December 20, 2007

COMPILATION OF PUBLIC COMMENTS ON CCSP SYNTHESIS AND ASSESSMENT PRODUCT 3.3

"Weather and Climate Extremes in a Changing Climate: Regions of Focus: North America, Hawaii, Caribbean, and U.S. Pacific Islands"

I. Introduction

The 45-day public comment period for CCSP Synthesis and Assessment Product 3.3 ended on October 5, 2007. All comments received during this period were evaluated in accordance with the *Guidelines for Producing CCSP Synthesis and Assessment Products*. This compilation provides a record of the comments received and the Author Team responses.

II. Names of Commenters

Comments were received from one team and from five individuals:

Name: Thomas L. Delworth, Isaac Held, and Gabriel A. Vecchi
Organization: NOAA Geophysical Fluid Dynamics Laboratory
Area of Expertise: Climate

Name: Jim Elsner
Organization: Florida State University
Area of Expertise: Hurricanes

Name: Indur M. Goklany
Organization: Office of Policy Analysis, Department of the Interior
Area of Expertise: Policy Analyst, Science & Technology Policy

Name: Chris Landsea
Organization: NOAA/NWS/National Hurricane Center
Area of Expertise: Tropical Cyclone Research/Historical Hurricane Data

Name: Max Mayfield
Organization: Hurricane Specialist for WPLG-TV
Area of Expertise: Former Director of NOAA's Tropical Prediction Center/National Hurricane Center

Name: Dave Panzer
Organization: Not Given
Area of Expertise: Not Given

Names: Guoyu Ren
Beijing, China

Organization: National Climate Center, Beijing, China
Area of expertise: Climate change, Paleo-climatology

III. Report Section Sorting Structure

The comment sorting routine followed the Report section structure:

Abstract
Preface
Executive Summary
Chapter 1
Chapter 2
Chapter 3
Chapter 4
Appendix A

One comment that was not addressed to a specific report location was labeled General and is included at the end of this compilation.

IV. Response Sorting/Labeling System

For the purpose of responding to the comments, responses were labeled with the commenter's name and the Report section addressed. As an example of the labeling system:

Doe ES-1 would be John Doe's first comment on the Executive Summary
Doe ES-2 would be John Doe's second comment on the Executive Summary
Doe CH1-1 would be John Doe's first comment on Chapter 1

ABSTRACT COMMENTS AND RESPONSES

Delworth, et. al., ABS-1 and ES-1, In the abstract (lines 141-144), Executive Summary (lines 431-433), and elsewhere, the following text (or something close to it) appears: "The balance of evidence suggests that human activity has caused a discernible increase in tropical storm/hurricane and major hurricane frequency in the North Atlantic". This is a crucial topic, but in our opinion there is insufficient scientific basis at this time to make that statement. We have tried to capture our sense of current understanding in the paragraph below. We do not offer the text below as something that should be included in the report; rather, we offer this to articulate our assessment of the state of understanding.

Our assessment:
It is likely that an anthropogenic increase in greenhouse-gases has made a discernible contribution to the increase in tropical Atlantic SSTs over the past century and to the more rapid increase over the past 30 years. These tropical Atlantic SSTs are one of the factors that affect Atlantic tropical cyclone activity. This fact has motivated

observational studies many of which indeed suggest that there has been an anthropogenic greenhouse gas component in the observed changes in hurricane activity over the century. However, models/theory suggest that the changes in storm intensity due to this increase in SST, while positive, may be too small to isolate from the observational record. Also, models have not yet converged on a robust projection for changes in tropical storm numbers due to increasing greenhouse gases. Given this state of models/theory, and given ongoing questions about data quality, it is not appropriate at this time to make a likelihood statement attributing past changes in tropical storm activity to increasing greenhouse gases or other human factors.

Response: We have deleted this sentence in the revised Abstract and revised the text in the Executive Summary to better reflect current understanding.

Goklany, ABS-1, Page 5, Lines 137-138: Replace, for the sake of balance and accuracy, everything following the comma on line 137 with the following: "but not in others. Accordingly, results will vary in direction and magnitude from location to location."

Indur Goklany, Department of the Interior

Response: We have changed the sentence and the current version implies that there are areas unaffected by increases in drought severity.

PREFACE COMMENTS AND RESPONSES

No Comments Received

EXECUTIVE SUMMARY COMMENTS AND RESPONSES

Delworth, et. al., ES-1 and ABS-1, In the abstract (lines 141-144), Executive Summary (lines 431-433), and elsewhere, the following text (or something close to it) appears: "The balance of evidence suggests that human activity has caused a discernible increase in tropical storm/hurricane and major hurricane frequency in the North Atlantic". This is a crucial topic, but in our opinion there is insufficient scientific basis at this time to make that statement. We have tried to capture our sense of current understanding in the paragraph below. We do not offer the text below as something that should be included in the report; rather, we offer this to articulate our assessment of the state of understanding.

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factors that affect Atlantic tropical cyclone activity. This fact has motivated observational studies many of which indeed suggest that there has been an anthropogenic greenhouse gas component in the observed changes in hurricane activity over the century. However, models/theory suggest that the changes in storm intensity due to this increase in SST, while positive, may be too small to isolate from the observational record. Also, models have not yet converged on a robust projection for changes in tropical storm numbers due to increasing greenhouse gases. Given this state of models/theory, and given ongoing questions about data quality, it is not appropriate at this time to make a likelihood statement attributing past changes in tropical storm activity to increasing greenhouse gases or other human factors.

Response: We have deleted this sentence in the revised Abstract and revised the text in the Executive Summary to better reflect current understanding.

Goklany, ES-1, all pages**:** The Executive Summary for the most part seems to view changes in extreme events in a relatively short term context. There is no effort in this Summary to put extreme events into their long term context. This is important not only because this report is concerned with climate change, which necessarily requires examination of long term data, but it deals with extreme events, which should mean looking at even longer records because of the relative rarity of such events. The following are a number of recommendations to remedy this oversight. First, Figure ES.2 goes back only as far as 1980. Yet data on annual property losses from hurricanes and floods are available back to 1900 and 1903, respectively. Providing graphs for those categories of events would be more useful and give the reader a wider perspective (literally). Second, incorporate Figure 2.7 (and, possibly, 2.6) into the Executive Summary to put current droughts into their long term context because drought and its consequences have significant interest for policy makers and the public at large. Similarly, in Figure ES.3 the observational data seems to commence about 1950 but such data should be available at least from 1895 onward if Figures 2.3 and 2.6 are to be given credence. It would, in fact, be more useful to provide Figure 2.3(a), which commences in 1895 and superpose the various model results on that. This would have several advantages. It would give readers a better idea of how well these models reproduce (or not) past heat wave indices, and provide a better indication whether these models have adequately accounted for natural variability. Model results would be more compelling if such a composite figure shows they can reproduce the indices from earlier times, e.g., the 1930s, when greenhouse gas forcing was lower. Figure 2.3(a) also provides very useful context to readers regarding present-day potential for heat waves compared to what it has been in the relatively recent past, i.e., the 1930s. Fourth, there should be a brief discussion in the Executive Summary of the paleotempestology results in Section 2.2.3.1.5 so that current trends can be viewed in their long term context. Fifth, because of impacts of floods on the public at large, there should be some discussion of the paleo record with respect to the frequency and magnitude of floods, and how they compare with present-day events (including events within the instrumental record). I note that the Executive Summary and the assessment discuss precipitation in quite some detail, but that's only one factor contributing to floods (and droughts), and is less significant socioeconomically than floods (and droughts). Finally, there should be a discussion regarding whether studies have been attempted using models to reproduce the US drought record for the 20th century and earlier periods, and what have been the results of such efforts or, if not, then Recommendation 4.5 (on page 21) should be expanded to indicate that such intercomparisons between models and long term data should be a top research priority. Such analyses would help shed light on whether models reproduce (or not) past spatial and temporal patterns of droughts in the US. In addition, to providing a better indication whether these models can adequately account for natural variability, they will also shed light on how much credence resource managers can give to model-based projections of future drought events.

Indur Goklany, Department of the Interior

Response: We appreciate the comment and the desire for additional figures and detail in the Executive Summary, however including all the additional material suggested by this review would go well-beyond the appropriate length for an executive summary. CCSP guidance in the preparation of the Executive Summary indicates that the ES should be brief and limited to the key issues addressed by the product, the background and context of the issues, and the major conclusions. All of the suggested material is included and discussed in the respective chapters referenced.

Goklany, ES-2, Pages All: In order to provide context, the Executive Summary should note, first, that deaths due to extreme weather events typically contribute to less than 0.06% of all-cause mortality in the United States (based on 1,275 deaths annually from 1979-2002) (Goklany 2006). If one adds to this the 1,525 fatalities estimated for the 2005 hurricane season (Blake et al. 2007), this increases to 0.13%. Second, current mortality and mortality rates due to extreme temperatures, tornados, lightning, floods and hurricanes are below their peak levels of a few decades ago. The declines in annual mortality for the last four categories range from 62 to 81 percent, while mortality rates declined 75 to 95 percent. Regardless of whether extreme weather has indeed become more extreme (for whatever reason), global and U.S. declines in mortality and mortality rates suggest that societies' collective adaptive capacities are increasing, perhaps owing to a variety of interrelated factors — greater wealth, increases in technological options, and greater access to and availability of human and social capital — although luck may have played a role. Equally important, mortality due to extreme weather events has declined despite an increase in all-cause mortality suggesting that humanity is adapting better to extreme events than to other causes of mortality (Goklany 2006). Third, US losses in 2005 from weather related events amounted to less than 0.4% of cumulative wealth. [This assumes losses of $120 billion based on Blake et al. (2007) and NOAA (2007). Wealth is estimated at $41.6 trillion according to BEA (2007). All figures are in current dollars.]

References:

BEA. 2007. *Fixed Asset Table: Table 1.1. Current-Cost Net Stock of Fixed Assets and Consumer Durable Goods.* Available at http://www.bea.gov/bea/dn/FA2004/TableView.asp?SelectedTable=16&FirstYear=2001&LastYear=2006&Freq=Year

Blake, E.S.; Rappaport, E.N.; Landsea, C.W.; and Miami NHC 2007. *The Deadliest, Costliest, and Most Intense United States Hurricanes from 1851 to 2006 (and other Frequently Requested Hurricane Facts).*

Goklany, I.M. 2006. Death and Death Rates Due to Extreme Weather Events: Global and U.S. Trends, 1900-2004, Höppe, P., and R. Pielke, Jr., eds., *Workshop on Climate Change and Disaster Losses: Understanding and Attributing Trends and Projections*.Hohenkammer, Germany, May 25-26, 2006, available at http://sciencepolicy.colorado.edu/sparc/research/projects/extreme_events/munich_workshop/workshop_report.html

NOAA. 2007. *2005 Annual Summaries*. Available at http://www.ncdc.noaa.gov/oa/climate/sd/annsum2005.pdf

Indur Goklany, Department of the Interior

Response: The value of a human life is an ethical issue and to assess this is beyond the scope of this report. The Report acknowledges the value of human adaptation and this is indeed a factor related to the human and economic impact of weather and climate extremes.

Goklany, ES-3, Page 12, Lines 279: Add at the end of this sentence the following: "Accordingly, long term trends in loss of life and property provide indications regarding whether society as a whole is becoming more or less resilient or vulnerable to extreme events."

Indur Goklany, Department of the Interior

Response: As described in the Preface the focus of this report is on weather and climate extremes and we acknowledge their impact varies depending on societal and environmental factors.

Goklany, ES-4, Page 12, Lines 283-284: Modify the sentence on these lines to note that some events may occur more frequently, while others may occur more sporadically, e.g., cold snaps and, in some areas, perhaps fewer droughts.

Indur Goklany, Department of the Interior

Response: Agree, we have modified the sentence to reflect this fact.

Goklany, ES-5, Page 12**, Lines** 286-288: It is not a given that back-to-back events will necessarily lead to larger impacts than if the two events were spread out in time. You can only level a standing structure once, for example.

Indur Goklany, Department of the Interior

Response: Agree and sentence is modified to reflect this.

Goklany, ES-6, Page 13, Lines 294-297: See the second set of comments above. What is the source of this data? Have these data been adjusted to account for inhomogeneities due to various factors. Note that GISTEMP, for example, has revised its list of hottest years, which now indicates that the 1990s and 2000s are not significantly warmer, if at all, than the 1930s.

Indur Goklany, Department of the Interior

Response: The data is not based on GISTEMP, but rather NOAA data is used as described in the response above (see www.ncdc.noaa.gov/oa/climate/research/ushcn).

Goklany, ES-7, Page 14, Lines 334-337: What is the experience from the 1930s and 1940s and other periods during which it seems to have been comparably warm in the Arctic region as it is currently. See, e.g., Polyakov et al. (2003), Chylek et al. (2004), Karlen (2005), Soon (2005) and Vinther et al. (2006).

References:

Polyakov, I.V., Bekryaev, R.V., Alekseev, G.V., Bhatt, U.S., Colony, R.L., Johnson, M.A., Maskshtas, A.P. and Walsh, D. 2003. Variability and trends of air temperature and pressure in the maritime Arctic, 1875-2000. Journal of Climate 16: 2067-2077.

Chylek, P., Box, J.E. and Lesins, G. 2004. Global warming and the Greenland ice sheet. Climatic Change 63: 201-221.

Karlen, W. 2005. Recent global warming: An artifact of a too-short temperature record? Ambio 34: 263-264.

Soon, W. W.-H. 2005. Variable solar irradiance as a plausible agent for multidecadal variations in the Arctic-wide surface air temperature record of the past 130 years. Geophysical Research Letters 32 L16712, doi:10.1029/2005GL023429.

