interval as 1982-2006 have generally poor signal-to-noise ratio. “deeper waters” suggests that these are measurements over some depth, so either this should be specified, or if SST is meant then it should be stated.
482	P454/L22	R	Have glaciers also thinned, in addition to melting at “their fringes”?
483	P455/L1-14	S	These paragraphs stray from the topic of this section, viz., warming, stratification, and circulation changes.
484	P455/L12-14	V	This assertion needs a citation.
485	P456/L6	V	Delete “more”
486	P456/L6-13	R	This paragraph contains long, complex sentences and should be revised to improve flow.
487	P456/L8-13	R	Not clear what is meant by “rate of acidification”; what is typically observed is that the changes in partial pressure of CO_2 in the surface ocean tracks those of the atmosphere on a seasonally-average basis, but with a geographically varying “disequilibrium”.
488	P456/L36-P457/L11	R	This paragraph is confusing to read, and would benefit from careful editing/rewriting.
489	P456/L36	R	Not clear what is meant by “less buffered against pH change”.
490	P457/L7-8	R	What is the difference between “sensitivity to ocean acidification” and “lower buffering capacity” ? The use of the word “sensitivity” seems more appropriate to organisms or ecosystems than to seawater.
491	P457/L15	V	Specify whether the CO_2 increase referred to here is in the ocean or in the atmosphere.
492	P457/23-25	S	This final sentence doesn’t seem to fit here.
493	P457/L36	R	The word “tremendous” should probably be omitted, it is not clear at what point “pressure” would rise to “tremendous pressure”, and unless the cited reference addresses this, it seems overstated.
494	P457/L15	V	Clarify whether this is oceanic or atmospheric $p(CO_2)$.
495	P457/L20	R	Recommend using Gt instead of Pg for greater familiarity with the wider scientific and policy community.
496	P458/L20-21	R	This sentence is ambiguous—is the driver “CO_2 emissions” intended to refer to “climate-induced” (as above)? Increased discussion of “anthropogenic nutrient input” as a driver for ocean deoxygenation would be beneficial.
497	P458/L23	S	Anaerobic respiration is of course possible too. Clarify.
498	P459/L6-9	V	Has this been shown? If noting this, also need to state that plant WUE also increases with climate (CO_2) change (discussed on page 341).
499	P459/L6-7	R	Warming on land *increases* plant WUE (see Chapter 10)—but this

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			is far from the only hydrologic effect of warming. Changing seasonality and increased ET are bigger effects. Clarify (quantify?) how these processes play a role in increasing nutrient transport to the coastal ocean.
500	P459/L22-23	S	It seems likely that the rates of net loss of wetlands are too small to be a factor?
501	P459/L35	V	Should this be "nitrite" here, or "nitrate"?
502	P459/L38	R	Recommend providing the comparison of the rates of N_2O production through this mechanism and terrestrial anthropogenic production
503	P462/L16	V	The full citation to "Rahmstorf et al. 2015" is not included in the References section.
504	P463/L20	V	This web-site for CDIAC ocean data is in the process of being subsumed into NOAA, and may be unavailable soon. Recommend additional citations, if possible.
505	P463/L26	V	Do these citations really claim increases in upwelling? The cited Feely et al. (2008) is based on a single cruise and Harris et al. (2013) on a 5-y time series. Suggest inclusion of additional citations if available.
506	P463/L32	V	Should be "were" not "where"
507	P463/L30-32	V	Minor revisions: "remain"…"yr^{-1}"…"were".
508	P465/L8	R	It is not clear what "naturally corrosive materials" might be present as "riverine loads". Is this intended to describe the CO_2 composition of the rivers (and how they differ from the ocean)? Please clarify.
509	P466/L5	V	Might be clearer if the amount 6 Sverdrups were parenthetically equivalenced to 6 x $10^{\wedge 6}$ $m^{\wedge 3}$/s, rather than simply an equivalence for a single Sverdrup.
510	P466/L6	V	Is the change of 100% to 150% from present day? Clarify.

