

**U.S. Climate Change Science Program  
Synthesis and Assessment Product 3.3**

**Weather and Climate Extremes in a Changing Climate**

**Response of the Authors**

**to the**

**National Research Council  
of the National Academies'**

**Review of the Draft Report**

Responses are keyed to comment numbers used in the NRC Report with specific Review Panel wording in *red italics*, followed by the response.

## Major Comments

- 1. The authors have provided a broad and useful assessment of this important issue. The review committee commends the U.S. Climate Change Science Program (CCSP) and the authors of the draft SAP 3.3 for producing a broadly formulated and generally solid assessment of the scientific underpinnings of this most important topic. Indeed, the committee agrees that potential changes in extreme weather and climate events resulting from global climate change have serious socio-economic and environmental implications. In seeking to address this important issue, the authors have provided a document that addresses the goals, objectives, and intended audiences, including scientists, policymakers, resource managers, stakeholders of climate change science, the media, and the general public, as set forth in the document prospectus; all should benefit from the information provided in this Product.*

Response: Thank you and we agree this draft provides a useful basis for this assessment.

- 2. The Abstract and Executive Summary are inconsistent with the document content. The Abstract and the Executive Summary are disconnected from the material presented in the four chapters of the document in tone and scope. The Abstract in particular takes an overly alarmist tone. The authors should ensure that the language is tempered to reflect the implications of changes in extreme events only as supported by the scientific material presented elsewhere in the document. The committee understands that the front material should be understandable and readable for a lay audience with a high school education; however, the scope of this material falls short of that target. The committee suggests that the technical level could be increased and the readability would remain appropriate.*

Response: We have addressed the NRC suggestions as detailed in the Abstract and Executive Summary responses.

- 3. The content is weighted excessively toward tropical cyclones. The committee understands that trends in tropical cyclones (intensity, number of storms, and other characteristics) are an important topic of considerable interest to many audiences; however, in the context of this SAP, they are but one of several types of extreme events with significant socio-economic consequences. Please see specific comments on the relevant sections of the draft document for the committee's suggestions on how to reduce and consolidate the discussion of tropical cyclones.*

Response: The tropical cyclone sections have been significantly reduced in length following suggestions from the NRC Panel.

- 4. Some claims of trends in extreme events are insufficiently supported. In some cases, trends are inferred and trend lines are drawn on figures when the data do not appear to justify it. Key issues are whether a given time series is long enough to infer or deduce a trend, whether the underlying data are of sufficient homogeneity to draw conclusions, and whether the trend is statistically significant. In the case of the latter, there are many instances where small changes in the start time for the time series produce changes in the magnitude of the trend that are probably non-significant. One notable example of this is the apparent difference in*

*the trend for Atlantic tropical cyclones when the start time is 1880 versus 1900. In this case and others, such a difference may reflect a problem with the trend assessment technique rather than an actual signal.*

*In general, the word “trend” is used too loosely and often interchanged with “variation” or “increase.” These words should be associated with precise statistical definitions. Furthermore, when statements are made, the authors should indicate whether the claim is based on rigorous statistical analysis of a particular dataset (or datasets), expert elicitation, or the informed judgments of the authors.*

Response: A sizeable number of changes have been made, as follows:

(1) Since this is an assessment, we are limited by what is reported in the published articles. Where available in these articles, we have added information (often in footnotes) about the statistical significance of the findings. In a few cases, we have performed additional statistical analyses which are now in the report as an appendix. These include the trends in extreme heat and cold waves and extreme precipitation event frequencies.

(2) One particular concern of the panel is the lack of consistency in statistical significance results for adjusted tropical storm count trends beginning in 1878 (insignificant) vs. 1900 (significant) suggesting that the results imply problems with the trend assessment method. Further statistical testing methods are being explored which are not as dependent on the AR (1) persistence assumption as two of the three methods used, but the results are not yet submitted for publication and available for the report. We state in the revised version:

“The trend from 1900 begins near a local minimum in the time series and ends with the recent high activity, perhaps exaggerating the significance of the trend due to the presence of strong multidecadal variability. On the other hand, high levels of activity during the late 1800s, which lead to the insignificant trend result, are indirectly inferred in large part from lack of ship track data, and the uncertainty in the late 1800s storm counts is greater than that during the 1900s.”

This emphasizes that the real issue is not merely a problem of identifying the proper statistical model for noise. One must also accept the inferred adjustment as reliable in order to lend credence to high levels of activity leading to the insignificant result from 1878. This is another unsettled issue as uncertainty grows as one goes further back in time, with sparser ship track data. Therefore, we retain both trend results in the report, augmented by the caveat/discussion above. Further work is progressing on this issue outside of the report.

- 5. The levels of uncertainty inherent in the trends should be discussed in more detail. The levels of uncertainty associated with trends (both observed and projected) in various types of extreme events should be elaborated upon. For an observed trend in a particular type of extreme event or variable, please discuss the underlying scientific and technical reasons for that uncertainty, and discuss its implications for projected trends in the extreme event or variable in question. When feasible, compare and contrast the issue for a particular variable to the underlying issues for other variables. As an example, consider that it is reasonable to*

*assert that more is known about observed trends in temperature than trends in heavy precipitation, and even less is known about trends in the frequency and/or intensity of tropical cyclones. What are the technical reasons for this and what are the implications for projected trends?*

Response: See the response to the previous comment for further discussion of trend uncertainty in the tropical storm count context. Of note is that the report contains the authors' assessment of likelihood levels, and that those are lower for the tropical cyclone trends than for temperature trends. This is now also described in a box in the Executive Summary.

6. *Some cited material is not yet published. Some key literature cited in the report was not available to the peer review committee. In many cases, the literature in question was cited as "to be submitted." The committee understands that all literature cited and used as scientific evidence in SAP 3.3 should have reached at least "in press" status by August 2007. The authors of the SAP should ensure that all cited works are publicly available before the public release of the SAP. The committee further recommends that the authors use caution in drawing too heavily on papers and information that are not yet scientifically mature. The authors should minimize reliance on "grey literature" and non-refereed works.*

Response: We agree with this recommendation, and the Co-chairs and CLAs have emphasized the need for the author team to rely heavily on published literature throughout the preparation of this assessment. The public review draft version contains more than 820 citations. A count performed late in its development showed that 93% of the citations referenced articles that had been published as of August 1, 2007, 4% referenced articles were "in press" as of August 1, 2007, and 3% referenced articles were awaiting "in press" status at that time.

7. *The discussion of drought should be strengthened. The discussion of drought is not consistent among the chapters and sometimes contradictory (in terms of observed trends). In some cases, apparent trends for particular geographic regions are used to make statements on broader geographic trends that are not justified. This discussion could be strengthened by including a figure for precipitation analogous to Figure 2 in Chapter 1. Notwithstanding the discussion of observed trends and projections, the background information on droughts should better define the different types of drought (e.g., meteorological, hydrological, agricultural, etc...) and the uncertainties associated in quantifying drought severity using the Palmer Drought Severity Index (PDSI) and other indices. We note that PDSI is the only index discussed in the draft document. Furthermore, the document should address the uncertainty associated with climate model design (e.g., the model treatment of land-surface processes and parameters) and its impact on model representation of drought conditions.*

Response: We have added additional discussion about drought and the various ways to define it including additional indices. The length of this section has been increased from about two to seven pages. We have included a rather comprehensive discussion of drought indices and a more expansive discussion of findings for Canada and Mexico. A time series of the percent area with severe to extreme drought for North America was added to Figure 2.6.

8. *The discussion of ecological impacts should be expanded. The draft briefly describes ecological impacts. It would be helpful to expand on these and carry them through with brief pointers, elsewhere in the document, particularly in discussions of future impacts in Chapter 3. The committee recognizes CCSP SAP Goal 4 addresses the subject of impacts in detail, but those SAPs address impacts of climate change in the broader sense and do not necessarily address per se the impacts of extreme events. SAP 3.3 should acknowledge the impacts addressed in other SAPs and incorporate some by example. Impacts to consider by example include wildfires and heat stress, which are “compound impacts” of temperature and moisture extremes.*

Response: SAP 3.3 is focused on the questions that were listed in the Prospectus outlining the contents of the report. These included defining extremes in relation to impacts, but did not continue to include the actual discussion of these impacts. We agree, however, that it is desirable to recognize the importance of assessing the impacts of weather and climate extremes in a changing climate, and have chosen several examples of impacts that are now included and carried through the Chapters 1 to 3.

9. *The continuity and cohesion among the chapters needs improvement. The individual chapters read as stand-alone documents. They should be connected with some common themes and examples that are carried through (not just in terms of impacts as discussed in 8 above). There is a considerable amount of overlap and repetition, particularly between Chapters 2 and 3. Each chapter authorship team should coordinate with the other three teams to ensure that redundancies are eliminated.*

Response: We agree and have eliminated overlap and duplication.