Vinther, B.M., Andersen, K.K., Jones, P.D., Briffa, K.R. and Cappelen, J. 2006. Extending Greenland temperature records into the late eighteenth century. Journal of Geophysical Research 111: 10.1029/2005JD006810.

Indur Goklany, Department of the Interior

Response: The sentence in question is about future climate change, not 20th Century climate.

Goklany, ES-8, Page 17, Lines 389-400: It should be noted that the effects of drought on crops and vegetation in general as well as on species (including humans) that rely on such biomass for food energy (and other ecosystem services), would be tempered by the increase in water use efficiency in plants due to higher CO2 concentrations that will necessarily accompany warming.

Indur Goklany, Department of the Interior

Response: This report is not about impacts and the carbon cycle and related biogeochemical processes are discussed in CCSP 2.2.

Goklany, ES-9, Pages 17-19, Lines 404-439: This subsection should be modified to integrate information from paleotempestology studies discussed in Section 2.2.3.1.5 so that current trends can be viewed in their long term context. It is poor scientific methodology to ignore paleoclimatic information. Specifically, this should address whether current frequencies and intensities of storms are unusual when viewed in the context of long term observational data based on paleo studies. With respect to attribution (lines 428-433), the Executive Summary should note whether the precise methods employed in the attribution studies were tested against the results of paleo studies and, if so, how well did these methods reproduce the spatial and temporal patterns of storms (and the intensities) suggested by the paleo studies. This would give us an indication regarding how well the attribution methods incorporate the sources of natural variability. On the other hand, if the attribution studies didn't undertake such studies, the Executive Summary should address the level of confidence that can be ascribed to their ability to model natural variability in the absence of such studies.

Indur Goklany, Department of the Interior

Response: Uncertainties about hurricanes frequency and intensity in the 19^{th} century are discussed in Chapter 3 and mentioned in the ES and this affects our confidence in trends. Paleoclimate data is not mature enough to make any statements about trends relative to today's climate.

Goklany, ES-10, Pages 19, Lines 431-433: Append to the end of this sentence the following: "but historical and paleotempestological data do not indicate any increase in US landfalling hurricanes." Without explicitly alluding to "US landfalling hurricanes" some readers may conclude that the sentence as it currently stands also applies to the mainland USA, and readers are owed clarity (and anticipating and avoiding ambiguity is one aspect of that).

Indur Goklany, Department of the Interior

Response: See response above.

Goklany, ES-11, Page 22, Lines 507: There should be a recommendation that any data that has been created, obtained, modified, or processed using public funding shall be made available on request to any one, as well as any methodologies, or procedures including programs and algorithms, and that researchers are encouraged to make this information available on the Internet.

Indur Goklany, Department of the Interior

Response: This policy issue is beyond the scope of this report, but we note that all model output and observed data used in this report are available from standard data archives.

Goklany, ES-12, Page 23, Figure ES.1: Is there any reason to believe that the probability curves will just be shifted to the right in all case? In some cases it could be shifted to the left, e.g., probabilities for cold snaps and, in some areas, droughts may decline.

Indur Goklany, Department of the Interior

Response: These diagrams are for illustrative purpose, but in response to another comment we have added more descriptive information about possible changes in the shape of the distributions.

Goklany, ES-13, Pages 25, Lines 526-532, Figure ES.3: Please see the first two sets of comments above. As noted, because the observational data seems to commence about 1895 (see Figure 2.3 and 2.6), it would be more useful to reconstruct Figure ES.3 by using Figure 2.3(a) for the empirical data and superposing the various model results (extending into the future) on that. This would give readers a better idea of how well these models reproduce (or not) past heat wave indices, and provide a better indication whether these models have adequately accounted for natural variability. Model results would be more compelling if such a composite figure shows they can reproduce the indices from earlier times, e.g., the 1930s, when greenhouse gas forcing was lower. Also, Figure 2.3(a) by itself provides very useful context to readers regarding present-day potential for heat waves compared to what it has been in the relatively recent past, i.e., the 1930s.

Indur Goklany, Department of the Interior

Response: This report covers all of North America and complete data sets are not available at this time prior to 1950. We have noted the heat and droughts of the 1930s in the US elsewhere in the ES.

Goklany, ES-14, Pages 25, Lines 526-532, Figure ES.3: The provenance and quality of some of the historical data used in this document is unclear (e.g., Figures ES.3 and 2.3). Presumably much of the data are from the US Historical Climatology Network (USHCN). I have had an opportunity to view photographs on www.surfacestations.com of a number of sites that are part of the USHCN. They raise a number of troubling questions about data quality. Specifically, the measurements from some sites — one doesn't know how many — could be affected by their proximity to asphalt, parking lots, roadways, trees, other kinds of land cover, heating and air conditioning units in the vicinity of the monitoring station, and so forth. And, of course, there are always problems associated with location changes, new instrumentation, and erratic or non-uniform maintenance of sites and their immediate environments. All of these factors can affect temperature measurements in general and extreme temperatures in particular. As stated by Williams et al. (2005) on CDIAC's USHCN page, "In summary, while the HCN/D stations represent the best long-term climate records available for the contiguous U.S., no station is completely free of changes that could possibly affect its instrumental record; therefore, it is recommended that users make full use of the information contained in the station histories when performing analyses with these data. The data have not been adjusted for station relocations, heat island effects, instrument changes, or time of observation biases. The nature of inhomogeneities arising from such factors depends on a station's climatic regime." This raises the following questions. First, what are the sources of the temperature data used in this assessment and the papers underlying this report? Second, have the temperature (and precipitation) data used in this assessment been quality assured and, where necessary, corrected for problems alluded to above, as they should be unless one discards data from the affected stations? If the data have been so adjusted, the raw data, details about these corrections and/or reasons for discarding specific data, the algorithms employed, and rationale for each adjustment should be made publicly available so that they can be replicated and verified by other parties should they choose to do so to gain confidence in the information contained here (or for whatever reason). This information is essential if one wants robust, defensible and replicable estimates of changes/trends in extreme events. I note that an examination of Kunkel et al. (1999), which Fig. 2.3 is based on, doesn't provide details on any adjustments to station data. [Also, I was unable to locate Peterson et al. (2007), which is referenced in conjunction with Fig. 2.2, so it's not possible for a reader to deduce whether they QA'ed the data, made any adjustments to the data or discarded it as unusable, as appropriate.] On the other hand, if such data were neither adjusted nor discarded but used in the study, that too should be explicitly noted. While on this topic, I note that the authors of this assessment shouldn't assume that if a paper has been published in a peer reviewed journal then it necessarily means that the author(s) undertook such quality control, or that the peer reviewers assured that they did. However, since this a government sponsored assessment, the results of which could drive public policy decisions, the authors of this assessment have to shoulder the burden of verifying not only that the data were quality assured but that any procedures used to adjust or discard any data were appropriate and appropriately implemented. A reader shouldn't have to do detective work to determine the precise methodology/procedures used in the study. That is what the authors of the assessment should be doing, evaluating and reporting upon, among other things. Note that while one expects to see in the Executive Summary only the briefest summary of the general methodology used in quality assuring and, if necessary, adjusting or discarding the underlying data, the body of the assessment itself should contain an outline of the procedures for the interested lay reader, and Internet links should be provided so that one can, if one chooses (a) examine these issues further, and/or (b) recreate similar curves for specific sites/regions.

Indur Goklany, Department of the Interior

Response: The comment is based on inaccurate information. The Williams quote refers to data that has not been corrected for potential time-dependent biases. There is an extensive set of peer-reviewed papers that detail the numerous aspects of potential biases. Many of these issues were addressed in CCSP 1.1. The NOAA NCDC web site provides additional detail on this topic that is beyond the scope of this report (www.ncdc.noaa.gov/oa/climate/research/ushcn).

Goklany, ES-15, Pages 26, Lines 533-537, Figure ES.4: The discussion surrounding Figure ES.4 should explicitly note that it doesn't inspire much confidence in the model results as far as precipitation events go. The observations seem to lie outside the 95% confidence interval for the model results for much of the period for which both model results and observational data have been plotted, and one suspects that much of the correspondence may be due to the fact that the models were trained using a substantial portion of that record.

Indur Goklany, Department of the Interior

Response: The speculation about 20th Century model simulations reflects a lack of understanding of how models are developed and tested. Details are provided in CCSP 3.1.

CHAPTER 1 COMMENTS AND RESPONSES

Goklany, CH1-1, Page 29, Lines 570-571: This finding should be expanded to address whether and to what extent the changes seen so far are within the bounds of natural variability and to what extent they are due to climate change (which is not the same thing as ascribing it to, or being consistent with, climate change).

Indur Goklany, Department of the Interior

Response: While these are important points that need to be addressed, they are not relevant to why extremes matter which is the subject of Chapter 1. Natural variability is addressed in Chapter 2. Attribution is addressed in Chapter 3.

Goklany, CH1-2, Page 29, Lines 574-575: It is not a given that back-to-back events will necessarily lead to larger impacts than if the two events were spread out in time. As previously noted, one can only destroy or immobilize a structure only once (unless its rebuilt).

Indur Goklany, Department of the Interior

Response: A change in the wording in Chapter 1 has been made to make the statement precisely accurate. Also, in the text where this is discussed, Pielke et al. (2007) is cited as the source of this statement in regards to the back-to-back hurricanes in Florida in 2004.

Goklany, CH1-3, Page 32, Lines 617-631: This paragraph is quite garbled because it mixes up US effects with global effects. First, while trends worldwide are of general interest, this assessment should focus on US trends. It should be noted that for the US, with respect to hurricanes, floods and tornados, any upward trends in property losses disappear when losses are calculated as a fraction of total wealth at risk (i.e., adjusted for inflation and wealth) (Downton et al. 2005, Brooks and Doswell 2001; Pielke et al. 2007).

References:

Brooks, H.E.; Doswell III. C.A. 2001: Normalized Damage from Major Tornadoes in the United States: 1890–1999. Weather and Forecasting, 16, 168–176.

Downton , M.W.; Miller, J.Z.B.; Pielke Jr., R.A. 2005. Reanalysis of U.S. National Weather Service Flood Loss Database. Natural Hazards Review, February 2005: 13-22.

Pielke, Jr., R.A., et al. 2007. Normalized Hurricane Damages in the United States: 1900-2005. Natural hazards Review (accepted).

Indur Goklany, Department of the Interior

Response: The domain of this document is North America, defined to include Canada, the U.S., Mexico, U.S Pacific Islands, and the Caribbean islands, which is why references for the world are relevant in addition to U.S.-specific comments. Additional wording has been added to make some of the points clearer. The references listed by the reviewer have been added to the discussion.

Goklany, CH1-4, Page 39 Lines 794: Insert the following at the end of this sentence: "Nevertheless, mortality and mortality rates from extreme weather and climatic events have declined substantially in the past few decades both in the US and globally (Goklany 2006). On the other hand, property losses have kept pace with the growth in wealth in the US (Downton et al. 2005, Brooks and Doswell 2001; Pielke et al. 2007)." References have been provided in the foregoing.

Indur Goklany, Department of the Interior

Response: Earlier in Chapter 1, we have made points about the influences of adaptation to past weather events citing other references as well as all the references listed in this comment with the one exception being Goklany (2006), which is not peer-reviewed. Text has been added that addresses the question of normalization of damage costs in response to the comment. Regarding the reviewer's assertion that mortality rates are decreasing, the *Annual Disasters Statistical Review, Numbers and Trends 2006,* available from:
http://www.em-dat.net/documents/Annual%20Disaster%20Statistical%20Review%202006.pdf
provides statistics that indicate the opposite trend as that ascribed in the reviewer comment to Goklany (2006). The above-mentioned work is based on the Emergency Events Database (EM-DAT) created by WHO and the government of Belgium, which was also used by the reviewer.

Goklany, CH1-5, Pages 41-42, Lines 827-846: There should be a discussion of whether it is known that the changes noted are necessarily detrimental and the extent to which the changes are due to temperature, precipitation or carbon dioxide concentrations.

Indur Goklany, Department of the Interior

Response: The studies cited on lines 827-846, as mentioned in comment CH 1-5, explicitly statistically relate observed changes in biological systems to temperature trends as well as extreme events as stated in the text. We did not address the impact of increasing carbon dioxide concentrations in Chapter 1 because (A) gradually increasing carbon dioxide is not an extreme weather or climate event, (B) the literature does not support carbon dioxide fertilization as a driver of observed changes in species' distributions or phenologies and (C) assessment of impacts of climate change, including carbon dioxide, on biodiversity is being addressed in CCSP reports 4-2, 4-3 and 4-4. It is beyond the scope of this Chapter to provide assessments of whether each of the observed changes is detrimental or not.

Goklany, CH1-6, Pages 41-42, Lines 861-863: These lines may need to be modified in light of Franks et al. (2007).

Reference:

Franks, S.J., Sim, S., and Weis, A.E. 2007. Rapid evolution of flowering time by an annual plant in response to a climate fluctuation. *PNAS*. 104 (4): 1278-1282.

Indur Goklany, Department of the Interior

Response: Review of Franks et al. (2007) indicates that it is consistent with other studies cited on the observed evolutionary response to climate change and therefore does not necessitate modification of the text.

Goklany, CH1-7, Pages 41-42, Lines 904: Change "have been shown to break down" to "may break down". Breakdown doesn't automatically follow.

Indur Goklany, Department of the Interior

Response: The text has been modified to better reflect the results of the studies. These were studies of the past. The specific wording change suggested by the reviewer was not incorporated because no projections are made in the cited studies.

Goklany, CH1-8, Pages 41-42, Lines 927-929: It should be noted that the insurance industry isn't a disinterested party in that it has an incentive to overestimate risks and create the perception of greater risk.

