SHAKY SCIENCE: INCONVENIENT TRUTHS IGNORED BY EPA IN ITS PROPOSAL TO REGULATE CARBON DIOXIDE EMISSIONS

A review of important scientific findings on global warming based on the comments by Patrick J. Michaels submitted to the Environmental Protection Agency in response to its July 11, 2008 "Advanced Notice of Proposed Rulemaking [ANPR] Regulating Greenhouse Gas Emissions under the Clean Air Act." Patrick J. Michaels is a senior fellow in environmental studies at the Cato Institute and a research professor of environmental sciences at the University of Virginia. His latest book, *Climate of Extremes: Global Warming Science They Don't Want You to Know* (with Robert C. Balling) will be published in January 2009.

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The following document details important scientific findings on global warming that are generally not known to those involved in ongoing policy debates. A recent EPA call for public commentary on a proposed rulemaking on carbon dioxide emissions provided a remarkable opportunity to comprehensively analyze recent science that is often ignored in the push for carbon dioxide regulation. Readers are encouraged to cite the vast number of findings noted in this rather extensive paper.

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On July 11, 2008 the U.S. Environmental Protection Agency (EPA) announced an "Advanced Notice of Proposed Rulemaking [ANPR] Regulating Greenhouse Gas Emissions under the Clean Air Act." This 570-page document was largely in response to the 2007 Supreme Court decision, *Mass v. EPA*, which concluded that carbon dioxide was a pollutant requiring regulation under the Clean Air Act Amendments, enacted in 1990. It can be downloaded from http://www.epa.gov/climatechange/anpr.html

The EPA opened its ANPR for public commentary for 120 days following its initial release.

The publication of the ANPR provided a unique opportunity for a substantive and comprehensive criticism of EPA's scientific rationale for proposed regulation.

The scientific rationale for the ANPR is provided in an accompanying *Technical Support Document* [TSD] *for Endangerment Analysis for Greenhouse Emissions under the Clean Air Act*, a 136-page document released by the EPA as a "Sixth Order Draft" on June 21, 2008. This document can be downloaded from http://www.regulations.gov/fdmspublic/component/main?main=DocumentDetail&o=09000064806691e8

These comments on the ANPR are primarily based upon the completeness or complexity of the science upon which the EPA may base its determination of whether to make an "endangerment finding" triggering regulation of greenhouse gas emissions under the Clean Air Act.

Part I, beginning on Page 3, is a detailed set of comments made in direct response to the *Technical Support Document for Endangerment Analysis for Greenhouse Emissions under the Clean Air Act [TSD]* and addresses an ANPR inquiry on page 185 of the July 11 document:

> The following provides a summary of the underlying science that was reviewed and utilized in the Endangerment Technical Support Document for the endangerment discussion, which in turn relied heavily on the IPCC Fourth Assessment Report [subsequently "AR4", the 2007 climate

> compendium issued by the United Nations' Intergovernmental Panel on Climate Change]. ***We seek comment on the best available science for purposes of the endangerment discussion…***

The reasons why the EPA should *not* rely on the IPCC AR4 reports are made obvious in comments in Part I of this submission.

Part II, beginning on Page 111, is to answer the question posed on pages 387-388 of the ANPR:

> *Section 108 also requires that once a pollutant is listed, EPA issue "air quality criteria" encompassing "all identifiable effects on public health or welfare," including interactions between the pollutant and other types of pollutants in the atmosphere. We are interested in commenters' views on whether and how developing air quality criteria for GHGs* [greenhouse gases] *would differ from developing such criteria for other pollutants such as ozone and particular matter, given the long-lived nature of GHGs and the breadth of impacts and other special issues involved with global climate change.* ***EPA also invites comment on the extent to which it would be appropriate to use the most recent IPCC reports, including the chapters focusing on North America, and the U.S. government Climate Change Science Program*** [CCSP] ***Synthesis Reports as scientific assessments that could serve as an important source or as the primary basis for the Agency's issuance of "air quality criteria."***

In July, 2008, the U.S. Climate Change Science Program released a document, *Global Climate Change Impacts in the United States, Unified Synthesis Product*, for public comments through August 14, 2008. This document summarized the CCSP documents that served "as an important source" for the ANPR

As evidence why the EPA should *not* rely on Climate Change Science Program synthesis reports, we also submit our comments of the CCSP's *Unified Synthesis Product* that we filed with the CCSP during their public comment period**.** This is Part II of this report.