10. *The recommendations are not properly organized. Recommendations, and statements that are in effect recommendations but are not labeled as such, are scattered among the chapters. Some of these are repeated in Chapter 4 and some are not. Recommendations should be combined and contained only in Chapter 4 and a highlighted (bold) short sentence corresponding to each recommendation should appear in the Executive Summary.*

Response: We have addressed this concern. Recommendations made in the first three chapters are now carried forward to Chapter 4 and also integrated into the Executive Summary.

11. *The Preface should not contain scientific or technical material. The Preface should contain “big-picture” information on the CCSP and the Synthesis and Assessment Products, and some background on the process and motivation pertaining to SAP 3.3. From a technical writing perspective, a preface is the appropriate location for framing and context; it should not contain technical information that is presented elsewhere in the report. Please see Chapter 3 of this review for more specific suggestions for content.*

Response: The Preface has been revised as suggested.

*12. The content is limited in geographical scope. The rationale behind the minimal coverage of regions outside the North American mainland (e.g. “Hawaii, Caribbean, and U.S. Pacific Islands”) should be explained. The SAP focuses heavily on North America, but the prospectus and the title suggest some appreciable coverage of other geographic locations.*

Response: There are limitations to the data and analyses in these other regions as we now note in the Preface, but we have added more discussion about tropical cyclones in these regions. This is a primary concern for these regions with respect to extreme events.

## Abstract

*The Abstract should read as a summary of the Executive Summary or a “one pager” for policymakers. In its current form, the Abstract provides a sufficient summary of the key issues; however, the tone is inconsistent with the balanced and objective tone projected elsewhere in the document. The authors of the document’s four chapters should ensure that the language of the revised Abstract is not alarmist and that statements are supported by the scientific content provided.*

Response: This has been addressed through responses to specific suggestions.

### Specific Suggestions:

1. *Line 10: Make this sentence consistent with the Intergovernmental Panel on Climate Change (IPCC) language it alludes to and refer to the source IPCC material. For example, begin the sentence with “The IPCC assessment concluded...” In addition, replace “planets” with “Earth’s atmosphere and oceans,” or a similar phrase.*

Response: Done.

2. *Line 11: The second or third sentence should state that, unlike the IPCC assessment, the SAP focuses on North America (this information is currently withheld until Line 17).*

Response: Done.

3. *Line 16: The committee feels that it is unjustified to refer to trends in drought since 1950 over North America. If the authors of the SAP disagree, they should justify disregarding data prior to 1950. The observational record over North America is sufficient to infer trends beginning prior to the 1930s, when the most significant drought in the record occurred.*

Response: We now reference and discuss other drought measures over Canada, US and Mexico. The document also now explains the data limitations over North America and describes the changes back to the earliest period of reliable data.

4. *Line 18: Please see comment on Line 220 regarding the use of the phrase “hurricane activity.”*

Response: The comment has been reviewed and the phrase “hurricane activity” is not used in the revised Abstract.

5. *Line 21: Longer duration aggravates the impacts of heat waves, droughts, downpours, and to some extent tropical cyclones. This paragraph should be modified to reflect this consideration, consistent with statements in the body of the SAP.*

Response: The suggested modification has been made.

- 6. Lines 29-30: Erosion and inundation of coastal lands are arbitrary selections of impacts. The committee suggests either removing these two impacts or expanding the list.*

Response: The selection of these events are not arbitrary, but rather reflect compound events which the NRC recommends we should address later on in their review. For example, the rise of sea level, retreat of sea ice, and increase in intensity of extratropical storms leads to increases in coastal erosion and inundation in Alaska. Sea level increases with increased tropical storm intensity leads to similar impacts in areas farther south.

- 7. The Abstract should mention conclusions made within the body of the report on mid-latitude cyclones (blizzards or “nor’easters”), severe thunderstorms, and tornadoes. All other phenomena discussed in the report are mentioned in the Abstract.*

Response: The suggested modification has been made.

## **Preface**

*The committee suggests that a preface should not contain scientific or technical material; rather, it should provide background information and outline the process that led to the formulation of the document. In this case, the background information should outline purpose and goals of the U.S. Climate Change Science Program (CCSP) and the SAPs, and provide a brief summary of the purpose of SAP 3.3. The Preface in its current form does address the purpose of SAP 3.3; however, background on the CCSP is absent. Remaining material of a technical nature should be moved to other sections of the document.*

Response: The Preface has been revised as suggested.

## **Specific Suggestions:**

- 1. Display and discuss Figure 1 in Chapter 1 and in the Executive Summary, but not in the Preface.*

Response: Agree, this has been done.

- 2. Place Figure 2 in a “Box” and compare it to the analogous table in the IPCC assessments, by displaying the IPCC figure or via a description of that figure.*

Response: We disagree with this suggestion. We believe that adding another box with another assessment of different likelihood ranges would lead to confusion. We have, however, made reference to other approaches to expressing likelihood statements. Those readers interested in how others have addressed likelihood can read the referenced material, and for an overview can refer to SAP 5.2.

- 3. Acknowledge the similar figure included in SAP 5.2, which was originally conceived to provide guidance for communicating uncertainty in the formulation of other SAPs. Note that*



*SAP 5.2 is currently under revision but should be released publicly before SAP 3.3 is finalized.*

Response: We have referenced SAP 5.2 as suggested.

## **Executive Summary**

*The Executive Summary is more balanced in tone than the Abstract; however, the committee notes several instances where statements in the Executive Summary are not well supported in the four chapters. The committee does not necessarily disagree with these statements, but only recommends that the authors ensure they are supported if they are included in the Executive Summary. In particular, and in keeping with Major Comments 4 and 5, claims of trends (increases or decreases), which are listed at length, should be rooted consistently in statistical significance and the underlying uncertainties summarized.*

*The Executive Summary contains several sections with seemingly random, single-sentence paragraphs that should be consolidated, when possible, into coherent themes. Section 6 should be revised to concisely and adequately reflect the recommendations provided in Chapter 4 (see Major Comment 10).*

Response: The Executive Summary has been revised to address these concerns.

## **Specific Suggestions:**

- 1. Lines 26-29: This paragraph is vague and may be interpreted as alarmist; it should focus on some specific extremes that definitely appear to be changing (e.g., maximum temperatures and precipitation intensity). At a minimum, the committee suggests inserting “some” before “extremes” on line 26.*

Response: We have provided several examples as suggested and revised the paragraph.

- 2. Line 66: This sentence on problems of climate models simulating extremes is correct, but it is not obviously related to the previous two sentences, which describe that changes in climate averages imply changes in the tails of the distribution and hence in climate extremes. Perhaps a more direct connection could be made or the sentence put in a separate paragraph where the difficulties for models in simulating extremes are elaborated a little, including both resolution and process limitations.*

Response: We agree and have revised our wording and provided a diagram to describe the issue of changes in the averages as they affect the extremes.

- 3. Line 87: What is the conclusion of this paragraph?*

Response: This paragraph has been completely revised.

4. *Line 113: What does “are likely to be attributable to” mean? Does it mean that they have been attributed to, or does it mean they would be attributed to, but the relevant studies haven’t been done yet? There are no formal attribution studies that attribute the global changes in these phenomena to anthropogenic forcing. There is a single attribution study on these by Christidis et al. (2005) but it only considers limited global coverage, as there are no data in many regions.*

Response: The text in this line has been revised and now refers to attribution of SSTs in the main Atlantic and Pacific tropical cyclogenesis regions based on work of Santer et al. (2006) and Gillett et al. (2007).

5. *Line 118: In this case there are a number of studies that have detected and attributed observed changes in Sea Surface Temperature (SST) to increasing greenhouse gases. Does this statement refer to global average SST or regional SST in the tropics?*

Response: The statement refers to regional SST in the tropics, (see comment above).

6. *Line 120: There is a new study by Christidis et al. (2007) that attributes observed increases in growing season length (GSL) (based on a simple temperature threshold to define GSL) globally and in North America to increasing greenhouse gases. The main changes are associated with earlier spring onset and are consistent with observed changes in the frost-free period. This should be mentioned as it is just as robust as the other attribution studies on changes in temperature extremes.*

Response: Agreed, we have not included this study. We will include it in Chapter 3 when it is revised after the public review if the paper appears sooner than the deadline for publications to be cited.

7. *Line 121: Sections 2.3 and 3.1 list societal and ecological impacts for some forms of severe weather but not others. All paragraphs could benefit from some examples of impacts.*

Response: We have now noted the impact of several events to many of the observed and projected changes.

8. *Line 164: There appears to be no basis for this statement in Chapter 2 on observed changes.*

Response: The sentence has been removed.

9. *Line 170: If the authors agree that the material presented elsewhere in the document illustrates increases in extreme precipitation, substitute “increases” for “changes.”*

Response: Done.