Indur Goklany, Department of the Interior

Response: Regardless of the perceived motivation of the insurance industry, what is reported in Chapter 1 is what happened. Therefore, no statement of possible insurance industry motives is appropriate.

Goklany, CH1-9, Pages 57, Lines 1188-1191: An alternative view of the European heatwave is provided in Chase et al. (2006). This should be discussed too.

Reference:

Chase, T. N., K. Wolter, R. A. Pielke Sr., and I. Rasool, 2006. Was the 2003 European summer heat wave unusual in a global context? *Geophysical Research Letters*, 33, L23709, doi:10.1029/2006GL027470.

Indur Goklany, Department of the Interior

Response: The Chase et al. (2006) paper found that "extreme warm anomalies equally, or more, unusual than the 2003 heat wave occur regularly." This is in contrast to the paper we cited, as well as numerous other papers such as Stott et al. 2004, Trigo et al. 2005, Meehl and Tebaldi 2004, Menzel 2005, etc. which find 2003 to be a highly unusual event. The problem with Chase et al.'s analysis is that they used 1000 to 500 mb thickness anomalies as their metric. As pointed out in a comment on Chase et al., using the Chase et al. method but applying it to surface temperatures reveals that the summer of 2003 was indeed a unique record (Connolley, 2007). Mortality depends on surface temperature not the temperature averaged over 1000 mb to 500 mb which is a measure from near the surface up to about 5.5 km. Indeed, Kalkstein et al. (2007) analysis of analog European heat wave events for U.S. cities estimates that a similar magnitude heat wave in New York City would have a heat related mortality of 3,253. Since such high mortality does not occur regularly in the U.S., this analysis also indicates that the European heat wave of 2003 was an unusual event.

Connolley, W.M., 2007: Comment on Chase et al., "Was the 2003 European summer heat wave unusual in a global context?" *Geophysical Research Letters*, submitted.
Kalkstein, L.S., J.S. Greene, D.M. Mills, A.D. Perrin, J.P.Samenow and J.-C. Cohen, 2007: The development of analog European heat waves for U.S. cities to analyze impacts on heat-related mortality. *Bulletin of the American Meteorological Society*, in press.
Meehl, G.A. and C. Tebaldi, 2004: More intense, more frequent and longer lasting heat waves in the 21st Century. *Science*, **305**, 994-997.
Menzel, A., 2005: A 500 year pheno-climatological view on the 2003 heatwave in Europe assessed by grape harvest dates. *Meteorologische Zeitschrift*, **14**, 75-77.
Stott, P.A., D.A. Stone and M.R. Allen, 2004: Human contribution to the European heatwave of 2003. *Nature*, **432**, 610-614.
Trigo, R.M., R. Garcia-Herrara, J. Diaz, I.F. Trigo and M.A. Valente, 2005: How exceptional was the early August 2003 heatwave in France? *Geophys. Res. Lett.*, **32**, L10701, doi:10.1029/2005GRL022410.

Goklany, CH1- 10, Pages 62-64, Lines 1312-1342: These lines should be deleted. This discussion is irrelevant to the US. Diarrhea and dengue are the result of poverty at levels that the US hasn't seen for decades, and is unlikely to see again. They have a major impact in poor countries because the inhabitants of those countries suffer from malnutrition and hunger (again at levels unheard of in the US), lack the almost universal access to safe water and sanitation that exists in the US, lack public health services and the wherewithal to afford medicines (Goklany 2007a). Moreover, the 150,000 estimate for climate change related mortality made by WHO is suspect, to put it mildly. Its authors (McMichael et al. 2004) themselves note in a paper describing their methodology that "climate change occurs against a background of substantial natural climate variability, and its health effects are confounded by simultaneous changes in many other influences on population health….Empirical observation of the health consequences of long-term climate change, followed by formulation, testing and then modification of hypotheses would therefore require long timeseries (probably several decades) of careful monitoring. While this process may accord with the canons of empirical science, it would not provide the timely information needed to inform current policy decisions on GHG emission abatement, so as to offset possible health consequences in the future. Nor would it allow early implementation of policies for adaptation to climate changes." Hence, the estimates were based on scientific short cuts and policy expediency rather than rigorous science. They also employed modeling studies, with quantification based on anecdotal information. In addition, the temperature-disease relationship used to develop the estimate for diarrhea, for example, was based on 6 years worth of data from Lima, Peru, and 20 years of data from Fiji. In addition, the amount of climate change estimated for 2000 was based on the results of a general circulation model at resolution of 3.75 deg longitude and 2.5 deg latitude. The results of such models, which are inexact at best at the global level, tend to greater uncertainty as the resolution gets finer. Moreover, for all the reasons noted above, there is no parallel between the ability of the US and countries such as Peru and Fiji to deal with and respond to such diseases (see also Goklany 2007b).

References:

Goklany, I.M. 2007a. *The Improving State of the World: Why We're Living Longer, Healthier, More Comfortable Lives on a Cleaner Planet* (Cato Institute, Washington, DC, 2007).

Goklany, IM. 2007b. "Integrated Strategies to Reduce Vulnerability and Advance Adaptation, Mitigation, and Sustainable Development." *Mitigation and Adaptation Strategies for Global Change. DOI 10.1007/s11027-007-9098-1.*

McMichael, A., et al. 2004: Global climate change. In: *Comparative Quantification of Health Risks: Global and Regional Burden of Disease due to Selected Major Risk Factors*. World Health Organization, Geneva, pp. 1543-1649.

Indur Goklany, Department of the Interior

Response: There are two parts to this comment.

1. The reviewer stated that it isn't relevant to the U.S.
    a. The domain of interest of this report is more than just the U.S. It is Canada, the U.S., Mexico, U.S. Pacific Islands, and the Caribbean, which includes some very poor countries such as Haiti.
    b. The text cites 7 studies of U.S. cases. Contrary to what the reviewer stated, Dengue Fever is endemic in several cities in Texas (Brunkard et al., 2007). This reference has now been added to the text.
2. The reviewer says the methodology of McMichael et al. (2004) is suspect, based on short cuts and policy expediency, and not on rigorous science. This is an inaccurate interpretation by the reviewer. Interestingly, this reference wasn't even cited by our chapter. However, upon review of the document, we added this reference to the many other references that we use in this section. The McMichael document cites over 100 sources, not just the two pointed out by the reviewer.

Brunkard, J.M., J.L. Robles López, J. Ramirez, E. Cifuentes, S.J. Rothenberg, E.A. Hunsperger, C.G. Moore, R.M. Brussolo, N.A. Villarreal, B.M. Haddad, 2007: Dengue Fever Seroprevalence and Risk Factors, Texas-Mexico Border, 2004. *Emerging Infectious Diseases*, **13**, 1477-1483.

Goklany, CH1-11, Pages <u>89 and 92</u>, Figures <u>1.7 and 1.10(a)</u>: It would be very instructive if these two graphs were commenced in 1895 and were superimposed on each other. It would give a crude indication of the change in the US's adaptive capacity.

Indur Goklany, Department of the Interior

Response: Figure 1.7 already infers an increase in the U.S. adaptive capacity. Extending this information back to 1895 would require additional data that we do not have.

CHAPTER 2 COMMENTS AND RESPONSES

Elsner, CH2-1, I believe the results from our recent paper entitled Climatology models for extreme hurricane winds near the United States, Journal of Climate, v19, 3220-3236, July 2006 should be considered. In particular the graphs shown in Fig 6c & d are relevant to the discussion about hurricane activity and climate change. They clearly show that in warmer years the frequency of the strongest hurricanes (highest return levels) is greater compared with cooler years. The amount of change 6-12% at an 80-year return level is consistent with model projections so this result addresses the attribution issue.

Response: We thank the reviewer for pointing out this oversight and have included reference to this study. In Section 3.3.9.2 on projections of tropical cyclone intensity, we note that:

"The statistical analysis of Jagger and Elsner (2006) provides some support for the notion of more intense storms occurring with higher global temperatures, based on observational analysis. However, it is not yet clear if the empirical relationship they identified is specifically related to anthropogenic influences on global temperature."

New Reference:

Jagger, T. H., and J. B. Elsner: 2006. Climatology Models for Extreme Hurricane Winds near the United States. *J. Climate*, **19** (13), 3220–3236.

Goklany, CH2-1, Pages <u>All</u>: Although Chapter 2 discusses precipitation, its discussion of floods, runoff and streamflow is very cursory. [Precipitation is only one factor that contributes to these other types of events.] Also there is no discussion of long term reconstructions of such events using paleo techniques. As a start, I recommend discussing results from the following list of studies, which is not comprehensive.

Brown, P., Kennett, J.P. and Ingram, B.L. 1999. Marine evidence for episodic Holocene megafloods in North America and the northern Gulf of Mexico. Paleoceanography 14: 498-510.

Fye, F.K., Stahle, D.W. and Cook, E.R. 2003. Paleoclimatic analogs to twentieth-century moisture regimes across the United States. Bulletin of the American Meteorological Society 84: 901-909.

Garbrecht, J.D. and Rossel, F.E. 2002. Decade-scale precipitation increase in Great Plains at end of 20th century. Journal of Hydrologic Engineering 7: 64-75.

Lins, H.F. and Slack, J.R. 1999. Streamflow trends in the United States. Geophysical Research Letters 26: 227-230.

Molnar, P. and Ramirez, J.A. 2001. Recent trends in precipitation and streamflow in the Rio Puerco Basin. Journal of Climate 14: 2317-2328.

Ni, F., Cavazos, T., Hughes, M.K., Comrie, A.C. and Funkhouser, G. 2002. Cool-season precipitation in the southwestern USA since AD 1000: Comparison of linear and

nonlinear techniques for reconstruction. International Journal of Climatology 22: 1645-1662.
Noren, A.J., Bierman, P.R., Steig, E.J., Lini, A. and Southon, J. 2002. Millennial-scale storminess variability in the northeastern Unites States during the Holocene epoch. *Nature* 419: 821-824.
Schimmelmann, A., Lange, C.B. and Meggers, B.J. 2003. Palaeoclimatic and archaeological evidence for a 200-yr recurrence of floods and droughts linking California, Mesoamerica and South America over the past 2000 years. The Holocene 13: 763-778.
Shapley, M.D., Johnson, W.C., Engstrom, D.R. and Osterkamp, W.R. 2005. Late-Holocene flooding and drought in the Northern Great Plains, USA, reconstructed from tree rings, lake sediments and ancient shorelines. The Holocene 15: 29-41.
Wolfe, B.B., Karst-Riddoch, T.L., Vardy, S.R., Falcone, M.D., Hall, R.I. and Edwards, T.W.D. 2005. Impacts of climate and river flooding on the hydro-ecology of a floodplain basin, Peace-Athabasca Delta, Canada since A.D. 1700. Quaternary Research 64: 147-162.
Indur Goklany, Department of the Interior

Response: We added two paragraphs to include current information on trends in high streamflow. The problems here are that (a) we cannot separate for most of the U.S. climatic trends in high streamflow from increases throughout the past century in regional anthropogenic impacts directed to mitigate the peak flow (water management and dam construction) and (b) therefore the results summarized in the currently available studies cover only about 20% of the conterminous US territory.

Goklany, CH2-2, Pages 96, Lines 2149-2153: These two bullets should be merged and repeated in the Executive Summary. The focus should be the US. Also the fact that mega-droughts occurred in the past should also be included in this bullet (from p. 110). I recommend the following language: "Although there are recent regional tendencies toward more severe droughts in the southwestern U.S., for the continental U.S. the most severe droughts in the instrumental record occurred in the 1930s and there is no indication of an overall trend since 1895. However, the mega-drought of the 1500s was more widespread and longer lasting than the 1930s episode."
Indur Goklany, Department of the Interior

Response: The mega-drought of the 1500s is discussed in the main text of Chapter 2. However the main focus of this report is the period of increasing greenhouse gases which roughly coincides with the instrumental period starting in the late 1800s. Furthermore, the focus area of the report is North America, not just the U.S.; therefore we decline the suggested modifications.

Goklany, CH2-3, Pages 111-114, Lines 2483-2549: One of the major reasons for being concerned about drought is its impacts on plant growth (and on the various species, including human beings, that rely on plant matter for sustenance and ecosystem services). From this point of view soil moisture is a critical indicator. However, since CO2

concentrations will affect water use efficiency for plants and soil moisture, BOX 2.1 should also indicate the extent to which, and how well, models used to calculate the various drought indices for future conditions can and have accounted for changes in CO2 concentrations and its effects on soil moisture.
Indur Goklany, Department of the Interior

Response: The standard drought indices used for historical analyses and also employed in this report for future projections do not include this effect. The magnitude of this effect is a subject of active research. We have included references to some of the field studies addressing this effect. They tend to show an identifiable but limited (second-order) effect.

Goklany, CH2-4, Pages 116, Lines 2589-2597: Characterizing daily rainfall in excess of 2 inches as heavy may make sense by the standards of the Sahara Desert (or Death Valley) but for most other places it would not qualify as such. There should be a discussion of whether, and under what circumstances, this 2-inch criterion is meaningful from a socioeconomic perspective. I would recommend using a cutoff based on empirical information regarding the likelihood of severe flooding, appropriately defined.
Indur Goklany, Department of the Interior

Response: The study by Karl and Knight (1998) was the first nationwide assessment of heavy precipitation across the entire United States. They indeed used a 2-inch threshold to characterize heavy daily precipitation events over the nation. However, they could not select higher thresholds at that time because their analysis was based on a small number of century long daily time series over the country (~200). But follow-up studies (cited in the Report) have used the time series of several thousand stations and upper percentiles (up to 0.1% of daily rain events) as characteristics of precipitation extremes (Groisman et al. 2001, 2004, 2005; Kunkel et al. 2005). A large body of work on data collection, rescue, and quality control preceded this new generation of studies in the United States. In the report, we nevertheless believe that it is worthwhile to present the first study on extreme precipitation change across North America and to show how it compares with later studies.