The *Federal Register* lists certain guidelines for EPA, stating "influential" data must involve "the best available science and supporting studies conducted in accordance with sound and objective scientific practices." (EPA Guidelines at §§5.1, 6.1-6.2, 6.4.). As shown below, there are many aspects of the Technical Support Document for the ANPR that do not meet this criterion.

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A. General Comments

Overall, the Endangerment TSD suffers from three primary deficiencies, each contributing to greatly weakening the foundation, tone, and conclusions of the report. These deficiencies render it unsuitable for basing an endangerment finding and subsequent regulation of carbon dioxide emissions.

1) There is a lack of basis in the most recent peer reviewed scientific literature—specifically, that published since cut-off date for inclusion in the United Nations' Intergovernmental Panel on Climate Change [IPCC] **Fourth Assessment Report** ["AR4"] **(late 2005/early 2006).**

Climate science is rapidly evolving, and basing a rulemaking of this magnitude on science which is now nearly three years old is a recipe for failure.

As an example, repeated claims are made in the TSD (based upon the IPCC) that increases in tropical cyclone activity, including current trends and future projections, are likely largely influenced by human greenhouse gas emissions. While this may have been our best understanding several years ago, it no longer is the case. Many studies, published since the IPCC *AR4* publication, support this contention. Vecchi et al. (2008) summarize recent results, including Vecchi and Soden (2007a), Bengtsson et al., (2007), Knutson et al. (2008), and Emanuel et al., (2008) and present a picture of future Atlantic tropical cyclones that is not unlike that of today—with natural cycles dominating the patterns of variability. A new study by Pielke Jr. et al. (2008) demonstrates that the escalating damages from recent hurricanes can be completely accounted for by demographic changes (and the changing value of the dollar)—with no sign of any impacts of climate changes**. As hurricanes are one of the leading causes of weather related damages in the U.S., and will likely remain so in the future, it is essential that any endangerment finding must recognize the complexity of this issue and that our best science currently suggests that our vulnerabilities largely arise from our *climate* and our behavior, rather than from human-induced climate *change*.**

Another major topic in which post-AR4 findings have had a major impact on our level of scientific understanding is on the rate of observed and future sea level rise. These findings question the extent to which the IPCC's suggestions of a recent accelerated sea level rise represents a sustained change or simply short-term variations and whether we are even able to make such a determination (Holgate, 2007; Wunsch et al., 2008; Willis et al., 2008). Other researchers have found a significant meteorological influence on the rate of observed sea level rise along the U.S. East Coast (Kolker and Hameed, 2007). And perhaps most significantly, research has demonstrated that the outflow of Greenland ice sheets is unlikely to dramatically speed up as a result of surface warming, thus countering the claims of rapid sea level rise this century (van de Wal et al., 2008; Joughin et al., 2008). All of these findings invalidate conclusions of the Endangerment Technical Support Document [TSD].

Additional findings published subsequent to the IPCC AR4 alter our best understanding in other topics as well. These are described in the body of these comments. It is essential that the large (and growing) body of scientific understanding that has arisen in time since the IPCC AR4 be considered in EPA's Proposed Rulemaking. The TSD is simply out of date.

2) The TSD contains logical errors leading to unsupportable and ill-informed conclusions that render it unsuitable for basing an endangerment finding.

Prime examples of this are in the "Human Health" and "Food Production and Agriculture" sections of the TSD. The TSD does not adequately factor in changing population age-structure and changing technologies in projecting harm to health and agriculture from a shifting climate. Had these factors been considered properly, a vastly

different conclusion would have resulted—one in which climate change would have been shown to have little detectable impact.

3) Perhaps the greatest deficiency in the Endangerment TSD is that it fails to recognize that the observed trends in measures of U.S. health and welfare during a time of increasing anthropogenic greenhouse gas emissions and resultant climate change are generally in the opposite sense than those predicted to occur.

Our on-going response to climate change invalidates pessimistic projections of a net negative impact. The Endangerment TSD completely negelcts constant advancement in fossil-fuel powered societies in the quality and length of life.