10. *Lines 192-197: These statements concerning drought are acceptable, but they are inconsistent with the Lines 202-203, “... it is likely that the increasing temperatures are already contributing to droughts that are longer and more intense.” The committee could*

*find no evidence in the material presented (the observed record) of longer, more intense droughts relative to the 1930s and 1950s droughts over North America. These statements also do not comport with the perception conveyed in the Abstract that drought is increasing over North America since 1950.*

Response: We have extensively revised our drought discussion to properly reflect the extreme droughts of the 1930s and 1950s.

*11. Line 205: Section 4.3 boldly asserts increased evapotranspiration and decreased spring runoff in the mountains without providing any compelling evidence. First, it is likely that potential evapotranspiration will increase because of warmer air temperatures but actual evapotranspiration will decrease because of decreased soil moisture. Second, the authors state elsewhere (Lines 182-183) that increased spring snowmelt (in higher latitudes) may contribute to extremes in river flooding. Does the first assertion apply only to mountains at low latitudes? Does the second apply only to lowlands at high latitudes? Please clarify this. The peak in spring snow melt run-off is likely to occur earlier, exacerbating the problems of summer water availability, but it unlikely to decrease in magnitude.*

Response: We have clarified that we are discussing the potential for evapotranspiration and we discuss the limitations of such an approach. We have added and clarified material related to the earlier runoff in the mountainous western United States as it affects summer water reliability.

*12. Line 220: This section concludes that it is “very likely” that hurricane activity is increasing. The case seems weaker in light of the general uncertainty in the database and Figure 2.30. Moreover, hurricane activity is defined here specifically in terms of frequency and destructive power yet the phrase is used throughout the document in seemingly less definitive ways. The authors should ensure that when the phrase “hurricane activity” is used anywhere in the document, it refers to a precise and consistent concept.*

Response: We have removed the term “hurricane activity” and clarified what specific aspects of hurricane intensity, frequency, or duration we refer to.

*13. Line 227: If the significance of the trends in hurricane frequency is sensitive to a small change in the start date of the period, it may mean that the statistical estimation of the trend significance has not been performed correctly or that assumptions in the noise model are inappropriate. The data do not appear to support a real significant trend in hurricane frequency. This sentence is not appropriate for the Executive Summary.*

Response: We have clarified the reliability, trends and changes in the data set. We now present linear trend results (and statistical significance results) for hurricane frequency using a range of start dates from 1895 to 2005 and discuss this in a new Appendix. We show that the significance depends on the start date. Similarly to the tropical storm counts, the results are not significant if one includes relatively high levels from the 1800s (which are also more uncertain than the counts from the 1900s.)

*14. Line 245: Over what time period?*

Response: The time period has now been defined.

*15. Line 248: The document provides a case for increasing severity of hurricanes but the case for increasing frequency is much less compelling. Thus the changes in activity (see comment above) may reflect changes in severity.*

Response: The above statement is true for future projections, but for past observed data the case is stronger for an increase in frequency of hurricanes than for an increase in severity. We have now clarified and described the case for increasing frequency through the use of new terminology.

*16. Line 302-303: This trend is not supported by an analysis for statistical significance.*

Response: We have added an appendix to clarify statistical significance of trends.

*17. Lines 310-315: The authors state that data are not adequate to make definitive statements on observed changes in tornadoes and severe thunderstorm (Lines 310-312). Then, in an apparent contradiction, they state (lines 312-313) that data related to severe thunderstorms 'are reliable' (suggesting that the presence or absence of trends in conditions can be determined). The trends in conditions for severe thunderstorms (lines 313-315) have not been shown in the text, and in fact are inconsistent with material presented in Chapter 2 (Lines 144-145; 154-157). Unless the trends in conditions are shown, this statement should be restricted to simply state that the data are not adequate to make definitive statements about trends in severe thunderstorms and tornadoes.*

Response: We have addressed this issue and revised our text.

*18. Lines 327-329: This paragraph does not specify a particular region within the Pacific basin. It seems unlikely that the statement applies to the entire basin. Furthermore, this paragraph relies on Figure 5 to justify its conclusions. If Figure 5 applies globally, the authors should briefly state why this figure supports their conclusion regarding increased extra-tropical cyclone severity. Chapter 2 suggests extra-tropical cyclone severity off the U.S. east coast has decreased because the tracks have shifted northward. The authors should comment on whether this trend is expected to continue.*

Response: This section has now undergone substantial revision to clarify the discussion of observed and projected storm track changes. We agree that the previous text was subject to misinterpretation.

*19. Line 379: The inflation-adjusted curve in Figure 1 does not compellingly support the notion extreme events are changing because of a changing climate. If 2005 (Katrina) is removed, there is no significant upward trend. Moreover, any increase in damage (in terms of dollars) could be attributed to increased vulnerability (e.g., expanding infrastructure and population near the coastlines) as much as changes in extreme events. The report needs something more*

*compelling. One possible alternative is to disaggregate the data by phenomenon (e.g., provide inflation-adjusted trends corresponding to the parameters presented in Figures 2-5).*

Response: We now use Figure 1 (now Figure 2) only to show that extreme events have important economic impacts and make no reference to trends either positive or negative.

## Chapter 1

### Why Weather and Climate Extremes Matter

#### Major Comments:

- 1. Weather and climate extremes matter primarily because of their socio-economic and environmental impacts, yet the Chapter does not discuss socio-economic impacts in any significant detail. A good starting place for such a discussion would be to include an improved version of Figure 1 from the Executive Summary in this chapter (see above for specific suggestions on how to improve this figure). The authors should also consider providing supporting material for an improved Figure 1. Supporting material could include facts and figures illustrating the migration of the U.S. population to vulnerable coastal areas and time series of deaths due to extreme weather events.*

Response: The socio-economic impact has now been increased. Figure 1 from the Executive Summary has been moved to Chapter 1 and improved by combining it with two additional figures that are new to the chapter (See the current Figures 1.1, 1.2, and 1.3) and supporting information. Reference to population growth has been made. Additional information about deaths due to extreme weather events and the lack of trend in these deaths is now included in the paper.

- 2. Chapter 1 provides material on many topics but the topics are not well connected. The lack of cohesion is apparent in the architecture of the sub-sections, wherein there are multiple one-sentence paragraphs that do not always support a more general theme. One remedy would be to combine these short paragraphs; another is to enhance each of them by developing a stronger lead sentence and providing supporting material. In addition, the boxes at the end should be integrated into the chapter rather than grouped together. In some cases, these boxes could be moved into other chapters (e.g., Box F could be moved to Chapter 2).*

Response: Chapter 1 did not and does not have any one-sentence paragraphs, so no action is necessary. The boxes will be integrated into the chapter in the final publication. Box F has been moved to Chapter 2 as recommended.

#### Specific Suggestions:

- 1. Line 31: This is an excellent recommendation but it should be augmented to explain why it is important. For example, Chapter 4, Figure 1, which states “Mitigation of adverse impacts through better planning and decision making.” The recommendation on Line 31 should include language to this effect.*

Response: Agreed. The recommendations have been rewritten.

- 2. Line 66: This sentence is unclear and Figure 1 is not very effective. Figure 1 should be removed or revised to differentiate between natural and human systems.*

Response: Agreed. This sentence and the figure it refers to have been removed from the chapter.

3. *Line 68: This chapter should emphasize a key point: Changes in extremes are key mechanisms by which climate change affects society and the environment. IF these changes can be forecast reliably, society can adapt to minimize their impacts. This point could be the lead-in sentence to the paragraph starting on line 68. The authors should consider moving the resulting paragraph to the beginning of the chapter.*

Response: Agreed. Done.

4. *Line 83: Section 1.3 is a summary but not a catalogue of extreme event indicators. Moreover, there probably should be a catalogue to help explain what extremes were considered and why. This report should provide a first pass at such a catalogue, as the authors imply that future work should refine a catalogue based on dialogue with the stakeholder community (lines 34-37 and Chapter 4, lines 255-258).*

Response: We agree that such a catalogue would be useful and will try to produce one for inclusion in the final report.

5. *Line 103: The authors should replace “determine the probability” with “estimate.”*

Response: Agreed. Done.

6. *Line 109: Insert “some” before “extreme events.”*

Response: Agreed. Done.

7. *Line 110: Figure 2 is a valuable figure but its value is diminished because of the lack of discussion. The authors should walk the reader through the figure more carefully, perhaps looking at some specific numbers and examples (e.g., in the previous climate the probability of seeing a 10° temperature was 10%, when the mean shifts, the probability becomes 20%).*

Response: Agreed. Done.

8. *Line 127: The title of Section 1.1.2 is ambiguous. One possibility might be to rename it, “Important Characteristics of Extremes.”*

Response: Agreed. Section titles have been changed to make them clearer.