Goklany, CH2-5, Pages 131, Lines 2925: Insert "by some" after "thought".
Indur Goklany, Department of the Interior

Response: This sentence has been modified due to other review comments and this comment is no longer pertinent.

Goklany, CH2-6, Pages 135, Lines 3013-3015: Insert at the end of this sentence, the following: "but on the other hand, the latter index is much more relevant to socioeconomic impact on the US. Notably, neither fatalities, fatality rates nor property damage from hurricanes show an upward trend if the latter are corrected for inflation and

wealth at risk (Goklany 2006; Pielke et al. 2007).” References have been provided previously.
Indur Goklany, Department of the Interior

Response: The Goklany reference is not a peer-reviewed paper. We do not have access to the Pielke et al reference. We are leaving the text as is. Our concern is with changes to the physical climate change. We have declined to make changes in the text.

Goklany, CH2-7, Pages 136, Lines 3034-3038: These two sentences should be included in the Executive Summary.
Indur Goklany, Department of the Interior

Response: The data uncertainty issue is addressed with the following two statements in the Executive Summary:

"…data uncertainty is larger in the early part of the record compared to the satellite era beginning in 1965." and "There is increasing uncertainty in the data as one proceeds back in time."

Landsea, CH2-1, The conclusion of "upward trends in …the frequency of North Atlantic tropical cyclones (hurricanes)…are notable changes in the North American climate record" is strongly disputed. Specifically, the conclusions of (1) "increases [in both frequency and Power Dissipation Index - PDI] …are likely substantial since the 1950s and 60s, in association with warming Atlantic sea surface temperatures", (2) "there has been an increase in tropical cyclone frequency in the North Atlantic over the past 100 years", and (3) "the frequency of major hurricanes has increased coincident with overall tropical cyclone numbers" are not agreed to. Details of these points are given below.

General Response: These points of disagreement with the reviewer are addressed in the detailed comment section below.

Landsea, CH2-2, Page 134 and 135: The whole basin PDI presented here from Emanuel (2005) is a primary reason used to justify conclusion (1) above. Given that some tropical cyclones were missed completely and the lifetime of many in the eastern Atlantic would be only partially sampled before geostationary satellite coverage in 1966 (Landsea 2005, 2007), the duration of pre-1970s tropical cyclones will be biased low. Indeed, the chapter says that earlier on page 132: "estimates of the duration of storms are considered to be less reliable prior to the 1970's due particularly to a lack of good information on their time of genesis." Aircraft reconnaissance in the late 1940s to the late 1960s (and today) typically only monitors about half of the Atlantic basin. Here is a direct quote from Landsea (2005): "It is also likely that values of PDI from the 1940s to the mid-1960s are substantially undercounted owing to the lack of routine aircraft reconnaissance and geostationary satellite monitoring of tropical cyclones far from land." (More is discussed regarding intensity monitoring back in time below.) This low bias in duration and intensity before the advent of geostationary satellite could easily be why 1995 to today has larger PDI values than the late 1940s to the late 1960s.

It is recommended that the conclusion in (1) be changed to: "PDI has substantially increased in the Atlantic since 1995 compared with the 1970s to the mid 1990s. PDI values are also higher now than the late 1940s to the late 1960s, but a direct comparison is problematic because of the missed portion of tropical cyclones due to no satellite and aircraft monitoring for cyclones far from land. Thus it is unknown whether a long-term trend exists in Atlantic PDI."

Response: The conclusion of a likely substantial increase in PDI means that the probability that the statement is correct is at least 2/3, so we are not saying that we are certain of the conclusion. Nonetheless, in considering the published evidence to date, the authors of the report conclude that a substantial increase in these metrics is likely since the 1950s and 60s, so it appears that we disagree with the reviewer on this point.

Landsea, CH2-3, Page 135: The criticism that the U.S. landfalling record only contains 1% of the whole basin PDI record and thus is too noisy is a red-herring. The U.S. landfalling record is able to strongly observe El Nino Southern Oscillation-forced variations (Bove et al. 1998) as well as variability due to the Atlantic multidecadal oscillation (AMO) (Landsea et al. 1999, Goldenberg et al. 2001). The strength of the U.S. landfalling record is due to it being less prone to undersampling of frequency and intensity that affects the whole basin record before satellite coverage began. Certainly, if there has been a large increase in overall tropical cyclone numbers and frequency in major hurricanes during the last 100 year, this would have been manifest in the U.S. record. To claim otherwise denies the fact that other interannual and multidecadal variations are easily observed in the U.S. record.

Response: There is no a priori reason to believe that the landfalling cyclone record is representative of the entire basin, and no evidence has been provided in support of this by Landsea (2007). Several studies have criticized the use of landfall as an overall basin indicator (Holland 2007, Mann et al 2007, Sabbatelli et al 2007) and others find substantially fewer missing cyclones prior to 1960 (Chang and Guo 2007, Vecchi and Knutson 2007, Sabbatelli et al 2007); we have been very much guided by these in our assessment. We also note that the Goldenberg et al (2001) study certainly saw a trend and noted that this was different to the multi-decadal changes….e.g. to quote: "the mean number of major hurricanes and mean NTC for 1995-2000 are the highest of any consecutive 6 years in the 1944-2000 record. While this recent period spans only 6 years, it clearly belongs to a different low-frequency climate regime than the previous 24 years (1971-1994)."

The ENSO signal in landfall power (which is correlated with damage) is very weak indeed, to the point where it stresses most definitions of statistical significance. By randomly drawing from either HURDAT or large, statistically stationary synthetic storm sets to create random Atlantic times series, it can be shown that it takes many hundreds of years to detect typical climate signals in landfall records. The reviewer also makes a self-contradictory statement: The anthropogenic signal in the Atlantic is really just a re-interpretation of what had been called the AMO, plus a linear trend; so if the latter is detected, than the former must be.

To summarize, the landfalling record clearly has lower signal to noise characteristics than the basin wide record owing to the smaller number of landfalling storms relative to total storms. If storm tracks changed over time, which there is evidence of from Kossin and Vimont's work on the AMM, then landfall becomes a poorer surrogate. So while the reviewer's opinion could possibly be correct, he/she overlooks viable alternative scenarios, and overstates the likelihood of his/her view (i.e., through use of the term "Certainly").

Landsea, CH2-4, Pages 138-140: The statement "Landsea (2007) has used the fraction of storm 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" is incorrect. Instead, Landsea (2007) found that the number was 2.2 storms per year. The additional roughly one storm per year was due to brand new (since ~2002) tools and techniques that have allowed the National Hurricane Center to identify tropical cyclones that previously would have been considered weak extratropical cyclones. Indeed, the assessment states this just a couple lines earlier: "there have been steady improvements in techniques and instrumentation, which may also introduce some spurious trends."

General Response: We have corrected the statement. We have also noted that Landsea's (2007) statement on there being one additional storm per year missing due to improved "tools and techniques" is presented without corroborative evidence. Such evidence will be required before this can be included in assessments such as here.

Moreover, the assessment goes on to state that "[Landsea's assumption] that the fraction of all storms that strike land in the real world has been relatively constant over time, which has been shown to be incorrect by Holland (2007)." Holland confirmed that the percentage has been stable for the last 40 years and was relatively stable at a higher rate of landfalling percentage for about 60 years before that. Holland did show that the percentage was lower back in the second half of the 1800s, but that analysis neglects the fact that much of the U.S. Gulf Coast (including nearly the entire Florida peninsula), Mexico, Central America, and portions of the Caribbean islands were extremely sparsely populated. For example, Miami was not founded until 1896. This is why Landsea (2007) began his analysis in 1900, which is consistent with earlier estimates of accurate storm counts along populated coastal areas based upon U.S. Census reports (Landsea et al. 2004). The assumption that all landfalling systems would show up in HURDAT before 1900 is incorrect and one would expect that the landfalling ratio would decrease simply because there would be less records of landfalling systems with less people on the coast. There is concern about the statement that a "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". This analysis does not discount the Landsea (2007). It only reconfirms that in the last few decades that cyclones that formed in the eastern Atlantic have been better monitored and are now being included into the dataset, while previously they would have been left out.

Response: The assumption of a constancy of landfalling proportion is not intuitively obvious, was not justified at all by Landsea (2007), does not concur with known changes in observing system practice, and has been strongly disputed by Holland (2007), Mann et al (2007) and Sabbatelli et al (2007). Given the lack of any justification of this assumption and the countering published information (including relevant information by Chang and Guo 2007, Sabbatelli et al 200) and Vecchi and Knutson 2007), we are unable to accept landfall proportion as an indicator of basin-wide activity. In particular, the reviewer is incorrect in the last two sentences: Holland (2007) showed that the increase in eastern Atlantic formations was accompanied by a "decrease" in the relatively well-observed western Caribbean, which lends considerable weight to there being a real increase in eastern Atlantic formations. This also provides one explanation for the lack of increased in landfalls (most cyclones forming in the western Caribbean and Gulf of Mexico make landfall, but the increasing number of eastern Atlantic formations often do not make landfall).

Finally, and most importantly, the two otherwise well-done studies of Chang and Guo (2007) and Vecchi and Knutson (2007) have not taken into account a crucial point: the COADS ship data that they have based their studies on have only just recently been included into a portion of the reanalysis of the hurricane database. COADS was not utilized for the reassessment of HURDAT for the period of 1851 to 1910. The ship database has now been incorporated into the reanalyses that are being completed for 1911 to 1920 (Landsea et al. 2007). The crucial point is that the COADS data and other sources have allowed the identification of 13 brand new tropical cyclones in a decade. (There was also one tropical cyclone removed from HURDAT because of the lack of meeting the criterion of a tropical cyclone by today's standards.) The addition of an additional 1.2 tropical cyclones per year in the early 20th Century must be added to the numbers estimated to be missing by Chang/Guo and Vecchi/Knutson because the studies are premised on all tropical cyclones that could be monitored by COADS are currently in HURDAT. Clearly, this is not the case. Chang and Guo also had an overly conservative assumption that only one observation of tropical storm force winds was needed to be included into HURDAT. As documented by Landsea et al. (2007), two independent observations are required for such inclusion. As shown by Vecchi and Knutson, two versus one observation has a small, but meaningful alteration on the estimate of missing tropical cyclones.

Response: While we agree that this is an evolving area, we have to place weight on published studies of the potential errors in the database, rather than un-reviewed and unknown sources. We consider that the current state of the science indicates 1-3 missing storms before 1900 decreasing to near zero by 1960 and this is consistent with the supposed additional cyclones added to the HURDAT in more recent times. We look forward to these data additions being fully justified and included in the data set that is made available for further research.

Taking into account tropical cyclones that will be added into HURDAT by fully considering COADS must be considered. The trend from 1900 to 2006 will be substantially reduced and the trend from 1878 onward will essentially be flat. While the choice of 1900 for a starting point for estimating missed tropical cyclone numbers in Landsea (2007) was dictated by the rough beginning date of enough coastal settlements and population, the use of this starting date in Chang and Guo (2007) and its emphasis in this assessment does not appear justifiable. As pointed out in the assessment (page 138) and in Vecchi and Knutson (2007), there are no large observing system advances that occurred around 1900. Focusing upon a starting point at 1878 is much more justifiable for two reasons: a) this was the year that the United States set up coastal monitoring stations along its coastlines and Cuba instituted a monitoring network and forecasting system at about the same time (Vecchi and Knutson 2007, Landsea et al. 2004); and b) the late 1800s were a multidecadal active period that corresponds with an AMO warm phase (Landsea et al. 1999). As pointed out in Landsea (2007), comparing the early 20th Century versus the last 12 years is problematic as it goes from a multidecadal quiet phase to a busy phase – an apples versus oranges comparison. To avoid this, trend lines from 1900 to 1994 (quiet to quiet) or 1878 to 2006 (busy to busy) are appropriate. Doing the latter – as shown in Vecchi and Knutson – gives no significant trend in whole basin tropical cyclone activity, even before taking into account that the HURDAT numbers would go up substantially for the late 1800s and early 1900s if COADS were to be incorporated.

Response: We consider that these uncertainties and considerations have all been adequately covered in the assessment discussion sections.

Thus conclusion (2) needs to be adjusted to the following: “Atlantic basin tropical cyclone frequency shows no significant trend over the last 125 years after accounting for the likely number of “missed” cyclones due to improved monitoring. This is consistent with no trends in landfalling records for the U.S. along, which are relatively complete back to 1900.”

General Response: We report both the significant results from 1900 and the nonsignificant results from 1878, This alerts the reader to the non-robustness of the significant linear trend result to addition of earlier years (with apparently heavy storm activity), We prefer not to abandon the result from 1900, as there is likely increasing uncertainty in the data as one goes further back in time, implying that the significant trend since 1900 deserves separate mention, as do the trend results from 1878.