During the past decades—a time in which greenhouse emissions from human activity around the world have been growing, atmospheric concentrations of these gases have been increasing, and the radiative forcing of the earth's climate has been altered—our quality of life has been improving through our direct actions, be they technological improvements, medical advances, regulatory measures, community programs, etc. **In America, our sensitivity to excessive heat has been declining, our air quality has been improving, our crop yields have been increasing, and all of this adds up to the most important measure of all—our lifespans have been increasing. The role that climate, much less climate *change*, plays into all of this is likely a small one.** Crop yields increase because of better farming practices and crop varieties, air quality has improved because of direct actions aimed at reducing pollutant emissions, and our sensitivity to excessive heat has declined with the widespread use of air conditioning, medical improvements, and community awareness programs.

More and more, weather and climate are becoming less and less a negative factor in our overall health and welfare. Our adaptations have improved our ability to handle the negative consequences of weather while enjoying the positive ones. We have flocked to the places with the weather and climate that we enjoy (generally warm places!), such as the Southwest and Florida. We have also headed to the cities and to the coasts, places with climate perils of their own, the former from urban warming and lower air quality, and the latter with tropical cyclones and rising sea levels. Yet these threats have not slowed our migrations. Strong hurricanes still overwhelm many of our defenses, but by and large, we have become more resilient coastal dwellers with stricter building codes, advances in weather forecasting and warning systems, and improved community response and planning programs. An unusually severe heat wave may still take its toll, as will a strong tornado, or a violent thunderstorm, but overall we are better prepared and better equipped for these and other types of extreme weather events. Whatever challenges that climate *change*—be it natural variations, or those borne from our own activities—may present us, our resourcefulness and quest for a better life will surely overcome them. This has become fabric of our society—a constant quest to make out lives better and more comfortable. **Without recognizing this, the EPA is seriously overestimating the negative impacts of future climate change in light of our past behaviors and responses. This is a fatal flaw in this TSD.**

Furthermore, I have, for a long time, been disturbed by the general nature of the IPCC's scenarios for future emissions, and related estimate reproduced in the *Unified Synthesis Product* of the U.S. Climate Change Science Program [CCSP], another heavily cited source for the TSD and the ANPR. Their implicit assumptions, in general, are that the world remains highly dependent upon fossil fuels and that they are primarily combusted in fashions not highly dissimilar to those employed today.

Consider the real technological changes in the last century. Does one really expect such innovation to cease in this one? 100 years ago, *no one* would or could have anticipated the technological changes that ultimately occurred. Would *anyone* have suggested, for example, that a small amount of an Element that did not exist (Plutonium), if held in a confined space, could destroy Washington DC? Or that energy and matter were related in such a way that nuclear reactions (something also undreamt in 1908) could be controlled in a way that the same city could be powered for 100 years? Would *anyone* suggest that we could access virtually all the information in the world from a small box carried in our shirt pocket while we "flew" between Europe and America in seven hours? Or that we could all instantaneously communicate pictures, words and information to anyone else on the planet, if we chose to?

The answers are obviously "no." So how vacuous is the pervasive assumption that no major energy or information technology (to just name two fields) will arise that will dramatically change the world in magnitudes similar to those that developed in the last 100 years? How strange that the TSD never contains even a glimmer of this obvious reality. Why? Because this reality may render the entire enterprise of carbon dioxide regulation moot. The lack of attention to this shows how poorly the TSD supports any finding of endangerment.

The TSD suffers from a failure to incorporate the best available science, misapplied logic, and the inability to recognize that observed trends in measures of human health and welfare run counter to those anticipated—despite on-going climate changes. As such, the TSD does not serve as an adequate basis for which to base an endangerment finding.

B. Specific Comments on the Endangerment TSD

Executive Summary

Comments on the Executive Summary are brief and are described with much more detail and specificity in Comments made in regard to the body of the TSD text.