9. *Line 133: The authors raise important points but then illustrate them with examples that are relatively unimportant. For example, it may not be important that the tornado season shifts if season duration is unchanged. More important examples might include: (a) time of the first snow melt (earlier in the Sierras means longer dry season with far reaching impacts on the ecology); (b) time of the wild-fire peak (earlier might pose threats to certain species).*

Response: Agreed. These excellent suggestions have been incorporated in the document.

*10. Line 144: The authors introduce the term “morphology” and use it very ambiguously throughout the report. The authors seem to define morphology to mean the detailed characteristics and properties of an extreme. The authors could in some cases employ more direct terminology; perhaps “characteristics” or “properties” would suffice.*

Response: Agreed. Done – though we still use “morphology” once.

*11. Line 151: The details of this discussion are quite good but the organization needs improvement. One possibility is to move Sections 1.1.3-4 into 1.2 since these sections focus largely on vulnerabilities.*

Response: Agreed. The chapter has been reorganized.

*12. Line 167: “Enormous” may be an overstatement if one considers the numbers of deaths resulting from other natural hazards and disasters (e.g., the Indian Ocean Tsunami).*

Response: Agreed. “Enormous” has been changed to “considerable.”

*13. Line 170: Consider combining sections 1.1.3 and 1.1.4.*

Response: Agreed. The chapter has been reorganized.

*14. Line 208: This sentence fails to summarize the important point. Consider replacing “the relationship between climate and society” with a phrase similar to “calculating losses due to extremes.” In addition, “statistics” can be deleted.*

Response: Agreed. Done.

*15. Line 212: This is a powerful statement but the authors offer no justification or evidence to support it.*

Response: The sentence has now been rewritten in a non-controversial manner.

*16. Line 222: Consider substituting “probability of extremes” for “magnitude of the exposure to which the system is subjected” if this statement refers to extremes but not vulnerability.*

Response: Agreed. Done.

*17. Line 243: The authors should point out that there is a lack of quantitative proof that adaptation and mitigation saves money. Many decision makers will not support the cost of mitigation unless the cost-benefit relationship is known.*



Response: We disagree with the Committee's view that there is a lack of quantitative proof that adaptation and mitigation saves money. For example, see <http://www.dca.state.fl.us/fdcp/dcp/chhsc/IBHS.pdf>, <http://www.renre.com/pdf/HurricaneCharleyDamageSurvey.pdf>, and the peer-reviewed paper we now cite in that part of the chapter to document the benefit from mitigation activities.

*18. Line 272: The concept of a binary classification is introduced yet it is not employed elsewhere in the report (e.g., in the boxes at the end of this chapter other chapters). Consider employing the concept elsewhere in the report. Please clarify the concept when it is first introduced and explain how a binary tree would work.*

Response: Upon review, we decided it was best to remove the binary classification discussion.

*19. Line 325: The last six words are emphatically policy prescriptive and should be removed altogether. The authors should not recommend courses of action for policymakers.*

Response: Agreed. Done.

*20. Line 355: This paragraph should be re-written using simpler terminology. At a minimum, "evolutionary" in first sentence should be replaced with "behavioral." The present paragraph implies that while some species have shown behavioral adaptation to on-going climate change, there are no species that have shown genetic adaptations (presumably, because they have not had time to evolve). If that is true then the paragraph deserves its own section and would not belong in Section 1.2.3 on Thresholds.*

Response: Agreed. The paragraph has been rewritten.

*21. Line 500: A recent study in press in the Proceedings of the National Academy of Sciences (PNAS) with Tom Knutson as a co-author has attributed the observed increase in coral bleaching in the Caribbean to anthropogenic forcing (Donner et al., 2007). This could be mentioned in this box.*

Response: Agreed. The results of this new article have now been included.

*22. Line 562: The sentence beginning on this line could be interpreted as a value judgment that compares human worth and suffering to animal or plant worth and should be deleted or phrased more precisely.*

Response: We disagree. This is not a value judgment but rather a restatement of comparative effects between natural systems, which don't have societal infrastructure to buffer them, and socio-economical systems that do. We are trying to write it as clearly as possible: "In spite of these statistics, disease outbreaks are normally strongly buffered by societal infrastructures (sanitation, medical care, and adaptive behaviors). Consequently, effects of climate change on human diseases are expected to be less dramatic than the effects on plant and animal diseases (Gubler et al., 2001)."

## Chapter 2

### Observed Changes of Weather and Climate Extremes

#### Major Comments:

- 1. In this chapter the authors address the key issues set forth in the document's prospectus regarding what is known about observed changes in extreme events. In several cases, however, the authors overstate the case for an observed trend. Such claims should be supported by the data, provided here or elsewhere in the peer-reviewed literature, with uncertainties quantified and the analyses of datasets subjected to tests for statistical significance. If the claims are based on expert elicitation and not data per se, this should be noted explicitly.*

NOTE: This Comment raises the same issues that were addressed in Major Comment 4 on the entire draft Synthesis and Assessment Product document. That answer is repeated below:

Response: A sizeable number of changes have been made, as follows:

(1) Since this is an assessment, we are limited by what is reported in the published articles. Where available in these articles, we have added information (often in footnotes) about the statistical significance of the findings. In a few cases, we have performed additional statistical analyses which are now in the report. These include the trends in extreme heat and cold waves and extreme precipitation event frequencies.

(2) One particular concern of the panel is the lack of consistency in statistical significance results for adjusted tropical storm count trends beginning in 1878 (insignificant) vs. 1900 (significant) suggesting that the results imply problems with the trend assessment method. Further statistical testing methods are being explored which are not as dependent on the AR (1) persistence assumption as two of the three methods used, but the results are not yet submitted for publication and available for the report. We state in the revised version:

“The trend from 1900 begins near a local minimum in the time series and ends with the recent high activity, perhaps exaggerating the significance of the trend due to the presence of strong multidecadal variability. On the other hand, high levels of activity during the late 1800s, which lead to the insignificant trend result, are indirectly inferred in large part from lack of ship track data, and the uncertainty in the late 1800s storm counts is greater than that during the 1900s.”

This emphasizes that the real issue is not merely a problem of identifying the proper statistical model for noise. One must also accept the inferred adjustment as reliable in order to lend credence to high levels of activity leading to the insignificant result from 1878. This is another unsettled issue as uncertainty grows as one goes further back in time, with sparser ship track data. Therefore, we retain both trend results in the report, augmented by the caveat/discussion above. Further work is progressing on this issue outside of the report.

- 2. The material on tropical cyclones is too lengthy. The committee provides several specific comments to indicate what material should be retained and what should be omitted in the revised SAP. In considering these comments, the authors should strive to capture the dimensions of the ongoing scientific debate vis-à-vis trends in tropical cyclones and climate change, while considering the need to limit the length of the discussion.*

NOTE: This Comment raises the same issues that were addressed in Major Comment 3 on the entire draft Synthesis and Assessment Product document. That answer is repeated below:

Response: The tropical cyclone sections have been significantly reduced in length following suggestions from the NRC panel.

### **Specific Comments and Suggestions:**

- 1. Line 69: This finding is inconsistent with earlier statements about drought (Line 17 of the Abstract and Line 193 of the Executive Summary)*

Response: This finding and the Executive Summary have been reworded and are now consistent. The Abstract is also now consistent, although length restrictions do not allow a description of all of the nuances.

- 2. Line 89: The change in start time from 1880 to 1900 is a small percentage of the overall length of the time series. Such a relatively small change should not impact the trend in a statistically significant manner. This difference likely reflects a problem with the trend assessment technique more than a difference in any actual trend in nature.*

Response: See response to major comment #4.

- 3. Line 103: There is no quantitative discussion accompanying the phrase “very unlikely” of the key uncertainties related to measuring the decadal variability.*

Response: This key finding was deleted.

- 4. Line 121: This does not rise to the level of a “key” finding.*

Response: We deleted the key finding.

- 5. Line 141: This key finding is repeated on line 1551 without any evidence. Please provide citations (if they exist) to support this.*

Response: A citation has been added.

- 6. Line 164: Are these regime changes associated with climate change issues? Is there evidence for a naturally varying climate system? Please provide citations (suggestion: Bell and Halpert 1995)? (Note: this would be a good location for Box F from Chapter 1).*

Response: We have taken the material in Box F of Chapter 1 and combined it with additional material related to temporal changes to produce a lengthy description of modes of variability, included as a box in this chapter (Box C). We now refer to this box at this point. Also, this chapter does not deal with attribution. To avoid possible misunderstanding, we changed “natural” to “integral.”

7. *Line 182: Delete “exactly.” As is, the sentence implies that exactness is the norm or at least is not rare.*

Response: Done.

8. *Line 184: What is meant quantitatively by “above to much above?” The National Oceanic and Atmospheric Administration (NOAA) defines these terms quantitatively. What are these definitions?*

Response: A footnote defining these terms was added at this point.