Landsea, CH2-5, Pages 140-141: “Atlantic basin total hurricane counts, major hurricane counts, and U.S. landfalling hurricane counts as recorded in the HURDAT database for the period 1851-2006 are shown in Fig. 2.17. These have not been adjusted for missing storms, as there was likely less of a tendency to miss both hurricanes and major hurricanes in earlier years compared to tropical storms, largely because of their intensity and damage potential." This analysis and statement are not defensible. (They are not cited as well. Work not appearing in papers that have not been peer-reviewed is not to be allowed.) The inner core of hurricanes and major hurricanes is mesoscale. For example, Dean's radius of maximum wind (where the major hurricane winds reside) when it was over the Caribbean Sea was a small, but not atypical, 10 nmi. It is likely that hurricanes and major hurricanes are more likely to be detected and included into HURDAT, because of their larger gale force wind radii compared with tropical storms. However, many times they would not likely be recorded AS hurricanes and major hurricanes, but instead as a tropical storm. (Why would a ship that encounters gale force winds continue to head to the center of the cyclone? Nearly all ship captains would do their best to steer away from the worst part of a tropical cyclone, as even in the 1800s – see the "Law of the Storms" by Piddington – ship captains knew that when the barometer began to drop that the center of a cyclone was off to the right when facing the wind.) As an example, see Figures 3 and 4 from Landsea et al. (2004) about how a modern major hurricane would be observed without the monitoring of aircraft reconnaissance and/or satellite imagery – a major hurricane would instead appear to be a weak tropical storm. Unless a major hurricane struck a populated coast or had a ship go through the eye would one know that a major hurricane occurred in the pre-aircraft and satellite era. Please note the large low biases in intensity that are estimated to occur in the late 1800s and early 1900s for over open ocean tropical cyclones (10-15 kt too low – Landsea et al, 2004).

General Response: The basis for Fig. 2.17 has previously been published in Holland and Webster (2007) and it is simply a plot of the HURDAT data. The statistical analysis was undertaken specifically for this assessment and has been independently reviewed; it also concurs with the published assessment of Mann et al (2007). As stated earlier, we are very much influenced by published studies in this assessment; qualitative statements such as given here are not acceptable. We note that the statement "large low biases in intensity that are estimated to occur in the late 1800s and early 1900s for over open ocean tropical cyclones (10-15 kt too low" was made in Landsea et al (2004) as an expert assessment without corroborating evidence. It is not consistent with even a simple plot of annual mean intensities, which have not shown any trend or long-period variability of any kind whatsoever over the past 150 years. Further, as shown by Holland and Webster (2007), the proportion of hurricanes was artificially high prior to 1900 but has remained very stable since then. The proportion of major hurricanes was low for the same period, indicating support for the lack of ability to analyze the most intense core winds, but this has been stable since 1900 (apart from a distinct multi-decadal oscillation). Taken together, the available evidence indicates a tendency to miss weaker storms rather than hurricanes prior to 1900.

Concerning our conclusions regarding increases in hurricane and major hurricane counts since 1900, we assume that tropical storms provide a surrogate for percentage changes in hurricane and major hurricane counts since 1900. While there may be missing storms or undercounts in all categories, the significant increase in tropical storm counts since 1900 (discussed earlier), which is robust to adjustment for missing storms based on ship track density (Vecchi and Knutson 2007), provides some support for a significant increase in the hurricanes and major hurricanes as well, despite data problems with the basin-wide counts for these metrics. Notice also our use the qualified term “likely”. We discuss the non-significant trend results for hurricane counts from the mid to late 1800s, and in the revised text we note that major hurricane counts from the 1800s are particularly problematic. Overall, our analysis of Fig. 2-17 and the above results is a reasonable and balanced view of the available evidence.

Such concerns about underestimating the true intensity in existing tropical cyclones before aircraft/satellite monitoring is why no one had calculated the trends in all basin hurricane and major hurricane frequency previously. (See discussion in Landsea 1993 and Landsea et al. 1999. Also a very relevant paper regarding the difficulty of monitoring tropical cyclone number and intensity back in time is that of Holland 1981.) Figure 2-17 must be removed and conclusions regarding the number of hurricanes and major hurricanes over the last 100 years discarded.

General Response: As noted earlier, and shown by Holland and Webster (2007) one remarkable feature of the North Atlantic data base is the constancy of hurricane and major hurricane proportions over the past 100 years. Tropical storms, hurricanes and major hurricanes have likely increased in sync with each other and the observed increase in, e.g. major hurricane numbers, has likely occurred largely from the overall increase in tropical cyclone counts rather than some artificial increase associated with improved observing systems.

Conclusion (3) should be changed to: “It is currently unknown how the frequency of hurricanes and major hurricanes for the whole basin have changed in the last 100 years due to unreliable data being available because of undersampling of these cyclones over the open ocean. However, records are relatively complete for U.S. landfalling hurricanes and major hurricanes since 1900, and these suggest no significant trend up or down.”

General Response: For the reasons outlined above, Fig. 2.17 and the associated discussion and conclusions are an important, if controversial, component of the overall assessment and are retained.

This reviewer’s interpretation: Discussion about the Atlantic Multidecadal Oscillation and how this may well account for observed Atlantic tropical cyclone activity must be included. As first suggested by Gray (1990), there exists significant multidecadal variability in Atlantic tropical cyclone activity that is linked to concurrent changes in both Atlantic SSTs and tropospheric circulation. These AMO phases were defined to be (Landsea et al. 1999, Goldenberg et al. 2001): Warm SSTs/low shear/high tropical cyclone activity: 1870-1902, 1926-1970, and 1995 to today; cold SSTs/high shear/low tropical cyclone activity: 1903-1925, 1971-1994. Vimont and Kossin (2007) showed that the AMO is the low-frequency manifestation of the more general Atlantic Multidecadal Mode, which has warm SSTs, weakened lower tropospheric tradewinds and upper tropospheric westerlies, reduced windshear, and enhanced convection in the main development region during active Atlantic hurricane seasons. Kossin and Vimont (2007) showed that the AMM is currently in an enhanced phase similarly to that observed back in the middle 20th Century. These active phases tend to have more easterly wave-induced formation of tropical cyclone in the main development region (see Landsea et al. 1999, Goldenberg et al. 2001). It is argued that the AMO is the main driver of tropical cyclone activity in the Atlantic on multidecadal timescales because of the combined dynamical and thermodynamical changes working together to either promote (in the warm phase) or inhibit (in the cold phase) tropical cyclone activity. In this context, the "jumps" suggested by Holland and Webster (2007) around 1930 and 1995 are indeed real, but are driven by AMO variations. What Holland and Webster's (2007) analysis misses is the jump downward around 1970 because it was partially masked by missing tropical cyclones in the pre-satellite era (that is, the actual number of tropical cyclones dropped around 1970, but was masked by the introduction of newly visible tropical cyclones due to complete satellite coverage). Mann and Emanuel (2006) and Trenberth and Shea (2006)'s suggestion that the AMO is much weaker than analyzed in Goldenberg et al. (2001) is problematic because of the methodology in both where global SST anomalies are subtracted from the Atlantic SSTs. First, the Atlantic SSTs contribute substantially to the global SSTs. Thus subtracting global SST anomalies from Atlantic is artificially removing the signal (i.e., throwing the baby out with the bath water). Secondly, both studies suppose that the AMO is an Atlantic only phenomenon. As discussed in Mestas-Nunez and Enfield (1999) and Zhang et al. (2007), the AMO also has significant weighting in other ocean basins as well. Thus the AMO should continue to be viewed as a feature that can modulate tropical cyclones in the Atlantic on the multidecadal timescale.

General Response: At no stage in this assessment is the presence of a marked multi-decadal oscillation denied, our findings are that there is also very likely a contribution to Atlantic SST warming by human influences.

The contributions by anthropogenic vs. natural oscillations to changes in tropical Atlantic SSTs have been discussed by, e.g., Santer et al (2006), Mann and Emanuel (2006), Trenberth and Shea (2006) and Holland and Webster (2007). They all find a substantial anthropogenic influence that is now larger than that from natural oscillations (and this is consistent with the more general IPCC findings). Vimont and Kossin (2007) also acknowledge the potential influence of anthropogenic warming on the AMM.

We include in the report discussion of modulation of TC activity by different climate modes, such as the Atlantic Meridional Mode. Some additional statements have been added to the report pertaining to the different views on the Atlantic Multidecadal Oscillation (AMO). There are still many open scientific questions about the role of the AMO.

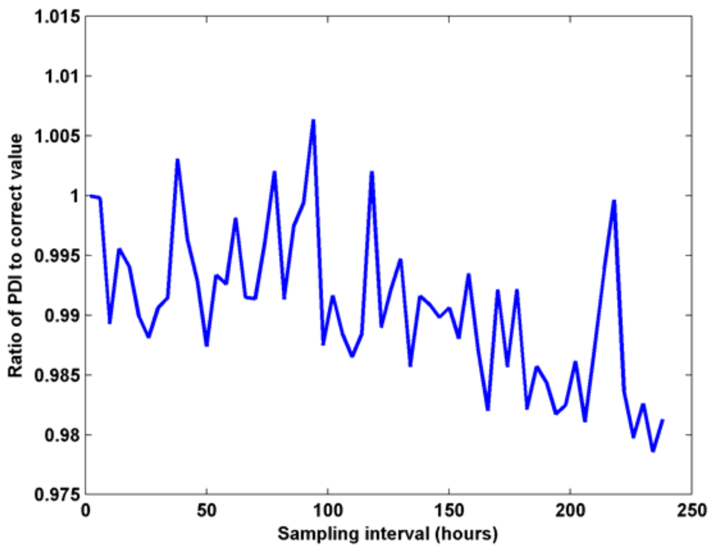
Concerning supposed large frequency changes across the satellite/pre-satellite era as inferred by Landsea (2007), the new ship-track based analysis of Vecchi and Knutson (2007) does not support the notion of such a large "step-function" inhomogeneity in storm counts near 1965. In order for Landsea (2007) to be correct on this point, there would need to have been a considerable number of storms (over 2 per year relative to later years prior to 2003), which were observed at least peripherally by ships but have not made it into the HURDAT database. The ships presumably would have recorded the winds, so if such storms existed there is likely evidence of them in ship reports. At this point there appears to be little evidence for the existence of a large number of such cases. The Landsea (2007) and Vecchi and Knutson (2007) missing storm adjustments do appear to be in better agreement during the early 20th century period.

Landsea, CH2-6, Page 131: "Documentation of the occurrence of tropical cyclones is thought to be reliable back to about 1945 in the Atlantic…(e.g. Holland and Webster 2007 and references therein)." This is incorrect. Landsea (2007) demonstrated that due to the lack of geostationary satellite imagery that began in 1966 that there was a step-function in monitoring over-ocean tropical cyclones with a substantial number missed before that time. Vecchi and Soden (2007) confirm that many tropical cyclones were likely undetected during the late 1940s, 1950s and early 1960s. The Holland and Webster (2007) reference is inappropriate here as they are simply assuming that 1945 was a reasonable starting date, rather than demonstrating this to be the case.

General Response: The reviewer misstates the level of agreement between Landsea (2007) and Vecchi and Knutson (2007) concerning numbers of missing storms in the 1950s and 60s. The former study finds a "step function" in missing storms in 1965, whereas the latter study does not find evidence for such a large step change. Unless there are substantial number of storms that were observed by ships (and presumably gale-force winds reported), but have not yet been included in HURDAT, it appears that the findings of Vecchi and Knutson (2007) do not support the notion proposed by Landsea (2007) of a step-function in missing storms in 1965 (beginning of satellite era). Landsea did not prove the existence of such a step function. Rather, he hypothesized its existence mainly by visual examination of the proportion of storms making landfall as a function of time. However, there are also physical reasons (e.g., Atlantic Meridional Mode or AMM) why the proportion of storms making landfall may also have multidecadal variation. Also, identification of a "step function" change in a noisy timeseries can be a tricky problem, as real climate changes, including shifts to different phases of internal oscillations such as the AMM, can also appear to happen rather abruptly.

Landsea, CH2--7, Page 134/Fig 2.13: Which bias-removal scheme from Emanuel (2005) or Emanuel (2007) was employed? This needs to be mentioned explicitly. Additionally, as mentioned above, it must be mentioned that the PDI values will be underestimated before the 1970s because of the low bias in duration, intensity and frequency due to lack of geostationary satellites.

General Response: The bias removal scheme was that described in Emanuel (2007). Since PDI is an integral quantity, it is not strongly affected by undersampling. The graph below shows the ratio of the estimated PDI to its true value made by undersampling 12 random synthetic tracks, as a function of the sampling interval. This is meant to represent a typical estimate over 1 year in the Atlantic. Even at 10-day sampling interval, the calculated PDI is 98% of its true value, even though whole storms are missed. We are not aware of any documented evidence for systematic intensity biases other than those documented by Landsea (1993) and corrected for according to Emanuel (2007).



<u>New References</u>:

Bove, M. C., J. B. Elsner, C. W. Landsea,, X. Niu and J. J. O'Brien, 1998: Effect of El Niño on U.S. landfalling hurricanes, revisited. *Bull. Amer. Meteor. Soc.*, **79**, pp. 2477-2482.

Gray, W. M., 1990: Strong association between West African rainfall and US landfall of intense hurricanes. *Science*, **249**, 1251-1256.

Holland, G. J., 1981: On the quality of the Australian tropical cyclone data base. *Aust. Met. Mag*., **29**, 169-181.

Landsea, C.W., 1993: A climatology of intense (or major) Atlantic hurricanes. *Mon. Wea. Rev.*, **121**, pp. 1703-1713.

Landsea, C.W., Pielke, Jr., R.A., Mestas-Nuñez, A.M., Knaff, J.A., 1999: Atlantic basin hurricanes: Indices of climatic changes, *Climatic Change*, **42**, pp. 89-129.

Mestas-Nuñez, A.M and D.B. Enfield, 1999: Rotated global modes of non-ENSO sea surface temperature variability. *Journal of Climate*, **12**:2734-2746.