14: PERSPECTIVES ON CLIMATE CHANGE MITIGATION

#	page/line	V/R/S	
511	P481/L10-18	V	This finding misses the key point that, independent of the warming target, stabilizing warming requires that CO_2 emissions go to zero.
512	P482/L7-8	R	Statement would benefit from clarification that the Paris goal is not exactly the same as 2°C or 1.5°C, it is "Holding the increase in the global average temperature to well below 2°C above pre-industrial levels and to pursue efforts to limit the temperature increase to 1.5°C above pre-industrial levels".
513	P482/L28-37	V	Distinguishing between committed warming and committed emissions is important here. The different scenarios diverge slowly

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			first because the problem is intrinsically one of cumulative emissions and second because, in the near term, annual emissions on trajectories of ambitious mitigation and continued high emissions are similar and diverge only through time.
514	P483/L1-15	V	The story in this paragraph is somewhat oversimplified. The key points that should be addressed are that (1) some SLCPs are coupled to CO_2, (2) some SLCPs are coupled to economic development, (3) some SLCPs can be tackled independent of CO_2, and (4) because SLCPs are short-lived, they can intrinsically be tackled any time (long-term climate changes are largely indifferent to cumulative SLCP emissions)
515	P483/L17-22	R	The framing of this paragraph is more appropriate for a key finding than the framing of the current Key Finding 2.
516	P483/L23-34	V	It is important to state the underlying probability when discussing allowable emissions for a target. The current wording could imply 100% confidence in staying below the target, when the numbers seem to be based on the "likely" range.
517	P483/L23-34	V	It is misleading to start by providing a CO_2 budget with no mention of other GHGs. This section could be improved by first introducing a budget based on a reasonable (and explicit) projection of non-CO_2 GHGs and then potentially mentioning that the budget would be bigger if emissions of non-CO_2 GHGs were smaller.
518	P483/L24	R	Clarification on whether quantities are presented in units of C or CO_2 is needed. While it is clear that units of C are better aligned with the physical and biological processes, the emphasis in the policy world on emissions in terms of CO_2 equivalents provides a strong motivation for converting everything in units of CO_2.
519	P484/L3-8	R	Any conclusion about untapped reserves of oil, gas, and coal depends strongly on weak assumptions about future relative preferences for the three fossil fuels. From a climate or a health perspective, it would make a lot more sense to think about utilizing more of the gas and less of the coal. Suggest including a caveat about uncertainties in future consumption patterns.
520	P484/L32	V	It is a little misleading to say that the concept of balance between sources and sinks in the Paris Agreement implies that CO_2 emissions need to drop to zero. The definition of a range of warming targets (any warming target) implies that CO_2 emissions need to fall to zero. The concept of balance in the Paris Agreement is an acknowledgement of this.
521	P484/L32-34	R	It should be noted that the ocean plays an important role in the C cycle and acts as a C sink. Marine ecosystems and species in the open ocean and deep sea, play a significant role in absorbing, moving, and storing carbon but are currently not considered or suited to be part of UNFCCC accounting mechanisms. Ignoring the ocean in mitigation strategies can create additional problems and/or acceleration of