9. *Line 187: Insert anomalies after temperature[s].*

Response: Done.

10. *Line 224: This statement may be inconsistent with lines 242-243. How do “very extreme” heat episodes differ quantitatively from “warm spells?” (Note: Peterson et al., 2007 is not yet published). These data do not support the tone of certainty projected by sentence that begins on line 12 of the Abstract.*

Response: Any perceived inconsistency arises from the differing periods of analysis, the warm spell analyses beginning in 1950. We added a footnote providing a definition of the very extreme heat episodes. We also added a sentence pointing out that the behavior of extreme heat episodes is roughly consistent with that of warm spells for the common period of analysis. The length restrictions of the Abstract prevent inclusion of these nuances, but the Executive Summary does include such nuances.

11. *Line 261: To minimize confusion, please characterize all changes in terms of either frost day occurrence or length of the frost season, but not both.*

Response: Since published studies include analyses of both, we believe that it is important to include both. However, to minimize confusion, we have reorganized that paragraph and added text highlighting that these are two different ways to characterize the freezing threshold climatology.

12. *Line 338: Define Palmer Drought Severity Index (PDSI). Provide a reference. Discuss its strengths and weaknesses.*

Response: As part of a significant expansion of the drought discussion, we have added a box with the suggested PDSI discussion along with discussions of other indices.

*13. Line 424: Please quantify what is meant by “disproportionately” or strike it.*

Response: We have changed the sentence to the following more explicit statement: “...it is highly likely that in recent decades the extreme precipitation events have increased more than light to medium events.”

*14. Line 426: It would be useful to note parenthetically that 101.6 millimeters is equivalent to 4 inches. If there are other instances in the document where seemingly random metric quantities relate to “round” English system numbers, please provide the equivalents.*

Response: There were altogether four instances and the equivalents have been added in all cases.

*15. Line 491: Cavazos 2007 is not yet published and may not yet have even been submitted.*

Response: This manuscript has been submitted to GRL.

*16. Line 539: Please better define “90-days duration precipitation episode.” This may be a misstatement (e.g., 90-day accumulation may be the intended phrase).*

Response: The wording has been changed to “frequency of 90-day precipitation totals.”

*17. Line 544: How is the temporal behavior similar? Does the comment refer to multi-decadal variability?*

Response: The comment does refer to multi-decadal variability. We have changed the text to be explicit, as follows: “...both show moderately high frequencies in the early 20<sup>th</sup> Century, low values in the 1920s and 1930s, and the highest values in the past 2-3 decades.”

*18. Line 643: The annual-average global total of 90 with a variance of approximately 10 is consistent with Poisson statistics, and indeed a range of studies provide no compelling evidence to reject a Poisson model (Gray 1968; Katz 2002; Frank and Young 2007). Since the process of tropical cyclogenesis involves many disturbances each with low probability of becoming a storm one would expect this sort of model to work reasonably well. This should be acknowledged.*

Response: We’ve updated the text as follows:

“Each year, about 90 tropical cyclones develop over the world's oceans, and some of these make landfall in populous regions, exacting heavy tolls in life and property. Their occurrence is often statistically modeled as a Poisson process. The global number has been quite stable since 1970...”

*19. Line 656: The more commonly quoted damage total for Katrina is \$80B; \$110-120B is the generally quoted cost for the entire 2005 season (NHC 2006).*

Response: We now use \$80B as the damage estimate.

20. *Line 676: The discussion of data limitations on pp. 27-30 is generally balanced and accurate. Note that routine reconnaissance began in 1944 (not in 1945 as stated on line 715), and the National Weather Service (NWS) attributed the limited loss of life ashore in the "Great" Hurricane of that year to this reconnaissance.*

Response: We changed 1945 to 1944 and added the suggested sentence.

21. *Lines 744: Although cyclone-size data are invaluable for many aspects of impacts modeling, this discussion is not essential here and could be removed. At a minimum, the concluding statement of this paragraph is inflammatory ("failure of governments to take seriously...") and should be removed.*

Response: The concluding statement was deleted.

22. *Lines 754: The discussion of spectra of hurricane occurrence is not essential to the thrust of this chapter. It should be reduced to mention El Niño/Southern Oscillation's (ENSO) modulation of Atlantic activity and the existence of substantial multidecadal component.*

Response: The discussion and figure on spectra have been deleted.

23. *Lines 773: Please define the Power Dissipation Index (PDI) and comment that statistical significance of PDI trends is dependant upon corrections applied to the records, listing all references. The detailed accounts of dueling corrections will make a fascinating review paper at some point, but they do not contribute to the message here. The assessment of the science is that eminently qualified investigators cannot yet forge a solid consensus. Nevertheless, the paragraphs that begin on lines 798 and 804 should be retained verbatim.*

Response: We added a definition into the text, cited Landsea (2005), and noted the dependence of the increase in PDI since the 1950s on the adjustment to the wind speeds from that era.

24. *Line 812: The material beginning here and ending on line 891 reflects a level of detail entirely appropriate for a review article, but not for the document at hand. The readers (at the level of a "Scientific American" readership) need not know the details of the argument, but need to know that different, but reasonable detrending strategies can yield either large trends or trends at the margins of detectability.*

Response: This section of the text was shortened substantially.

25. *Line 906: This paragraph provides too much detail for the proposed audiences.*

Response: The paragraph was streamlined considerably.

*26. Line 924: This section should be condensed into one page or less, incorporating key references that span the range of evidence and informed opinion. The committee does not necessarily disagree with the statements in this section but that the level of detail is not appropriate for a SAP. These are issues that should mature in the peer-reviewed, technical literature before they are presented in an assessment document.*

Response: The section in question has been shortened dramatically. The material cited has at least been submitted for peer review. If not accepted for publication by the December cutoff date, the material will be deleted unless a special case can be made to keep a limited amount in. These are the guidelines that we are operating under for the report.

*27. Line 930: Figure 2.27 would be improved if it showed tropical cyclone numbers subject to different adjustment strategies and resulting uncertainties in the trends, such as is shown in 2.30 (note: Figure 2:30 is “in preparation”).*

Response: Figure 2.27 illustrates the covarying nature of SST and tropical cyclone counts under the assumption of no adjustment. Further analysis, such as Figure 2.30, considers the effect of some proposed adjustments for missing storms. Figure 2.30 is from a Vecchi and Knutson manuscript that has now been submitted for publication to the Journal of Climate.

*28. Line 965: The literature documents that historical SST data for these time periods constitute a well-maintained database with clearly defined, acceptably small errors. Again, this is a point that would be essential in a review targeted at a professional audience, but it does not serve the purpose here.*

Response: The SST data quality discussion has been deleted.

*29. Line 998: The lack of an increasing trend in landfalling Atlantic tropical cyclones is important. The committee recommends that the material on observed trends in Chapter 3 (e.g., Pielke) be combined with this material and that it all be placed here in Chapter 2. The lack of trend due to signal-to-noise problems has implications for projection into the future. The message is that landfalls may be described by a Poisson process, thus a low mean implies a larger standard deviation relative to the mean and hence a large stochastic component, regardless of any trends with basin-wide numbers and intensities.*

Response: We agree the lack of trend in landfalling storms is important. This finding appears in the Abstract of the entire report. The issue of signal to noise is raised in the landfalling PDI discussion:

“...Emanuel (2005b) notes that a PDI series such as Landsea's (2005) based on only U.S. landfalling data, contains only about 1 percent of the data that Emanuel's (2005a) basin-wide PDI contains, which is based on all storms over their entire lifetimes. Thus a trend in basin-wide PDI may not be detectable in U.S. landfalling PDI since the former index has a factor of 10 advantage in signal to noise ratio.”

Another aspect of the decreasing trend in landfalling numbers that bears mentioning are some mostly new analyses indicating that a decrease in landfalling numbers is not necessarily inconsistent with increases in basin-wide numbers. We have included reference to, and discussion on, published or accepted papers by Chang and Guo (2007) on the potential errors in the earlier data set (which are less than those used here), Kossin and Vimont (2007) on an association with the AMM and the potential impacts of global warming on this mode, and Landsea (2007) and Holland (2007) on whether the proportion of landfalling storms can be used for inferring basin wide statistics. The main discussion in this area is as follows:

“Landsea (2007) has used the fraction of storms striking land in the satellite and pre-satellite era to estimate the number of missing storms per year in the pre-satellite era (1900 to 1965) to be about 3.2 storms per year. This assumes that the fraction of all storms that strike land has been relatively constant over time, which has been shown to be incorrect by Holland (2007). Holland also shows that the smaller fraction of storms that made landfall during the past fifty years (1956-2005) compared to the previous fifty years (1906-1955) is directly related to changes in the main formation location regions, with a decrease in western Caribbean and Gulf of Mexico developments and an increase in the eastern Atlantic.