Mayfield, CH 2-1, Page 96 , Lines 2154-2156: Suggest changing to something along the lines of "The Power Dissipation Index (which combines storm intensity, duration, and frequency) has increased since 1995 compared with the 1970s to the mid 1990s. A comparison with PDI in earlier years is uncertain due to sampling problems in these earlier years making it difficult to discern long term trends with confidence."
Max Mayfield, Former NHC Director

Response: We note the issue of limited coverage of aircraft reconnaissance in the report in several places. Nonetheless, we conclude that even with these limitations, it is likely (at least 67% chance) that PDI has increased substantially since the 1950s and 60s.

As we state in Section 2.2.3.1.3: "….Landsea (2005) commented on the quality of data comprising the index, arguing that the PDI from the 1940s to the mid-1960s was likely underestimated due to limited coverage of the basin by aircraft reconnaissance in that era. An updated version of this analysis (Emanuel 2007), shown in Fig. 2.13, confirms that there has been a substantial increase in tropical cyclone activity since about 1970, and indicates that the low-frequency Atlantic PDI variations are strongly correlated with low-frequency variations in tropical Atlantic SSTs. PDI, which integrates over time, is relatively insensitive to random errors in intensity. Taking into account limitations in data coverage from aircraft reconnaissance and other issues, we conclude that it is likely that hurricane activity, as measured by the Power Dissipation Index (PDI), has increased substantially since the 1950s and 60s in association with warmer Atlantic SSTs. The magnitude of this increase depends on the adjustment to the wind speed data from the 1950s and 60s (Landsea 2005; Emanuel 2007)."

Mayfield, CH 2-2, Page 96, Lines 2160-2161: Suggest changing to "The historical data base clearly shows an increase in tropical cyclone frequency in the North Atlantic over the past 100+ years. However, there was likely a number of tropical cyclones missed before the use of geostationary satellites. After accounting for these missed cyclones, one study shows no significant trend." (Landsea, 2007 EOS)
Max Mayfield, Former NHC Director

Response: As indicated in our earlier response, the Landsea (2007) analysis is based on an assumption of constant landfall proportion, which is not justified in the study and which has been strongly disputed by Holland (2007) and Sabbatelli et al (2007). We fully support further research into why the overall landfall frequency does not match the basin-wide studies. The ship-track based analyses of Vecchi and Knutson (2007) and Chang and Guo (2007) do not rely on an assumption of constant proportion of landfalling storms. The Vecchi and Knutson study finds an adjustment for missing storms that is somewhat smaller than Landsea's (2007). The resulting adjusted time series has a significant positive trend from 1900 to 2006, but the adjusted data from 1878 shows no significant trend. Thus, apart from the start year (1878 vs. 1900), this is a similar overall conclusion to that put forth in the reviewer's comment.

Mayfield, CH 2-3, Page 97, Lines 2164-2165: "It is currently unknown how the frequency of hurricanes and major hurricanes in the Atlantic have changed over the long term due to unreliable data."
Max Mayfield, Former NHC Director

Response: Below is quoted our revised text and discussion concerning Fig. 2.17 on long-term trends in hurricanes and major hurricanes. Note that we find some evidence for significant trends in these since 1900, a finding which is further supported by the significant trend in adjusted tropical storm counts from that era combined with the observation that a relatively constant proportion of tropical storms become hurricanes and major hurricanes over the long term. We also note the less compelling evidence for significant trends from earlier periods in the 1800s owing to the high reported hurricane activity from those periods, and the lack of significant trend in U.S. landfalling storms. This is a reasonable and balanced view of this issue.

"Atlantic basin total hurricane counts, major hurricane counts, and U.S. landfalling hurricane counts as recorded in the HURDAT data base for the period 1851-2006 are shown in Fig. 2.17. These have not been adjusted for missing storms, as there was likely less of a tendency to miss both hurricanes and major hurricanes in earlier years compared to tropical storms, largely because of their intensity and damage potential. However, even though intense storms were less likely than weaker systems to be missed entirely, lack of satellite coverage and other data issues imply that it would have been much more difficult to measure their maximum intensity accurately, leading to a potential undercount in the hurricane and major hurricane numbers. Using the unadjusted data, trends in hurricane counts, ending in 2005 and beginning in 1881 through 1921 are positive and statistically significant (p=0.05) whereas trends beginning in 1851 through 1871 are not statistically significant, owing to the high counts reported in the late 1800s. For major hurricanes, trends to 2005 beginning in 1851 through 1911 were positive and statistically significant, whereas the trend beginning from 1921 was positive but not statistically significant[1]. The significant positive trends since 1900 in hurricane and major hurricane counts are supported by the significant positive trends in tropical storm counts since 1900 and the observation that hurricane and major hurricane counts as a proportion of total tropical storm counts are relatively constant over the long term (Holland and Webster 2007). Regarding the trends from the 1800s, the lack of significant trend in hurricane counts from earlier periods is qualitatively consistent with the lack of significant trend in adjusted tropical storm counts from 1878 (Fig. 2.16). For major hurricanes, the counts from the late 1800s, and thus the significant positive trends from that period, are considered less reliable, as the proportion of storms that reached major hurricane intensity, though relatively constant over the long-term in the 20th century, decreases strongly prior to the early 1900s, suggestive of strong data inhomogeneities. There is no evidence for a significant trend in U.S. landfalling hurricane frequency."

Mayfield, CH 2-4, Page 131, Line 2917: Perhaps this would be an appropriate place to note that the Dvorak technique (subjectively using visible satellite imagery) was first published in 1973 and the more objective Dvorak technique (using enhanced infrared imagery) was not published until 1984.
Max Mayfield, Former NHC Director

Response: Thank you. This suggestion has been incorporated in the revised text.

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[1] Further details of the statistical analysis are given in the Appendix, Example 6.

Mayfield, CH 2-5, Page 131, Lines 2925-2926: Some may think the data is reliable back to about 1945 with the advent of recon, but I certainly don't. Surely some tropical cyclones were missed before the routine use of geostationary satellites. Please remember that in the Atlantic, aircraft reconnaissance is not done over the entire basin. In general the aircraft don't go east of about 55W and they are flown mainly on TCs threatening land.
Max Mayfield, Former NHC Director

Response: Please see response above to "**Mayfield, CH 2-1**, Page 96 , Lines 2154-2156."

Mayfield, CH 2-6, Page 134, Lines 2989-2994: A statement needs to be included about the bias correction that was used by Emanuel. It is my understanding that Landsea has rejected the use of this correction. Fig. 2.13 should not be shown without stating a bias correction has been made to the official data.
Max Mayfield, Former NHC Director

Response: The Emanuel bias correction has been adjusted and Landsea now acknowledges that this is not an issue.

Mayfield, CH 2-7, Page 137, Lines 3052-3056: Should be consistent with conclusion on page 96 and will hopefully be along the lines of "The Power Dissipation Index (which combines storm intensity, duration, and frequency) has increased since 1995 compared with the 1970s to the mid 1990s. A comparison with PDI in earlier years is uncertain due to sampling problems in these earlier years making it difficult to discern long term trends with confidence."
Max Mayfield, Former NHC Director

Response: Please see response above to "**Mayfield, CH 2-1**, Page 96 , Lines 2154-2156."

Mayfield, CH 2-8, Page 141-142, Lines 3162-3163: "…they appear to be insufficient…" is only true to some scientists. This should be stated.
Max Mayfield, Former NHC Director

Response: We've modified the text in this section (see below) and no longer use this statement, which addresses this comment.

"In summary, we conclude that there have been fluctuations in the number of tropical storms and hurricanes from decade to decade and data uncertainty is larger in the early part of the record compared to the satellite era beginning in 1965. Even taking these factors into account, it is likely that the annual numbers of tropical storms, hurricanes and major hurricanes in the North Atlantic have increased over the past 100 years, a time in which Atlantic sea surface temperatures also increased. The evidence is less compelling for significant trends beginning in the late 1800s. The existing data for hurricane counts and one adjusted record of tropical storm counts both indicate no significant linear trends beginning from the mid- to late 1800s through 2005. In general, there is increasing uncertainty in the data as one proceeds back in time. There is no evidence for a long-term increase in North American mainland land-falling hurricanes."

Mayfield, CH 2-9, Page 142, Lines 3163-3166: This should be consistent with conclusion on page 96 and along the lines of "The historical data base clearly shows an increase in tropical cyclone frequency and intensity in the North Atlantic over the past 100+ years. However, there was likely a number of tropical storms/hurricanes and major hurricanes missed before the use of geostationary satellites." In regard to major hurricanes, please remember that the very subjective Dvorak technique based primarily on visible imagery was not published until 1973. And the more objective Dvorak technique using enhanced infrared imagery was not even published until 1984. The very simple truth is that before 1984 we did not know for certain how intense hurricanes were without recon aircraft. And the aircraft were only flown for those tropical cyclones threatening land generally west of 55W. Surely no one thinks that we always had reliable ship reports documenting the intensity of major hurricanes. In my opinion, this is a very big flaw in the studies indicating that we now have more of the stronger hurricanes.
Max Mayfield, Former NHC Director

Response: This comment feels intuitively correct, but it does not stand up under a logical analysis of the available data. The maximum intensity achieved by any cyclone in any one year certainly increased with the combined onset of aircraft and satellite reconnaissance; but gross indicators of intensity have not changed over the last 100 years. For example: the mean intensity in any one-year has been remarkably constant, as has the proportion of hurricanes and major hurricanes (Holland and Webster 2007). This stability of the intensity record is an expected result from previous studies (e.g. Henderson-Sellers et al. 1998) that have shown the potential intensity change from the observed increase in SSTs would be at best several percent. Our revised summary statement for this section (see previous response), our inclusion of additional caveats on the limits of aircraft reconnaissance and the Dvorak technique, and our earlier statements on the likelihood (likely, but not certain) of a substantial PDI increase since the 1950s and 60s address this comment.

Ren, CH2-1, P99, L2210: Extreme events such as warm nights, warm days, cold nights, cold days, extreme heat waves, cold waves, frost days, frost-free season, and so on, are all related to the observations of surface air temperature. The records of the average air temperature and the events related to it might be affected by the urbanization processes. The effect has not been adjusted for most of the surface climate stations across North America. It would be good for the authors to make some evaluations of the possible biases resulting from the effect. (Guoyu Ren, National Climate Center, China)

Response: It is acknowledged that the results showing changes in warm days-nights, cold days-nights are from stations that have not been adjusted for urban warming. However, recent work (e.g. Peterson, T. C. and T. W. Owen, 2005: Urban Heat Island Assessment: Metadata are Important. *Journal of Climate*, **18**, 2637-2646; Peterson, T. C., 2003: Assessment of Urban Versus Rural In Situ Surface Temperatures in the Contiguous U.S.:

No Difference Found. *Journal of Climate*, **18**, 2941-2959.; Easterling et al. 1997) show that urban warming is only a small part of the observed warming since the late 1800s. A sentence has been added to state this point.

Ren, CH2-2, P106, L2364~2370: "…temperature range (Tmax minus Tmin) for the warm season (June-September) averaged over all of Mexico has increased by 0.26°C/decade since 1970 with particularly rapid rises since 1990 reflecting a comparatively rapid rise in Tmax with respect to Tmin. This behavior departs from the general picture for many regions of the world, where warming is attributable mainly to a faster rise in Tmin than in Tmax). ". This is indeed interesting, and the causes for the more rapid increase of maximum temperature and the difference from the other regions could be explained in the assessment report. Does this have something to do with the local human activities? (Guoyu Ren, National Climate Center, China)

Response: The document has been changed to reflect a discussion of why the DTR in Mexico has increased due to dynamic changes in rural population numbers, numbers of grazing animals and rates of change in rapid and severe soil erosion. This information was contained in the 2005 document.

Ren, CH2-3, P108, L2401~2405: "For the U.S., the percentage area affected by severe to extreme drought highlights some major episodes of extended drought. The most widespread and severe drought conditions occurred in the 1930s and 1950s. The early 2000s were also characterized by severe droughts in some areas, notably in the western U.S." and **Ren, CH2-4,** P110, L2460~2463: "There is evidence of earlier, even more intense drought episodes. A period in the mid to late 1500s has been termed a "mega-drought" and was longer-lasting and more widespread than the 1930s Dust Bowl". These observations are very interesting, and they imply that the 20th century warming over the continent might not have caused more severe drought. The statement that more severe droughts will occur in the inland regions under global warming might be incorrect, at least for North America. (Guoyu Ren, National Climate Center, China)

Response: The comment regarding the possibility that warming over the continent has not led to more droughts is directly addressed in the very recent paper by Easterling et al. (2007 GRL). This paper shows that the increase in precipitation in the U.S. has masked a tendency for more drought with the observed increased temperatures.

Ren, CH2-4, P141~142, L3159~3166: Data uncertainty is larger in the earlier parts of the record, as the authors have correctly pointed out. The upward trend may be caused by the more advanced observational technologies and denser observational network in some extent. However, the statement that "While there are undoubtedly data deficiencies and missing storms in the early record, they appear insufficient to remove the observed positive trends in basin-wide tropical storm counts." is acceptable, though the sentence following this could be reconsidered for minor revision. (Guoyu Ren, National Climate Center, China)

Response: We have changed the above sentence in response to this comment and another reviewer's comment. The revised summary statement is as follows:

"In summary, we conclude that there have been fluctuations in the number of tropical storms and hurricanes from decade to decade and data uncertainty is larger in the early part of the record compared to the satellite era beginning in 1965. Even taking these factors into account, it is likely that the annual numbers of tropical storms, hurricanes and major hurricanes in the North Atlantic have increased over the past 100 years, a time in which Atlantic sea surface temperatures also increased. The evidence is less compelling for significant trends beginning in the late 1800s. The existing data for hurricane counts and one adjusted record of tropical storm counts both indicate no significant linear trends beginning from the mid- to late 1800s through 2005. In general, there is increasing uncertainty in the data as one proceeds back in time. There is no evidence for a long-term increase in North American mainland land-falling hurricanes."