#	page/line	V/R/S	
			changes in internal dynamics of the coupled atmosphere-ocean system.
522	P485/L3-14	V	Discussion of allowable emissions budgets needs to be accompanied by a clear presentation of the associated probabilities of staying under the temperature targets.
523	P485/L15-24	V	It is important to make the point that none of the trajectories has a high probability of limiting warming to 2C or less.
524	P486/L1-7	S	Examples that demonstrate policy interactions that can enhance or degrade other efforts would be useful here.
525	P486/L9-15	V	Need probabilities and CO_2 units.
526	P486/L21-31	R	It would be very useful to add a comment about the magnitude of the projected removals in comparison to current emissions. Without such a comparison, it is hard to get a sense of the truly vast scale of the removals in the integrated assessment models.
527	P487/L3	V	One of the main conclusions form the IPCC AR5 (2013) is that adapting to a world with warming much greater than 2^{o}C is unlikely to be possible. It is important to avoid constructions that imply the opposite.
528	P487/L21-22	R	This sentence requires clarification. Particularly effective in comparison to what? If the idea is that smokestack capture looks more feasible than direct air capture, it would be good to say this.
529	P487/L32	S	“Leading” is too normative in this context.
530	P488/L8-17	S	It could be misleading to start discussing technical feasibility of solar radiation management before introducing the challenges of governance. If the point is that the technical issues are unlikely to be the main constraint, this should be stated more clearly.
531	P488/L32-P489/L3	R	Here as elsewhere in the report, this chapter would benefit from greater discussion of coupled system responses. An atmosphere and surface focus can have serious implications for atmosphere-ocean coupling, troposphere-stratosphere exchange and the changes that would incur in the earth system response.
532	P489/L4-15	R	It is worth mentioning that the quantity of available literature and analysis of all of the climate intervention options is a tiny fraction of that on climate change. Just as progress on climate change requires extensive science, so will balanced consideration of climate intervention.
533	P489/L16-25	R	Recommend using this paragraph as the introduction to climate intervention, not the concluding one.
534	P490/L9-13	R	This description of the available evidence misses the importance of cumulative emissions.
535	P492/L5-11	R	The comment about required emissions reductions even for stabilizing at less than 4^{o}C is important, but it is incorrect as stated.

#	page/line	V/R/S	
			The essence of a cumulative emissions budget is that CO_2 emissions need to go to zero. The conclusion that the required reductions are smaller for a higher target is only temporarily correct. In general, it is important to make sure that readers are aware of the distortions that arise from acting as if we care about this issue only through December 31, 2099.

15: POTENTIAL SURPRISES: COMPOUND EXTREMES AND TIPPING ELEMENTS

#	page/line	V/R/S	
536	P500/L15	R	Typo: "… than can BE well quantified
537	P500/L16-17	R	The terminology "correlation of extremes" then "changing correlations" (used later in chapter including page 501, lines 5-6), then "compounded extremes" (section 15.3) is confusing. "Compound extremes" makes much more sense given the examples that are showcased.
538	P500/L19	R	The notion that models tend to error on the "underestimate" side is also well documented/supported in the paleoclimate literature. This is mentioned near the end of the chapter but could be mentioned earlier.
539	P500/L28-33	R	Recommend revising the framing of these sentences in terms of earth system models. While earth system models are increasingly becoming more complete, they do not include or fully represent all known processes of a fully coupled planetary system. Noting that even if these models were complete, this is a complexity problem and all complex systems inherently have the element of surprise would benefit the message of this section.
540	P501/L14	V	Add the meridional overturning circulation to the list… perhaps replacing the ENSO example.
541	P501/19-23	R	These sentences are inconsistent. In one sentence the discussion is limited to the instruments observation record—why? In the next paragraph the reference is to observational record not just instrumental record.
542	P501/L26-31	R	Is this basically curve-fitting and extrapolation? An even greater weakness is that they also assume stationarity.
543	P502/L1-15	R	If there are land processes incorporated including vegetation dynamics, why aren't these models earth system models with bio-physical processes? Another feedback that the models do not include is the ocean-ice dynamics coupled system in the Arctic Greenland ice sheet. Recommend mentioning these limitations of the models.
544	P503/L17-18	R	Clarify whether this analysis looked at univariate or coincident occurrence?
545	P503/L24	R	The reference to Chapter 11 implies that the Fort McMurray fire was