Alternative approaches to estimating the earlier data deficiencies have been used by Chang and Guo (2007), Vecchi and Knutson (2007), and Mann et al. (2007). The first two studies use historical ship tracks from the pre-satellite era, combined with storm track information from the satellite era, to infer an estimated adjustment for missing storms in the pre-satellite era (assumed as all years prior to 1965). Mann et al. used statistical climate relationships to estimate potential errors. Vecchi and Knutson found 2.5 storms per year were missing prior to 1900, decreasing to zero by 1960. Chang and Guo found 1.2 storms missing around 1910 also decreasing to zero by 1960. Mann et al. estimated a more modest undercount bias of one per year back to 1970. The adjusted time series by Vecchi and Knutson (Figure 2.19) suggest a statistically significant ( $p=0.003$  or less) positive linear trend in adjusted storm counts of 55%/century since 1900. However, beginning with the trend from 1878, the trend through 2006 is smaller (+15%/century) and not statistically significant at the  $p=0.05$  level ( $p$ -value of about 0.3). It is notable that the degree of increase over the past century depends on the analysis methodology. When using a linear trend, as above, the increase from 1900 to 2005 is around 55% in the adjusted storm counts. However, using the, essentially non-linear approach by Holland and Webster (2007) of separate climatic regimes, the increase in adjusted storm counts from the 1900-1920 regime to the 1995-2006 regime is 85%. The trend from 1900 begins near a local minimum in the time series and ends with the recent high activity, perhaps exaggerating the significance of the trend due to multidecadal variability. On the other hand, high levels of activity during the late 1800s, which lead to the insignificant trend result, are indirectly inferred in large part from lack of ship track data and the uncertainty in the late 1800s storm counts is greater than that during the 1900s.”

*30. Line 1069: Fig 2.30 contributes significantly. A paragraph based upon lines 1069-1086, but without the details of the significance testing should be retained.*

Response: The revised version follows this suggestion.



*31. Line 1087: The material presented from here through line 1165 should be reduced because it is largely redundant with the discussion that begins on line 976.*

Response: The material cited has been reduced dramatically.

*32. Line 1166: The review panel endorses the recommendation to pursue paleotempestology; it is a promising method to extend the short, heterogeneous instrumental record. Historical investigations of existing, but obscure, written archives also show promise. However, this section could probably be reduced to a paragraph or two. (Note: please clarify here or elsewhere that as used in the SAP, "paleo" refers primarily to the Holocene).*

Response: We reduced the original section substantially, as suggested, although we also added in some discussion of two new paleotempestology references that appeared after our draft report was prepared.

*33. Line 1227: The discussion of the climatic role is interesting, but it may not belong in this document. This is about extreme events, not about the maintenance of the general circulation by hurricanes.*

Response: This section has been deleted.

*34. Line 1247: Is Hart et al. (2006) yet published?*

Response: The section referencing this paper has been deleted.

*35. Line 1296: The committee believes that in fact there are data over the central North Pacific to analyze extratropical cyclones; it may be limited, but it exists.*

Response: This reference to a lack of data was referring specifically to radiosonde data over the jet-stream entrance region over the North Pacific Ocean (as noted in Harnik and Chang, 2003). To clarify this point, we've modified the text, and added a footnote to the section with a sentence regarding pireps (i.e., pilot reports) that are routinely available at the altitude of the jet-streams over the Pacific and Atlantic basins.

*36. Line 1352: This statement is not supported by Figure 2.36, which indicates no trends whatsoever.*

Response: We modified the text to explicitly respond to this comment regarding a lack of any observable trends in nor'easters in the Hirsch et al. (2001) article. In the original paper, they had found a marginally significant (90% conf. level) increase in average storm pressure, but no statistically significant trends in frequency for all nor'easters, or for those that occurred over the northern region (>35 N) of the domain or for those that traversed the entire East Coast of the United States. Therefore, several sentences were added to explain these details, and specific wording was added regarding the lack of any statistically significant trends in nor'easter frequency.

*37. Line 1355: It would help to insert a sentence or two on the importance of increasing sea level as it relates to the 'perfect storm' nor'easter.*

Response: A sentence about the potential impact of rising sea levels was added.

*38. Line 1475: Table 2 should indicate whether there are significant trends, despite the textual reference to the five largest wave occurrences each year.*

Response: A column has been added to the table indicating which of the wave height measures exhibit statistically significant trends.

*39. Line 1573: Please clarify what is meant by W-shaped. Does this refer to a figure in the Changnon and Karl reference?*

Response: This indeed refers to a figure in the Changnon and Karl reference. Because we do not include the figure in the report, we decided to delete the "W-shaped" phrase and added a bit more specific detail about the behavior.

*40. Line 1612: If there is not a trend, please reconcile this with the statement that severe weather environments have increased and then decreased (Lines 314-315 of Executive Summary and Figure 2.42).*

Response: This statement refers only to tornadoes. It says nothing about non-tornadic severe thunderstorms.

*41. Line 1630: These changes do not appear to be significant and may just be natural variations. The results here may be the justification for the statement in Exec Summary lines 313-315 but since the trends are likely not significant, they do not justify being brought forward to the Executive Summary.*

Response: Figure removed and text added to discuss statistical significance of the recent trend and its relationship to natural variability.

*42. Line 1648: Please clarify what is meant by the climate shift in 1976-77. If this shift is important, why is it not discussed elsewhere?*

Response: The discussion has been expanded. This is mentioned mainly in conjunction with the observed increase in El Nino activity.

*43. Line 1639: Enhance this section with follow-up discussion from lines 662-752 of Chapter 1. The discussion should relate suppression of hurricane activity in the Atlantic to the enhanced ENSO index. The observed changes that have occurred in Pacific-North America pattern (PNA) and North Atlantic Oscillation (NAO) are related to the surface storm track that you have already discussed earlier (lines 1249-1354).*

Response: This section has been enhanced as suggested by the review panel. In addition, the material on modes of variability in Chapter 1 has been moved here to produce a comprehensive discussion of this topic.

*44. Line 1768: This section could be moved to Chapter 3. It is about projections (not the observed record) and combining models to predict future extremes.*

Response: This material has been removed.

## Chapter 3

### How Well Do We Understand the Causes of Observed Changes in Extremes, and What Are the Projected Future Changes?

#### Major Comments:

*1. The authors provide a good assessment of the scientific understanding of extremes and projected future changes; however, the chapter is too long. Moreover, the discussion on tropical cyclones is excessively lengthy while drought receives less attention than it should. The committee understands that although hurricanes are of considerable interest to a wide variety of audiences, the socio-economic implications of increases in droughts and heat waves are also very serious. Consider, for example, tens of-thousands who died in the European heat wave of summer 2003, or the 739 excess deaths in Chicago during the 1995 heat wave. Given these implications, the committee believes that the SAP 3.3 should expand the discussion of drought, particularly in regards to projections and uncertainties in those projections.*

Response: We agree. The projection of future drought discussion has been lengthened. The text has been modified to cross reference the drought discussion throughout the document. We have also updated the discussion of detection and attribution of 20<sup>th</sup> century changes in drought.

*2a. The authors should coordinate with the authors of Chapter 2 to eliminate the many redundancies with Chapter 3.*

Response: We have done this. Note however that for changes in temperature and rainfall extremes, we find that the attribution section in Chapter 3 is needed and useful to connect to the chapter's future changes section, since both make use of models and attribution evaluates their simulated changes, and since discussion of attribution requires discussion of detection.

*2b. Much of the detection section (3.2) should be moved to Chapter 2 to reduce redundancies. The authors should discuss the differences among detection in observed changes and their implications for projections. If these differences and the causes for them are presented adequately in Chapter 2, the discussion of attribution in Chapter 3 would flow more logically than in the present structure of the document.*

Response: We disagree, since detection and attribution plays a key connecting role between observed changes and their causes and prediction of future changes. However, we have clarified the role of these studies in the introduction to Section 3.2.1 and how they differ from detection of trends as used in Chapter 2 (where the origin of the trend is not readily determined).

*3. To reduce the material on tropical cyclones, the authors should focus on significantly reducing Subsections 3.2.4 and 3.3.3. Subsection 3.2.4 is twice as long as all other parts of Section 3.2 and reads as a text on mechanisms. Much of this material is not appropriate for an assessment. The revised versions of these subsections should outline the basic arguments*

*and summarize the range of informed opinion without providing the fine details of every researcher's arguments, in keeping with a style and level of detail appropriate for a Scientific American readership. The committee provides some specific suggestions in the comments below.*

Response: We have greatly reduced these sections in the revised version.

### **Specific Comments and Suggestions:**

1. *Line 36: The number of key findings (34) dilutes the effectiveness of each. The authors should reduce the number.*

Response: We have substantially rewritten the key findings, reducing their number, making them more succinct, and coordinating their statements with the Executive Summary.