CHAPTER 3 COMMENTS AND RESPONSES

Goklany, CH3-1, Pages 240-350, Lines 5386-8165: For a chapter that is titled, "How Well Do We Understand the Causes of Observed Changes in Extremes, and What Are the Projected Future Changes?" there is surprisingly little discussion of paleo studies, and whether and how well the spatial and temporal patterns of floods, droughts and hurricanes indicated in such studies can be explained based on our current understanding. In not discussing this matter, the assessment is ignoring a good part of the science that can help develop a better understanding of the processes that affect extreme events. Such understanding is essential if one hopes to project, with reasonable confidence, changes in the frequencies, durations and magnitudes of such events, as well as their future locations and, possibly, timing. Specifically, regarding attribution, the chapter should discuss whether the precise methods employed in the attribution studies were tested against the results of paleo studies and, if so, how well did these methods reproduce the spatial and temporal patterns of storms (and other variables) suggested by the paleo studies. This would give us an indication regarding how well the attribution methods incorporate the sources of natural variability. On the other hand, if the attribution studies didn't undertake such studies, the chapter should address the level of confidence that can be ascribed to their ability to model natural variability. Incidentally, the reference section cites several paleo studies on lines 7617-7636 and 7185-7205 but, unfortunately, I don't see these being used within the text.
Indur Goklany, Department of the Interior

Response: The primary focus of this chapter is to assess the causes of changes in extremes as observed in the modern instrumental record. Paleoclimate studies are taken into account in the discussion of observed changes in drought and hurricane activity in Chapter 2. However, these studies are not sufficiently accurate to be included in detection and attribution studies, particularly on extremes and on continental and smaller scales. Detection and attribution approaches have been applied to millennial reconstructions of Northern Hemisphere mean temperatures, and in that case have been shown to perform well (see for example, Chapter 9 of the IPCC WG1 AR4 report). The references for this chapter have been reviewed and corrected as appropriate.

Goklany, CH3- 2, Pages 240, Lines 5410-5411: As noted previously, precipitation is only one factor contributing to floods (and droughts), and is less significant socioeconomically than floods (and droughts). The discussion of observed changes in this chapter should be extended accordingly. It would also be worth discussing streamflow and runoff.

Indur Goklany, Department of the Interior

Response: The intent of the chapter is to discuss attribution of past events and projections of future events. There are no attribution studies we know of for floods, although we are aware that some work of this kind is underway in the UK for flooding events that have affected that country. We included available, relevant information about changes in runoff later in the chapter. Stream flow and runoff are not in themselves extremes and not a subject for SAP 3.3.

Goklany, CH3- 3, Pages 241, Lines 5415-5417: Append to the end of this sentence the following: "but historical and paleotempestological data do not indicate any increase in US landfalling hurricanes." Without explicitly alluding to "US landfalling hurricanes" some readers may conclude that the sentence as it currently stands also applies to the mainland USA, and readers are owed clarity (and anticipating and avoiding ambiguity is one aspect of that).

Indur Goklany, Department of the Interior

Response: The comment refers to a sentence in that has been completely revised based on our new attribution statement. The revised statement addresses the concern of the reviewer.

Goklany, CH3- 4, Pages 258, Lines 5811-5815: This is a significant finding and should, therefore, be included in the Executive Summary.

Indur Goklany, Department of the Interior

Response: We disagree. This result is based on only one study involving just one model. This does not provide a sufficiently robust basis to highlight the result as a significant finding in the executive summary.

Goklany, CH3- 5, Pages 277-310, Lines 6237-7006: Section 3.3 titled, "Projected Future Changes in Extremes, Their Causes, Mechanisms, and Uncertainties", doesn't address many relevant issues, perhaps because it is poorly structured from the point of view of logic. Regardless of the reason, it doesn't serve the reader well. In general, among the issues that should be addressed here with regard to future projections and how much confidence can be attached to current projections are the following (more or less in logical order):

(a) What methodologies were used for the projections?
(b) What was the "training" period and what, if anything, was done to ensure correspondence between results and observations during the training period? Were these modifications scientifically reasonable, and why? How did the results vary temporally, spatially, in frequency, intensity and so forth, from observations during the training period? [While on this issue, as noted in the earlier comment on Figure ES.4, this figure indicates that the observations apparently lie outside the 95% confidence interval for the model results during much of the relatively short period for which both model results and observational data were plotted. One suspects that much of the correspondence may be due to the fact that the models were trained using a substantial portion of that record. This illustrates why the issue posed here is important to allow both the authors of this assessment and readers to judge how much confidence can or should be attached to model results. In any case, this matter should be discussed in this assessment in Section 3.3.]
(c) Were the spatial and temporal patterns (regarding frequencies, intensities, and spatial and temporal variations) of extreme events from model results compared with instrumental and/or paleo data? How "good" was the correspondence related to these factors?
(d) What were the assumptions regarding future emission pathways and other sensitive factors, and how reasonable are they in light of experience? How would alternate assumptions affect the projections?
(e) What were the projections?
(f) What do answers to items (b), (c) and (d) above imply about the level of confidence that can or should be attached to the projections that were made?

[The reason for posing questions in the above order is that if the answer to (c), for instance, is "not good" then readers would probably save themselves the time and trouble of reading further about that study.] Unfortunately, chapter 3.3 does not systematically ask or answer questions such as those outlined above except, to a limited extent, for the case of tropical cyclone frequency in the Atlantic (in Section 3.3.9.6). In particular, questions (b), (c) and (d) are not addressed in most instances. So after all is read and done, it is hard to judge what credence, if any, can be attached to projections of other events (except for TC frequency in the Atlantic). The value of a systematic approach is confirmed by the limited discussion on tropical cyclones in Section 3.3.9.6 regarding reconciling future projections and past variations which notes that "In fact, the 20th century behavior in TC frequency has not yet been documented for existing models." This is very useful information. The assessment should have attempted similar reconciliation with respect to other categories of extreme events.

Indur Goklany, Department of the Interior

Response: The report follows a logical structure: introduction, past climate changes, future climate changes. In the future climate section, changes in extremes for each variable are discussed in detail.

Much of the specific information requested by the reviewer is available in other SAP documents. The text has been modified to reference those other reports and two new paragraphs have been added to describe the state-of-the-art climate models used through the projections chapter. In these new paragraphs an overview of the construction and evaluation of climate models is given. The reader is referred to the other SAP reports and the IPCC report for more details.

The evaluation of the climate models simulation of extremes over the historical period and their future projections for many variables are given in this report.

Goklany, CH3- 6, Pages 307, Lines 6937-6940: Considering what the assessment notes on lines 6928-6929 as well as on subsequent lines, the last sentence is speculation and should be deleted.
Indur Goklany, Department

Response: We disagree with this sentiment of the reviewer. Our comment is motivated by known potential limitations of models.

Landsea, CH3-1, The chapter concludes that (1) "the balance of evidence suggests that human activity has caused a discernable increase in tropical storm/hurricane and major hurricane frequency in the North Atlantic" and (2) "it is likely that surface wind speeds of the strongest hurricanes/typhoons will increase by about 2 to 10% per degree Celsius tropical sea surface warming". Both conclusions – especially the first – are extremely problematic. Details are provided below.

Response: The responses to the issues raised by the reviewer are presented at appropriate points in the reviewer's comments that are presented below.

Landsea, CH3-2, Pages 267-273: Conclusion (1) - Attribution of tropical cyclone changes to anthropogenic warming. It is curious and not logical that the attribution section should come BEFORE presentation of results from theoretical and numerical modeling work on how anthropogenic warming affects tropical cyclone activity. To date, there have been no (zero) global warming-tropical cyclone attribution studies. To come to the dramatic conclusion above, the assessment relies (page 272) "on statistical analyses and expert judgment to make attribution assessments". The argument made in these pages essentially is as follows: additional greenhouse gases have warmed the tropical oceans and thus the warmer SSTs have caused the observed increased trends in tropical cyclone activity. Such overly simplistic reliance upon SST changes neglects the reality that tropical cyclone activity is dependent upon numerous factors much more complex than just SSTs (e.g., potential intensity, tropospheric wind shear, low-mid level moisture, tropical wave vorticity, etc.). It is quite likely that when a thorough attribution study is conducted that it would not support such a bold conclusion due to the negligible to tiny changes to tropical cyclone activity that have been caused to date by anthropogenic climate change from theory and modeling results thus far. (For more about theory and modeling results, see below.) It is quite unlikely that (page 273) the authors of Kossin and Vitmer (2007), Vitmer and Kossin (2007), and Vecchi and Knutson (2007) would agree that these papers support the conclusion (1) reached in the assessment.

Response: The attribution statement that underlies this comment has been substantially revised and now addresses the reviewer's comment. The revised document also specifically notes that improvements in our understanding of the mechanisms that govern hurricane intensity would lead to better short and long-term predictive capabilities. It should also be noted that the author team does not accept the reviewer's characterization of modeled anthropogenic changes to tropical cyclone activity as "negligible to tiny". This is a matter of one's perspective. As an example, the results presented here suggest that (although not yet detected in observations) anthropogenic greenhouse gas forcing may have already caused hurricane core precipitation rates to increase by ~6% due to the 0.5^{o} C long-term warming of tropical Atlantic and Gulf of Mexico surface waters, and attendant increased water vapor. While this may seem "tiny" to the reviewer, consider the plight of some New Orleans and Mississippi residents who were trapped between rising flood waters and the ceilings in their homes during Hurricane Katrina flooding. It is conceivable that in some cases, relatively small (~6%) increments of near-storm precipitation might have meant the difference between survival and drowning – a stark reminder of threshold effects.

Modeling discrepancy: There must be some discussion in the assessment about the extreme disagreement between the large changes reported today in some observation studies and the tiny changes suggested today by the theoretical and numerical modeling work. Assuming the high end of the intensity sensitivity of 5% per degree C (see below), observed global warming-induced changes would force on the order of 1-2% stronger tropical cyclones today. The results reported in Emanuel (2005) are 500-800% too large and Webster et al. (2005) are 1200-1500% too large compared to these sensitivities. What have the numerical modelers and theoreticians concluded about attribution possibilities?

Knutson and Tuleya (2004) concluded that:

> "An important issue is whether and when any CO2-induced increase of tropical cyclone intensity is likely to be detectable in the observations. The magnitude of the simulated increase in our experiments is about +6% for maximum tropical cyclone surface winds . . . The SST changes observed for the past 50 yr in the Tropics imply that the likely SST-inferred intensity change for the past half century is small, relative to both the limited accuracy of historical records of storm intensity and to the apparently large magnitude of interannual variability of storm intensities in some basins. This further implies that CO2-induced tropical cyclone intensity changes are unlikely to be detectable in historical observations and will probably

> not be detectable for decades to come."

Emanuel (2004) similarly concluded that:

> "Can one detect an actual increase in global tropical cyclone intensity? . . . Since 1950 . . . one would expect to have observed an average increase in intensity of around 0.5 m/s or 1 knot. Because tropical cyclone maximum wind speeds are only reported at 5-knot intervals and are not believed to be accurate to better than 5 to 10 knots, and given the large interannual variability of tropical cyclone activity, such an increase would not be detectable. Thus any increase in hurricane intensity that may have already occurred as a result of global warming is inconsequential compared to natural variability."

Given that the theoretical and modeling studies have, if anything, suggested the sensitivity of tropical cyclone intensity change from global warming is even smaller in the Atlantic than what Knutson/Tuleya (2004) and Emanuel (2004) concluded, it is even less likely today that attribution of global warming changes will be detectable within the next few decades.

Because of the combination of the lack of believable trends in Atlantic tropical cyclone activity (see discussion in review of Chapter 2) and the continued tiny changes predicted today by theoretical and numerical modeling studies, please change the conclusion of (1) to: "the balance of evidence does not suggest that human activity has caused a discernable increase in tropical storm/hurricane and major hurricane frequency in the North Atlantic."

Response: The reviewer is incorrect in that there has been a substantial increase in Atlantic tropical cyclone activity, as measured by frequency or the combined power dissipation index. As stated in a number of earlier responses, the report's attribution statement has been modified. It recognizes the strong correlation between the hurricane frequency or power dissipation index and SST and notes the attribution of SST changes to human influence, but does not explicitly make the double attribution to hurricane changes and states that a confident attribution of hurricane changes to human activity awaits more data, research and analysis.

Modeling sensitivity: Conclusion (2) overstates the sensitivity of Atlantic tropical cyclone intensity to anthropogenic climate change. The new results of Vecchi and Soden (2007) get a consistent answer from the 18 IPCC global climate change models that the maximum potential intensity may not increase significantly for the whole basin (the main development region slightly decreases in MPI, while the subtropical waters slightly increases in MPI). Taken as a whole, the sensitivity from all of these models concludes a change in the Atlantic basin from 10 to 35N averages between 0-1% per degree C global warming with a range in the models going from -4% to +3%. Even assuming a fairly wide range given uncertainties and other modeling results, the projection should range from -2% (weakening) to +4% per degree C global warming. Both the bottom end and the top end of the projection indicated in the conclusion (2) must be lowered.

Please change the conclusion of (2) to: "Surface wind speeds of the strongest Atlantic hurricanes may change by about -2% (weakening) up to +4% (strengthening) per degree Celsius global warming. Pacific typhoon intensity may have a slightly higher sensitivity".