#	page/line	V/R/S	
			covered there but it was not explicitly mentioned nor quantified.
546	P503/L33-38	R	The example of compounded droughts is a good opportunity to mention the issue of non-resilient human communities.
547	P503/L35-38	R	The reference to limited resolution and increase in frequency of events ignores the possibility of inadequate incorporation of processes in the models that would produce the compounded events.
548	P504/L37	R	A better/additional reference to the warming hole would be: • Drijfhout, S., G. J. van Oldenborgh, and A. Cimatoribus. 2012. Is a Decline of AMOC Causing the Warming Hole above the North Atlantic in Observed and Modeled Warming Patterns? Journal of Climate 25(24):8373-8379. DOI: 10.1175/jcli-d-12-00490.1.
549	P505/L3-5	R	This should also be stated in Chapter 12 on SLR and currently is not.
550	P505/L16-27	V	An example of something like a tipping point was the accelerated loss of Arctic sea ice about 10 years ago and the models did not predict it. Consider using here an example?
551	P505/L16-30	V	Loss of Arctic sea ice may also accelerate the loss of Greenland land ice. For example: • Koenig, S. J., R. M. DeConto, and D. Pollard. 2014. Impact of reduced Arctic sea ice on Greenland ice sheet variability in a warmer than present climate. Geophysical Research Letters 41(11):3933-3942. DOI: 10.1002/2014gl059770.
552	P505/L32-33	V	This is an inaccurate representation of the findings of the cited Schuur et al. (2016) paper. While the quantity of C stored in permafrost soils is estimated at 1300-1600 Gt C, the paper indicates that only 5-15% is vulnerable to being released this century (although there is uncertainty). Therefore, it is very unlikely all this C would be released, as is suggested by the language in this sentence.
553	P506/L3-5	R	Refer back to the passage in Chapters 11 and 13 on hydrates (11.3.3 and 13.3.2).
554	P506/L21-24	R	Would add that it also depends on ice-ocean dynamics.
555	P506/L21	V	This sentence is misleading. Greenland responds “relatively slowly”, but Antarctica is different, because so much ice rests on bedrock far below sea level.
556	P506/L24	V	Robinson et al., 2012 report that even with an imposed 8ºC of warming, it takes ~1500 years for Greenland to loose ~85% of its ice. Recommend using the word “millennia” and not “centuries” for Greenland.
557	P506/L28-29	V	It is extremely important to bound this statement with rough timescales (centuries? millennia?)
558	P506/L29	V	This should read “… involving ocean-ice sheet-bedrock interactions”.

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			Marine ice sheet instability works in places where the ice-sheet bed slopes downward toward the continent.
559	P507/L7-11	V	This section is terrifying, though understated. At some point between present conditions and dramatically more CO_2, the planet does something completely different—and our climate models are missing whatever processes lead to that different state. This means that we cannot estimate at what point in the future we might activate those unknown processes. In contrast, the language in the executive summary is soothing. Suggest being consistent in how this issue is discussed
560	P507/L25	R	Some estimates of Pliocene sea level are 10-30m higher than today, requiring a substantial contribution to sea level from Antarctica. This also implies substantial polar amplification in both hemispheres (not just the Arctic) and extreme ice sheet sensitivity to modest warming.
561	P507/L34	R	Note that the referenced Huber and Caballero, 2011 paper reported 16xCO_2 to reproduce polar warmth in line with climate proxies.
562	P511/L6	R	Typo: "… than can BE well quantified"
563	P511/L19-21	V	Why is there no discussion of the known unknowns in science? Isn't the lack of this knowledge also a threat to our understanding of tipping points? Models do not yet incorporate all processes and coupling and there are known earth system science gaps that require attention.

SUGGESTED GLOSSARY TERMS

Aerosol-cloud interactions	Aerosol-radiation interactions	Agricultural drought
Albedo	Anticyclonic circulations	Atlantic meridional overturning circulation
Atmospheric blocking	Atmospheric river	Baroclinicity
Bias correction	Carbon dioxide removal	Climate intervention
climate sensitivity	CMIPs (general description)	CO_2 equivalent
CO_2 fertilization	Cryosphere	Denitrification
Deoxygenation	Dynamical downscaling	Earth system models
Effective radiative forcing	Empirical statistical downscaling models	Eutrophication
Extratropical cyclone	Geoengineering	Global temperature potential
Global warming potential	Hydrological drought	Hypercapnia

Hypoxia	Ice wedge	Instantaneous radiative forcing
Intended nationally determined contributions (INDCs)	IPCC	Long wave cloud radiative effect
Meridional temperature	Meteorological drought	Mode water
Model ability/model skill	Model bias	Model ensemble
Model independence	Model uncertainty	Negative feedback
Nitrogen mineralization	Ocean acidification	Ocean stratification
Oxygen minimum zones	Parameterization	Parametric uncertainty
Paris Agreement	Pattern scaling	Perfect storms
Permafrost	Permafrost active layer	Petagram
Positive feedback	Proxies	Radiative forcing
Relative sea level	Representative concentration pathways	Rossby waves
Saffir-Simpson storms	Scenarios	Sea level pressure
Shared socioeconomic pathways	Shortwave cloud radiative effect	Snow water equivalent
Solar radiation management	Special Report on Emissions Scenarios	Structural uncertainty
Teleconnections	Thermohaline circulation	Thermokarst
Tipping elements	Tipping points	Transient climate response
Tropopause	Undersaturation (vs. saturation)	Urban heat island
Zonal mean		