2. *Line 43: What period of time is being referenced in the first two bullets?*

Response: Information on time period has been added to the revised version of the key findings.

3. *Line 48: The paper by Christidis et al. (2007) supports a stronger conclusion for the attribution of GSL reductions in North America and globally over the last 50 years.*

Response: The key finding bullet has been revised, and a likelihood assessment of this particular finding is no longer given.

4. *Line 73: What is meant by tropical cyclone activity? Does activity refer to a combination of intensity, number of storms, and other characteristics, or something more specific?*

Response: In the revised key findings and elsewhere, we have tried to be specific and use terms such as frequency, intensity, etc.

5. *Line 108: Increased frequency of droughts is not supported by the evidence provided in this document. Insert potential before evapotranspiration.*

Response: This key finding has been revised.

6. *Line 128: Is this statement supported by published literature or is it based on expert elicitation? The conventional wisdom in the hurricane community is that shear is a bigger factor in the Atlantic and that as a result, Atlantic hurricanes often form under marginally favorable conditions. Thus, hurricane activity in the Atlantic is sensitive to small changes in environment near the threshold of formation. Other basins may exhibit similar sensitivity, but this sensitivity is geographically larger and less influenced by mid-latitude windshear (apart from the northern Indian Ocean).*

Response: The key findings on tropical cyclones have been substantially revised. In particular, this statement has been deleted from the key findings section. However, a similar statement still appears in the main text, with a recognition of current limits to our understanding: “The high sensitivity of tropical storm and hurricane activity in the Atlantic basin to modest environmental variations suggests the possibility of strong sensitivity of hurricane activity to anthropogenic climate change, though the nature of such changes remains to be determined.”

We do not attempt to distinguish wind shear versus SST versus other environmental factors here. Recent regional downscaling runs suggest that models can successfully capture the interannual and interdecadal dependence of Atlantic hurricane activity on environmental conditions but the runs have not been fully diagnosed yet to address this question of relative importance of shear versus SST versus other factors.

7. *If this is a key finding based on one study that provides an ensemble mean of climate model output, the uncertainty (spread of the ensemble) should be discussed. This statement on the changes in vertical wind shear appears to be based on the ensemble mean model result from one study. Given the likely poleward shift in the mid-latitude jets and the large variability between different models, there may be good reasons for being less confident about this statement.*

Response: This key finding has been deleted. However, the finding is still discussed in the main text. Note that although Vecchi and Soden (2007) is a single study, it is based on a multi-model ensemble comprised of 18 different climate models. As noted in the revised text, the increased vertical shear in the Caribbean region is simulated in about 14 of the 18 models. As to the role of the poleward shift of the mid-latitude jet streams, this is an effect that the models, in principle, should be able to capture. That is, despite the strong global warming signal, a shift large enough and pervasive enough to affect Caribbean wind shear is not seen in the composite model results. Perhaps the 18 AR4 models systematically underestimate the rate of poleward retreat of the mid-latitude jet streams, but this is not, to our knowledge, a finding in the peer-reviewed literature.

8. *Line 198: Mesoscale models (used in a regional climate modeling mode) can be used to help address these issues. The committee suggests explicitly mentioning this in this recommendation.*

Response: All recommendations have been moved to Chapter 4. Regional climate models are now discussed in the recommendations.

9. *In Table 1, it may be incorrect to assert better than even odds that global drought is attributable to anthropogenic forcing considering that this information is (1) based only on one study; (2) based only on one model, (3) the reference isn't available in the published literature; and (4) is based only on the PDSI, which has its limitations.*

Response: Table 1 has been removed, as it was largely repeating text in the main body of the chapter. Discussion of the referenced study (Burke et al., 2006) remains in the chapter.

However, for the detection and attribution section, we now do not include any likelihood statement. Rather, we note that their study supports the conclusion that anthropogenic forcing has influenced the global occurrence of drought. We also note that North American drought patterns are well reproduced in an atmospheric model with observed changes in SST and historical 20<sup>th</sup> century radiative forcing.

*10. Line 301: This reference is not listed. The U.S. Climate Extremes Index (CEI) uses annual mean maximum and minimum temperatures, not daily temperatures, so the observed increase in the U.S. CEI is largely due to an increase in the area of the U.S. with much above normal mean maximum and minimum temperatures and an increase in the area of the U.S. experiencing a much greater than normal proportion of precipitation from heavy one day events.*

Response: The description of the CEI given in the text has been improved and the cited reference added to the reference list. The referenced work is an AMS conference paper; a peer-reviewed paper is, unfortunately, not yet available at this time.

*11. Line 324: This paragraph is a little misleading, as the main external forcing of the observed global precipitation changes is the volcanic forcing. This should be stated explicitly, as elsewhere in this chapter, the main forcing is the increase in anthropogenic greenhouse gas and aerosol concentrations.*

Response: The paragraph has been revised, adding reference to a recent publication on attributable anthropogenic changes in rainfall, and clarifying that the finding the comment refers to was largely due to volcanic forcing.

*12. Line 427: In addition to comparing observations to the ensemble mean of climate model output, these types of comparisons should address ensemble spread and differences in the spatial patterns among ensemble members. All that is required for the observations to be consistent with the model ensemble is that the observed trend and spatial pattern of change lies within the full range of the model ensemble members, but not necessarily close to the ensemble mean. It appears that the authors may be neglecting to take account of the contribution of internal climate variability to the spatial pattern of observed changes, which could be very large regionally.*

Response: The text has been modified to distinguish between behavior by one realization and by ensemble average behavior.

*13. Line 482 - This result only applies globally, but there can be large spatial variations in the decrease in duration or frequency of precipitation events, with some locations having increases.*

Response: The distinction between global behavior and changes in individual regions is now noted.

*14. Line 766: Insert “observational” before “studies.”*

Response: This line has been altered to say, “Some studies of the observational record conclude...”

*15. Line 1024: This section needs more subheadings. Some suggestions include frost, snow, drought, and lake effect snow. In the current format, all of these events are lumped under “precipitation” and “temperature.”*

Response: We agree. We have added a number of new subheadings to section 3.3.

*16. Line 1041: The uncertainty ranges (shaded sigma bounds) are plotted incorrectly in Figure 1. It may be that the authors did not assess the trends in the control runs and eliminate models with significant control run drift before assessing uncertainty ranges in the projections.*

Response: The ranges for all panels have been modified to make them easier to understand by non-scientists.

*17. Line 1043: The statements on effects of soil moisture change are difficult to follow. Consider simplifying them to state that changes in soil moisture and land-surface parameters may differently affect changes in extremes for maximum and minimum temperature.*

Response: We agree. We have modified the text to note the impact of changes of the lower atmospheric boundary on extremes when climate changes.

*18. Again, the comparison of the observed spatial pattern of warming and decrease in frost days in Figure 2 with the ensemble mean pattern from models seems to expect the observed pattern of change to be close to the ensemble mean. However, consistency with the model forced changes plus natural internal variability just requires that the observed pattern of change be well correlated with at least one member of the model ensemble, not necessarily with the ensemble mean. Differences in the spatial patterns are likely due to natural climate variations at regional scales.*

Response: The text has been modified as above to distinguish between behavior by one realization and by ensemble average behavior.

*19. Line 1262: The introductory material is excellent, although here and elsewhere, the authors should reconcile the disconnect between model predictions of reduced tropical cyclone numbers due to increased shear with the subset of the observations that support increased numbers.*

Response: Our attempt at reconciliation of models with observations is in Section 3.3.3.6 - Reconciliation of Future Projections and Past Variations. Basically, the past observations and future projections cannot be reconciled at this point on such important aspects as TC frequency. See the discussion in this section for more detail.



20. Line 1271: *At some point, either here or (preferably) in the discussion of middle latitude cyclones and storm surge (Chapter 2), the authors should quote the amount of historical sea-level rise as measured by instruments like tide gauges. Note the apparent acceleration detected with satellite altimetry, and mention scenarios (e.g., West Antarctic Ice Sheet, Greenland Ice Sheet, reduction in formation of Atlantic Intermediate Water) that could lead to acceleration (Alley et al., 2005; Shepherd and Wingham, 2007).*

Response: The topic of sea level rise is covered in a separate CCSP document, as noted in the text. We have also coordinated with Chapter 2 on this point. We did not modify the text.

21. Line 1318: *This citation is not in the references section. Is it yet published?*

Response: This has been published and the reference added.

22. Lines 1362: *It would be useful to quote the intensity increases in terms of wind speed in addition to (or instead of) pressure. Wind speed is more meaningful to a broader readership and also avoids the issues of ambient environmental pressure and pressure-wind relationships.*

Response: Here we are quoting material from the previous assessment by Henderson-Sellers et al. Remarkably, those authors were not specific about whether the 10-20% increase referred to wind speed or central pressure drop. Chris Landsea has informed one of us (Tom Knutson) offline that this change refers to pressure drop, and that they had so much trouble getting agreement on language for the report that they never “closed the loop” on getting agreement on a wind speed change. So we prefer to leave it in terms of central pressure drop and not try to convert Henderson-Sellers et al. findings to wind speed.