This reviewer's interpretation: What has been observed since 1995 in the Atlantic is an active phase of the AMO (or AMM) with warm SSTs, weakened tradewinds and upper tropospheric westerlies, reduced shear and enhanced convection (Goldenberg et al. 2001, Kossin and Vimont 2007). The global warming signal, on the other hand, consists of warm SSTs and greater – not less – tropospheric wind shear (Knutson and Tuleya 2004, Vecchi and Soden 2007). Attributing the circulation and wind shear changes observed since 1995 in the Atlantic to anthropogenic global warming is thus impossible, given the expectations from the IPCC AR4 model results. Thermodynamical forcing, as described above, is fairly small even several decades from now and so tiny today (a couple knots at most even for a Category 5 hurricane) that it would not be observable with today's technology. There has been nothing published in the theoretical or numerical modeling perspective that would suggest the 3-5% per C intensity change is too conservative. Instead, as has been shown by Vecchi and Soden (2007) it appears that this may be too high.

Response: The reviewer is correct that there are subregions of the Atlantic with slight negative projections in Emanuel MPI as computed by Vecchi/Soden. Not mentioned by the reviewer are the elevated increases in Emanuel MPI near the U.S. coast and in the Gulf of Mexico, where landfalling intensity may be more affected. In any case, the remainder of the northern tropics all shows considerable increase, while we generally have less confidence in small-scale regional details than in tropics-wide (area-averaged) behavior. The conclusion that the reviewer is addressing has been modified from the previous draft. The discussion now projects that, for each 1ºC (1.8ºF) increase in tropical sea surface temperatures, core rainfall rates will increase by 6-18% and the surface wind speeds of the strongest hurricanes will increase by about 1-8%.

Landsea, CH3-3, Pages 295 & 301: In interpreting the large changes in both intensity and frequency of Atlantic TCs in the Oouchi et al. (2006), one needs to examine what the baseline they utilized for analyzing the control climate. They used observed global SSTs from 1982-1993 to drive the AGCM Earth Simulator model. This short decade long period for a control run is problematic for two reasons: 1) the heavy incidence of moderate and strong El Nino events during this period (Trenberth and Hoar 1996), and 2) the cold, high shear phase of the AMO was dominating the low-frequency variability during this time (and from 1971 to 1994 for the whole period – Landsea et al. (1999), Goldenberg et al. (2001), Bell and Chelliah (2006)). Both of these factors contributed toward making the period of the 1980s and early 1990s extremely low TC activity in the Atlantic. Indeed the early 1990s were the quietest period in the Atlantic record going back to the 1940s (Landsea et al. 1996). The results in Oouchi et al. are biased toward a substantially low base period of comparison. Thus the results from this study would need to be substantially adjusted to take into account this bias and would reduce substantially or possibly counteract both the frequency and intensity increases found for the Atlantic. Ideally, such simulation experiments should utilize a longer control period to minimize the impact of such strong low-frequency variability. (This same issue is also a concern with Sugi et al.'s (2002) study that used a very quiet Atlantic control period of 1979 to 1988.)

Response: The reviewer is mistaken about the Oouchi et al. methodology in one respect and correct in another respect. By using only 20-yr periods from their control and warm climate GCM runs, Oouchi et al, may well have mixed up climate change signal with internal climate variability from their model. Thus, their results should be treated with caution. We note this in our revisions to the report and also note this as a general problem for such Atlantic studies. The 1982-93 base period (quiet period in real world) is not really a major problem, as they apply a climate change "delta" signal to this period for the perturbation run, and the climate change "delta" is derived from two periods in their coupled model integration and thus has no relation in phase to the internal climate modes in the real world.

Landsea, CH3-4, Page 268: "modeling and theoretical studies…predict a relatively small increase of around 1 to 7% for the observed 0.5 to 0.7 degree C trend in tropical North Atlantic SSTs." The range is too large on the high end. The recent studies suggest only a range of up to 4% increase per degree C for anthropogenic global warming changes in the Atlantic. Given that only a portion (roughly half) of the observed trend is due to man-made causes as determined by attribution studies, the predicted change due to anthropogenic global warming is at most only 1 to 2% stronger today (also see Knutson and Tuleya 2004, Emanuel 2004). Suggesting substantially larger is not appropriate.

Response: The reviewer states, with no support, citation, or references that "roughly half of the observed trend is due to man-made causes as determined by attribution studies." We are not aware of this result and no reference is given, therefore that aspect of the comment could not be considered when the sensitivity estimate was revised in the current draft. (See earlier response concerning the sensitivity estimate.)

Landsea, CH3-5, Page 299: "The enhanced vertical shear feature (present in about 14 of 18 models in the Caribbean region)" is not correct. All 18 of the IPCC AR4 models used in Vecchi and Soden (2007) show increased vertical shear over the Caribbean. Only near Cuba do all 18 show this at the exact same point, but all 18 of the model do overall have increased shear in the region.

Response: Based on direct contact with the referenced study's author, we have changed the questioned wording in the text and now describe the specific region used for the analysis.

Landsea, CH3-6, Page 305: "An important question for regions along the periphery of tropical cyclone basins is whether regions with [sic] have never of only infrequently experienced tropical cyclones in recorded history may experience them more frequently in the future owing to climate change." This point was addressed in Henderson-Sellers et al. (1998) and should be reiterated here: "The broad geographic regions of cyclogenesis and therefore also the regions affected by tropical cyclones are not expected to change significantly. It is emphasized that the popular belief that the region of cyclogenesis will expand with the 26°C SST isotherm is a fallacy."

Response: The reviewer's concern has been addressed by revising this section to state: "Changes in tropical cyclone activity may be particularly apparent near the wings of the present climatological distributions. For example, locations near the periphery of current genesis regions may experience relatively large fractional changes in activity."

Landsea, CH3-7, Page 306: "The high confidence of there being future sea level rise as well as the likely increase of the strongest hurricanes, leads to an assessment that the potential for storm surge damage (per hurricane) is very likely to increase." Such an unquantified statement is not appropriate. While it is agree by this reviewer that sea level will continue to rise due to anthropogenic global warming, what kind of impact would this cause in conjuction with storm surges if it is on the order of the 0.3 m change by 2100 as concluded by the IPCC? Please add – in the absence of any meaningful studies on the topic – the following: "However, it is unknown whether such changes – expected to be on the order of 0.3 m by 2100 – will significantly increase hurricane-caused damages because of a lack of studies on the topic to date. Given the modest increase in sea level (0.3 m) from global warming in comparison to the surge caused by the strongest hurricanes today (9.0 m), such an answer is not straightforward to answer without further research."

Response: The author team has no evidence to indicate that sea level rise will not lead to higher surge levels than occur today. However, they have modified the text and no longer use the term damages, but just refer to storm surge levels.

Mayfield, CH 3-1, Chapter 3, Page 265, Line 5960: Change "World Meteorological Society" to "World Meteorological Organization."
Max Mayfield, Former NHC Director

Response: We thank the reviewer for pointing out the typo, which has been corrected.

Mayfield, CH 3-2, Page 265, Line 5961: The Sixth International Workshop on Tropical Cyclones (IWTC-VI) is the correct name for the November meeting.
Max Mayfield, Former NHC Director

Response: We thank the reviewer for the correction, which is in the revised document.

Mayfield, CH 3-3, Page 265, Line 2962: should be followed with the fact that the AMS has endorsed the WMO IWTC-VI statements on Climate Change and Tropical Cyclones (recent BAMS).
Max Mayfield, Former NHC Director

Response: We have included this as a factual statement. But we note that the IWTC-VI statement was based entirely in published information. There have been a substantial number of published papers since the IWTC-VI, not all supporting these conclusions.

Mayfield, CH 3-4, Page 265, Lines 5963-5982: All of the bullets from the WMO IWTC-VI should be included. For example, the last one "If the projected rise in sea level due to global warming occurs, then the vulnerability to tropical cyclone storm surge flooding would increase." seems extremely important to me.
Max Mayfield, Former NHC Director

Response: We agree the sea level rise/storm surge issue is important; however, we choose to highlight and discuss it elsewhere in the report, rather than quote the IWTC-VI finding. In our revised document, we have removed all the WMO bullets to avoid creating the impression that they are conclusions of this document.

Mayfield, CH 3-5, Page 267, Line 6013: The report "accepts the overall findings of WMO (2006)….as they related to the North Pacific." Why not accept the WMO IWTC-VI findings for Atlantic basin????
Max Mayfield, Former NHC Director

Response: As we have indicated earlier, the IWTC-VI report was limited to published findings as at November 2006. For the North Atlantic there have been substantial additional publications, which we also use in our assessment. This is not the case for the North Pacific, so we have relied more on the IWTC-VI assessment here.

Mayfield, CH 3-6, Page 269, Lines 6055-6060: The Emanuel results of the PDI increase should not be given without Landsea's reference stating that a bias correction was used by Emanuel that he doesn't feel was justified. Please get input from Landsea.
Max Mayfield, Former NHC Director

Response: As stated earlier, this is no longer an issue.

Mayfield, CH 3-7, Page 269, Lines 6060-6064: It is fine to present the Holland and Webster results but not without also presenting the Landsea results published in EOS (Volume 88, Number 18, 1 May 2007) given he is a contributing author stating that there is no significant trend if one accounts for the storms that were missed before the geostationary satellite era.
Max Mayfield, Former NHC Director

Response: We do include statements several places in the report on nonsignificant trends in Atlantic tropical storm numbers, beginning from 1878. The methodology used to arrive at that conclusion differs from that of Landsea (2007), which does not require the assumption of a stationary proportion of landfalling hurricanes (which has been disputed by Holland, 2007), but the basic conclusion (apart from the start date) is similar.

Mayfield, CH 3-8, Page 271, Lines 6107-6109: Don't state Chapter 2 concludes "there has been an increase in tropical storm/hurricane and major hurricane frequency in the North Atlantic over the past century or so, a time during which tropical Atlantic SSTs also increased" without at least mentioning the concern over the historical record before the geostationary satellite era.
Max Mayfield, Former NHC Director

Response: We have carefully assessed the available published information on the quality of the historical record and have concluded that this does not invalidate the conclusions stated here. However, we agree with the reviewer's principle of indicating the continuing debate in this area and have indicated this in the revised version.

Mayfield, CH 3-9, Page 272, Lines 6133-6135: I'm fine with conclusions based on "…must rely on statistical analysis and expert judgment to make attribution assessments" as long as the authors of the report agree. I'll be very surprised if they all agree with the conclusions as currently stated.
Max Mayfield, Former NHC Director

Response: Comment noted. We have substantially changed the attribution assessment in response to comments from both external reviewers and from authors on the report. All authors of the report have accepted the revised statement.

Mayfield, CH 3-10, Pages 272-273, Lines 6137-6144: "…the balance of evidence now suggests that human activity has caused a discernible increase in tropical storm, hurricane, and major hurricane frequency." is totally at odds with the WMO IWTC-VI first statement "Though there is evidence both for and against the existence of a detectable anthropogenic signal in the tropical cyclone climate record to date, no firm conclusion can be made on this point."
Max Mayfield, Former NHC Director

Response: The attribution statement has been substantially changed in response to both external reviewers and further discussion by the authors, and is now closer to the WMO statement. Note, however, that supporting statements in this document include additional research published since the WMO meeting (e.g., Holland and Webster 2007, Mann et al 2007, Kossin et al 2007, Guo and Chang 2007, Sabbattelli et al 2007, Vecchi and Knutson 2007), so SAP 3.3 statements do not necessarily agree in all details with earlier statements.

CHAPTER 4 COMMENTS AND RESPONSES

No Comments Received

APPENDIX A COMMENTS AND RESPONSES

No Comments Received

GENERAL COMMENT AND RESPONSE

Panzer, GEN-1: The impact to humans from recent, so-called extreme events, are statistical blips in the geological time-scale of things. Extreme events only became important when humans started developing their society and their infrastructure in harms way. Examples: building large houses and resorts on barrier islands that are exposed, not to just hurricanes, but also to relatively normal, but strong weather and seas; building fragile mobile homes and other housing on cheap land which is cheap for a reason (e.g., subject to tornados, thunderstorms, flooding, etc.); building cities next to wetlands that are being degraded by other human activities (e.g., draining, pipelines, etc.); trying to contain large rivers with levees which are not impregnable (the California delta, the Mississippi along much of its length). The ludicrousness of this boggles the mind and when our government starts spending billions of dollars on the climate change bandwagon; it becomes annoying to the taxpaying public (except those who have chosen to build or live in the more dangerous zones above).

Further, as the world changes, humans and the ecosystems around us will adapt. If there is anything humans hate the most, it is change. When climate changes, humans feel powerless since they can't control it so they try anyway, ineptly and expensively. I'm not denying that climate change is not occurring. I only question the choices we are making as a society and our government is making on our behalf in response, in large part, to a very loud minority supported by a loud and largely ill-informed media.

My true comment: NOAA, EPA and other agencies who have been tasked with studying climate change need to present a balanced discussion including the long list of things we don't know and have no answers to. If this report does not contain this, it should not be published.

Response: The Executive Summary and Chapter 4 specifically discuss measures that can be taken to improve our understanding of weather and climate extremes. Throughout the report, the author team has taken care to insure balance and scientific objectivity and to precisely convey the degree of certainty of various findings and projections. Terminology used throughout this report to express the likelihood of each key finding is presented in the Figure below. In cases where there is sufficiently strong evidence to draw a conclusion, but not enough to allow a determination of 'likely', the term 'the balance of evidence' is used to express our assessment of the state of the science. Statements made without likelihood qualifiers are intended to indicate a high degree of certainty.


Common Language Used to Express Considered Likelihood Judgment
very unlikely
unlikely
about as likely as not
likely
very likely
50%
0%
100%
Likelihood