Appendix B
Statement of Task

A new ad hoc Committee will conduct an independent review of the Special Report on Climate Change Science, which will be available in late 2016 to early 2017. The Committee membership will be comprised of expertise in key areas of relevance to the Special Report, with some members drawn from the Committee to Advise the U.S. Global Change Research Program. The Committee will conduct this review concurrent with the public review period for the Special Report and produce a report.

The review will provide an overall critique of the draft special report and address the following questions:

- Are the goals, objectives and intended audience of the product clearly described in the document? Does the report meet its stated goals?
- Does the report accurately reflect the scientific literature? Are there any critical content areas missing from the report?
- Are the findings documented 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?
- Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?
- Are the document's presentation, level of technicality, and organization effective?
- What other significant improvements, if any, might be made in the document?

Appendix C
Committee Biographies

DR. PHILIP W. MOTE (chair) is the founding director of the Oregon Climate Change Research Institute (OCCRI), a professor in the College of Earth, Ocean, and Atmospheric Sciences at Oregon State University, and director of Oregon Climate Services, the official state climate office for Oregon. Dr. Mote's current research interests include observed regional climate change, regional climate modeling with a superensemble generated by volunteers' personal computers, variability and change in western US snowpack, and adaptation to climate change. He is the co-leader of the NOAA-funded Climate Impacts Research Consortium (CIRC) for the Northwest, and also of the Northwest Climate Science Center for the US Department of the Interior. Other large OCCRI-involved projects include Regional Approaches to Climate Change for PNW Agriculture, Forest Mortality and Climate, and Willamette Water 2100. From 2005 to 2014 he was involved in the Intergovernmental Panel on Climate Change, which shared the 2007 Nobel Peace Prize. He was also, from 2010 to 2014, a coordinating lead author and advisory council member for the US National Climate Assessment. He earned a BA in Physics from Harvard University and a PhD in Atmospheric Sciences from the University of Washington.

DR. SUSAN K. AVERY is the former President and Director Emeritus of the Woods Hole Oceanographic Institution (WHOI) and is now retired. Dr. Avery is an atmospheric physicist with extensive experience as a leader within scientific institutions. Avery was the President and Director of WHOI from 2008 to 2015, the first atmospheric scientist and the first female scientist to take the position of director in the WHOI's history. Under Avery's leadership, WHOI increased the application of its knowledge to societal issues, providing high-quality data and analysis across a range of topics, from climate to biodiversity to resources to natural hazards mitigation. Dr. Avery came to WHOI from the University of Colorado at Boulder (UCB), where she most recently served as interim dean of the graduate school and vice chancellor for research. From 1994-2004, Avery served as director of the Cooperative Institute for Research in Environmental Sciences (CIRES), a 550-member collaborative institute between UCB and the National Oceanic and Atmospheric Administration (NOAA). Avery was the first woman and first engineer to lead CIRES. Dr. Avery was a member of the faculty of the University of Colorado at Boulder since 1982, most recently holding the academic rank of professor of electrical and computer engineering. Dr. Avery's research interests include studies of atmospheric circulation and precipitation, climate variability and water resources, and the development of new radar techniques and instruments for remote sensing. She also has a keen interest in scientific literacy and the role of science in public policy. She is the author or co-author of more than 80 peer-reviewed articles. In 2013, Dr. Avery was named to the United Nations' newly created Scientific Advisory Board that provides advice on science, technology and innovation for sustainable development. Dr. Avery is a fellow of the Institute of Electrical and Electronics Engineers, the American Association for the Advancement of Science, and the American Meteorological Society, for which she also served as president. She is a past chair of the board of trustees of the University Corporation for Atmospheric Research.