23. Line 1425: *The discussion of modeling lapses into far too much detail near this location. From here through line 1466, it needs to be condensed and simplified.*

Response: The length of this section has been greatly reduced.

24. Line 1467: *The material from here through line 1506 could be reduced to one or two paragraphs. The intensity changes per degree Celsius should be quoted in terms of velocity and pressure fall, or at least consistently in terms of one or the other.*

Response: The material was greatly reduced in length. Intensity results are now quoted in terms of maximum surface wind speeds except where not possible (i.e., in quoting Henderson-Sellers et al., who refer only to central pressures, or in citing Bengtsson et al., 2007, who regrettably use relative vorticity as their intensity metric).

25. Line 1542: *Beginning here, the material presented in Subsection 3.3.3 could be condensed.*

Response: This material was condensed.

26. *Line 1600: The caption for Table 3 is incorrect. The table provides percentages of 20th Century occurrence, not percentage changes.*

Response: The caption has been corrected.

27. *Line 1636: The summary that extends through should be reduced to a few lines and appended to the paragraph beginning on line 1675, which should be retained, because these paragraphs are to a large extent redundant with the discussion that begins on line 1599.*

Response: These suggestions have been followed in the revision.

28. *Line 1642: The material beginning here and through line 1655 should be removed because it addresses non-tropical cyclone convection over the oceans. While this connection may be real, it is too speculative for inclusion here.*

Response: The material has been removed.

29. *Line 1694: The authors probably mean accumulated rainfall at a locality in the storm's path. Radar meteorologists use the term total storm-lifetime precipitation, but for broad readership it implies a Lagrangian (not Eulerian) concept.*

Response: This is indeed what was meant, and the text has been modified accordingly.

30. *Line 1702: A citation to personal communication may be problematic, given the requirements of the prospectus.*

Response: The work has now been published and a full reference (Marchok et al., 2007) is now provided.

31. *Line 1725: Subsection 3.3.3.5 is well-written but the scientific support for it is perhaps the weakest subsections of 3.3.3. This committee suggests that this subsection in particular should be greatly condensed.*

Response: This section was condensed and uncertainty levels were raised in a few areas with little available guidance. A new summary statement was added on storm surge, as it was not mentioned elsewhere in relation to tropical cyclones:

“Storm surge depends on many factors, including storm intensity, size and track, local bathymetry, and the structure of coastal features such as wetlands and river inlets. Unknowns in storm frequency, tracks, size and future changes to coastal features lead to considerable uncertainty in assessing storm surge changes. However, the high confidence of there being at least a small increase of intensity and for sea levels to rise, leads to an assessment that the potential for increased storm surge damage (per hurricane) is likely.”

## Chapter 4

### Recommendations for Improving Our Understanding

#### Major Comments:

1. *The committee generally concurs with the recommendations of the authors but offers several specific comments to sharpen their impact. From a formatting perspective, the committee recommends that the authors consolidate all recommendations scattered among the chapters into Chapter 4.*

Response: Done.

2. *Stylistically, using both bold face and italics for the entire text of a recommendation statement dilutes its impact. Each numbered recommendation statement (e.g., line 115) should begin with a concise, high-impact sentence (in bold) followed by supporting (plain) text.*

Response: Done.

3. *Each of these first sentences should appear verbatim in the Executive Summary.*

Response Done.

4. *The committee noted that although the draft document devotes a considerable amount of space to tropical cyclone issues, there are no recommendations regarding this topic (text added). Notwithstanding the recommendation to reduce the amount of discussion within the chapters, the committee suggests that the authors add a recommendation to support research that seeks to improve our understanding of what governs hurricane intensity. Current theory (e.g., the Maximum Potential Intensity) does not adequately explain the correlation between higher sea-surface temperature and hurricane intensity. The mechanisms that govern intensity must be understood better in order to understand better the potential impacts of a warming world on hurricane intensity.*

Response: Recommendation added.

#### Specific Comments and Suggestions:

1. *Line 11: The sentence beginning on this line should be deleted. It's a parenthetical remark without much meaning that detracts from the point of the paragraph.*

Response: Sentence deleted.

2. *Line 30: Substitute "approximate" for "produce" (also on line 59). Producing a completely homogeneous time series is impossible.*

Response: Done.

3. *Line 57: This is a particularly balanced statement on the ongoing scientific debate and could be included in the Executive Summary.*

Response: After consideration of the recommendation, the statement has not been included in the Executive Summary. We feel that, while the statement fits nicely in its current place in the Chapter 4 recommendation to expand efforts to improve the data record, it does not summarize a major report conclusion that would warrant inclusion in the Executive Summary.

4. *Lines 74: Is there a reference that supports this conclusion (in Chapter 2 or 3 of the draft SAP)?*

Response: Reference included.

5. *Line 77: The authors may mean 1905 instead of 2005.*

Response: Yes, thank you. The correction has been made.

6. *Line 96: Please see comment on line 74.*

Response: Reference included.

7. *Line 109: Please see comment on line 74.*

Response: Reference included.

8. *Line 119: Why is this recommendation applied only to severe thunderstorms and tornadoes? Do observations of other types of events not suffer from inconsistent standards of data collection?*

Response: New recommendation added.

9. *Line 123: The authors could add to line 123 “Recover, digitize” before “homogenize.” In the four recommendations surrounding this line, there is nothing about digitization or recovery of data*

Response: The text has been modified to include “digitize.”

10. *Line 129: Recommendation 3 does not rise to the level of generality as the other recommendations and could be incorporated into recommendation 2, which should emphasize long term analysis of observational data. This combined recommendation should be sufficiently overarching and include a statement to the effect that many improvements are needed in many types of observing systems in order to address the issues set forth in the SAP. In a combined and broader recommendation, extra-tropical Cyclones (ETCs) and extreme*

*wave heights could be mentioned as two types of extremes for which better analysis of long-term data is required. If the authors are raising ETCs and extreme wave heights for a particular reason and they wish for Recommendation 3 to remain separate, then those particular reasons should be further elucidated.*

Response: This was broadened and the ETC phrase deleted.

*11. Line 133: This recommendation should be broadened to include paleo datasets that can be used to infer time series of extreme hydrologic flows (paleogeological datasets) and droughts (tree rings and other paleohydrologic datasets). These datasets could provide information in addition to time series of temperature and precipitation.*

Response: Done, recommendation modified.

*12. Line 140: The sentence beginning on this line should be a dependent clause of the one that immediately follows. Please provide a reference.*

Response: This was revised.

*13. Line 144: The acronym is WGCM, not WGNE.*

Response: Sentence removed.

*14. Line 167: What is "this" resolution, in kilometers?*

Response: Sentence removed.

*15. Line 177: The Hurricane Rainband and Intensity Change Experiment (RAINEX) results show that approximately 2km may be adequate (Houze et al., 2007).*

Response: After consideration, the change has not been made since we feel that this is a matter of some scientific debate and requires further study.

*16. Line 184: In this recommendation, replace "with only minor modifications" with "within the same conceptual framework." Insert "of weather and climate extremes" after predictions. Insert "to enhance spatial and time resolution" before "to recreate." In addition, this recommendation should be consistent with what appears in the document (see text beginning on line 175 of the Executive Summary) and should further recommend use of, or consideration of, regional climate models.*

Response: Sentences removed in revision.

*17. Line 192: A better first sentence could be "We recommend that modeling groups make available data at the highest spatial and temporal resolution from existing simulations of the climate of the 20th and 21st century."*

Response: The sentence has been modified.

*18. Line 240: This example has been explored adequately in previous chapters. The committee suggests deleting the sentence on lines 240-241, and everything from “threshold” in line 244 through the end of the paragraph on line 247.*

Response: The text addressed by this recommendation has been revised.

*19. Line 242: What changes are “these” changes?*

Response: The text addressed by this comment has been removed.

*20. Line 250: Remove “Considering the rapid pace of climate change,” because this sentence is a statement of fact regardless of the current or future pace of climate change.*

Response: Sentence removed.

*21. Line 259: The Summary, while it is a straightforward statement of what needs to be done, needs editing to make it more vigorous and emphatic. Also, need to emphasize the need not only for scientists and users to communicate, but in this case, the weather and climate communities need to learn to talk to one another.*

Response: The Summary has been modified.

*22. Line 329: Figure 1 would be improved if the text in the boxes is replaced with single words, such as “Observations”, “Models”, “Understanding”, “Impacts”, “Adaptation and Mitigation.”*

Response: After consideration of the suggestion, we feel that keeping the text helps in understanding the figure.