DR. BEN BOND-LAMBERTY is a research scientist at the Joint Global Change Research Institute, a collaboration between the DOE Pacific Northwest National Laboratory and the University of Maryland, College Park. Dr. Bond-Lamberty's research interests include carbon cycling, disturbance effects, ecosystem respiration, multiscale modeling, and climate change. His research concerns carbon and nutrient cycling in terrestrial ecosystems. Dr. Bond-Lamberty earned his PhD in 2003 from the University of Wisconsin in forest ecosystem ecology. He is a member of the American Geophysical Union, Ecological Society of America, and American Association for the Advancement of Science.

DR. ROBERT M. DeCONTO is currently a professor at the University of Massachusetts, Amherst. His work combines numerous disciplines within the Earth sciences, including atmospheric science, oceanography, glaciology, and paleoclimatology. DeConto's research interests include computer modeling of climate systems, and the dynamics of the Greenland and Antarctic ice sheets. Dr. DeConto is one of the world's leading Antarctic climate researchers and was awarded the 2016 Tinker-Muse Prize for Science and Policy in Antarctica, for his work on Antarctica's potential for past and future contributions to sea-level rise. Dr. DeConto received his PhD from the University of Colorado in 1996, followed by research appointments at NOAA, and the National Center for Atmospheric Research (NCAR).

DR. ANDREW G. DICKSON is a professor of marine chemistry in the Marine Physical Laboratory division at the Scripps Institution of Oceanography. Dickson's research focuses on improving our understanding of the chemistry of carbon dioxide in seawater, with a current emphasis on the effects of ocean acidification. He has played a key role in developing quality control standards for oceanic carbon dioxide measurements and leads a program to prepare, certify, and distribute CO_2 reference materials to the world's marine scientists. Prior to joining Scripps, Dickson served as a postdoctoral research associate at the Marine Biological Association Laboratory in Plymouth, England and as a postdoctoral associate in the University of Florida, Department of Chemistry. He joined Scripps as an assistant research chemist, became an associate research chemist, a professor-in-residence of marine chemistry, and then a professor. Dr. Dickson's laboratory participates in hydrographic cruises sponsored by the Climate Variability and Predictability (CLIVAR) project of the World Climate Research Programme. He is also part of a multiinstitutional collaboration to study the implications of ocean acidification on a variety of organisms that are important to US west coast fisheries. Dickson is a member of the OceanSITES Data Management Team and the PICES Section on Carbon and Climate. He has served as editor or as an editorial board member of several journals, including most recently Journal of Geophysical Research, Oceans. Dr. Dickson received a B.Sc. degree and a PhD from the University of Liverpool.

DR. PHILIP B. DUFFY is currently the president and executive director of the Woods Hole Research Center. Dr. Duffy is a physicist who has devoted his career to the use of science in addressing climate change. His research interests include climate change impacts adaption, extreme weather risk, hydrological impacts of climate change, and climate modeling. Prior to joining WHRC, Dr. Duffy served in the White House National Science and Technology Council as the Senior Advisor to the US Global Change Research Program, and as a Senior Policy Analyst in the White House Office of Science and Technology Policy. In these roles he was involved in international climate negotiations, domestic and international climate policy, and coordination of US global change research. Before joining the White House, Dr. Duffy was Chief Scientist for Climate Central, an organization dedicated to increasing public understanding and awareness of climate change. Dr. Duffy has held senior research positions with the Lawrence Livermore National Laboratory, and visiting positions at the Carnegie Institution for Science and the Woods Institute for the Environment at Stanford University. He has a bachelor's degree from Harvard and a Ph.D. in applied physics from Stanford.

DR. CHRISTOPHER B. FIELD (NAS) is the Perry L. McCarty Director of the Stanford Woods Institute for the Environment and Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies at Stanford University. His research focuses on climate change, ranging from work on improving climate models, to prospects for renewable energy systems, to community organizations that can minimize the risk of a tragedy of the commons. He was, from 2008 to 2015, co-chair of Working Group II of the Intergovernmental Panel on Climate Change, which led the effort on the IPCC Special Report on "Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation" (2012) and Working Group II contribution to the IPCC Fifth Assessment Report (2014). Field's research has been recognized with several American and international awards, including the Max Planck Research Award and the Roger Revelle Medal, and with election to learned societies, including the National Academy of Sciences (2001). Field received his PhD from Stanford in 1981 and has been at the Carnegie Institution for Science since 1984.

DR. JAMES L. KINTER, III is director of the Center for Ocean-Land-Atmosphere Studies (COLA) at George Mason University, where he manages all aspects of basic and applied climate research conducted by the Center. Dr. Kinter's research includes studies of climate predictability on sub-seasonal and longer time scales, focusing on phenomena such as monsoons, El Niño and the Southern Oscillation, and modes of extratropical variability. Dr. Kinter is also a professor in the department of Atmospheric, Oceanic and Earth Sciences of the College of Science. He is affiliated with the Climate Dynamics Ph.D. Program, having responsibilities for curriculum development and teaching undergraduate and graduate courses on climate change, as well as advising Ph.D. students. After earning his doctorate in geophysical fluid dynamics at Princeton University in 1984, Dr. Kinter served as a National Research Council Associate at NASA Goddard Space Flight Center, and as a faculty member of the University of Maryland prior to helping to create COLA. Dr. Kinter has served on many national review panels for both scientific research programs and supercomputing programs for computational climate modeling.

DENNIS P. LETTENMAIER (NAE) is a distinguished professor at University of California, Los Angeles. Dr. Lettenmaier's research and area of expertise is hydrological modeling and prediction; water and climate; and hydrologic remote sensing. Prior to his time at UCLA, Dr. Lettenmaier was a professor of Civil and Environmental Engineering at the University of Washington from 1976-2014. He is an author or co-author of over 300 journal articles. He was the first chief editor of the American Meteorological Society Journal of Hydrometeorology, and is a past president of the Hydrology Section of the American Geophysical Union. Dr. Lettenmaier is a fellow of the American Geophysical Union, the American Meteorological Society, and the American Association for the Advancement of Science, and is a member of the National Academy of Engineering. He earned his Ph.D. from University of Washington in 1975.

LORETTA J. MICKLEY is a Senior Research Fellow at the John A. Paulson School of Engineering and Applied Sciences at Harvard University and a co-leader of the Harvard Atmospheric Chemistry Modeling Group. She received an MS in Chemistry from the University of Illinois at Chicago in 1990, and a PhD in Geophysical Sciences from the University of Chicago in 1996. Mickley's research focuses on chemistry-climate interactions in the troposphere. For example, she seeks to understand how short-term variations in weather and long-term climate change affect the composition of the atmosphere. She also studies the regional climate response to trends in tropospheric aerosols. Recent research topics include the impact of climate change on surface air quality, the effects of changing wildfires in the western U.S. on air quality and health, and the influence of anthropogenic pollution on Arctic climate change.

DR. DANIEL J. VIMONT is a Professor in the Atmospheric and Oceanic Sciences Department at the University of Wisconsin, Madison. He also the Director of the Nelson Institute Center for Climatic Research, and serves as co-chair of the Wisconsin Initiative on Climate Change Impacts (WICCI). Dr. Vimont joined the faculty in the Atmospheric and Oceanic Sciences Department at the University of Wisconsin-Madison in 2003. His research interests include understanding mechanisms of climate variability and climate change, interactions between weather and climate, and global and regional impacts of climate change. In support of these research interests, Dr. Vimont uses observational analyses, designed experiments using models of varying complexity, simple and advanced statistical techniques, and theoretical analyses. In his role as co-chair of WICCI, he is interested in organizational structures that enable sustainable management within complex adaptive systems. Dr. Vimont received his Ph.D. from the University of Washington in 2002 under the direction of David Battisti and Ed Sarachik. After a brief post-doctoral appointment at the Joint Institute for Study of the Atmosphere and Ocean (JISAO) and the Columbia University Earth Institute, he joined the Department of Atmospheric and Oceanic Sciences and the Nelson Institute's Center for Climatic Research at UW-Madison.