Page ES-1, lines 3-4

Much of the content of this report, which is largely based on the IPCC AR4, has become out-dated in the three years since the last new science that was included in the AR4. Major scientific findings have appeared since the publication of AR4 which

have altered our best scientific understanding from that characterized in the AR4. These include, but are not limited to, findings on Atlantic tropical cyclones (e.g., Knutson et al., 2008; Vecchi et al., 2008) and Greenland ice loss (and its effects on sea level rise) (Joughin et al., 2008; van de Wal et al., 2008). It is imperative that such findings be incorporated in the EPA's assessment of how future climate change may impact the U.S. The implication of these, and other post-AR4 findings are detailed in the body of these comments.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page ES-1, line 40-42 "Global mean surface temperatures have risen by 0.74ºC (1.3ºF) over the last 100 years. The rate of warming over the last 50 years is almost double that over the last 100 years."

While this is a true statement, it is an extremely unfair assessment of the nature of global temperature changes over the course of the past 100 years (1908-2007). Over this time period, global temperatures rose during the first third, cooled (or remained relatively steady) during the middle third, and rose again during the most recent third. The rate of warming during the first third (from 1908-1942) was 0.16ºC/decade, the same rate as that during the most recent third (1974-2007). Comparing the rate during the last 50 years, with that during the past 100 years, is done to make it seem like the rate of temperature rise has been increasing recently compared with past changes. This demonstration relies on a judicious selection of start dates rather than on a fair assessment of global temperature patterns. In fact, the rate of warming in recent decades is no different than that observed during an equal length period in the early 20th century.

This biased presentation by in the TSD does not reflect the true nature of observed changes.

Page ES-1, line 45, pertaining to U.S. temperatures: "With an increased rate of warming during the past 30 years."

Why does EPA only refer to the last 30 years here instead of the last 50 years as they did for the global record? In fact, the past 30 years is a much better time period to describe that warming taking place than the past 50—the earliest 2 decades of which temperatures declined.

In the U.S., the recent temperature behavior is much more complex than this short summary makes it out to be. A big jump occurred after the 1998 El Niño, which has the impact of increasing the apparent rate of warming over the past 30 years, despite the fact that over the past 10 years (since the step up from the 1998 El Niño) the temperatures have not risen at all. In fact, they show signs of recovering to their *pre*-1998 levels. Given time, such a recovery will significantly lessen the apparent rate of temperature rise in the U.S. This possibility cannot be overlooked in the Rulemaking consideration.

Page ES-1, lines 50-51

Regional climates, such as those of North America, are influenced by natural variations and cycles that are not well captured (especially the timing) by climate models run with anthropogenic and natural forcings.

Much of the climate of northwestern North America is influenced by the Pacific Decadal Oscillation, the timing and variability of which is not captured by climate models. Thus, the actual observed climate is not going to be well-replicated, or if it is so, the result is probably fortuitous. In Alaska, for instance, there has been no net temperature rise since 1976, but a large apparent trend from 1950-present. This temperature pattern has been driven largely by the PDO (e.g. Hartmann and Wendler (2005), rather than "global warming," yet climate models that project an increasing trend from the 1950s would seem to match the observed trend in Alaska—but for the wrong reasons. In fact, natural variations *do* explain most of the temperature change there for the past 50 years. Other natural cycles (El nino/Southern Oscillation [ENSO], the North Atlantic Oscillation [NAO], the Atlantic Multidecadal Oscillation [AMO], etc.) and circulation patterns (the Arctic Oscillation [AO], the Pacific-North American Pattern [PNA], etc.) play significant roles in determining the climate and climate trends over other portions of North America. Getting the timing and magnitude of these is essential for explaining the observed changes—something the climate models do not do well.

The TSD should have noted the significant impact that natural variability (not well-captured by climate models) has on local and regional climate and climate trends.

Page ES-2, lines 1-5

In much of the TSD, EPA describes the negative impacts that a lack of water will have in trying to meet American's growing needs. Why, then, doesn't the TSD would acknowledge that the 6% precipitation increase over the past century—a time when the EPA reports that temperatures have been increasing—is a largely positive and beneficial development. Instead, the TSD concerns itself with increases in heavy precipitation amounts, despite voluminous research showing that 1) along with increasing annual rainfall totals comes more rain from heavy events (Michaels et al., 2004), and 2) the rainfall increases have not led to increases in flood events (Lins and Slack, 1999; Downton et al., 2005; Small et al., 2006). Further, increases in heavy rainfall while annual total rainfall declines is not the nature of the precipitation frequency distribution.

The TSD should have acknowledged that the observed increases in rainfall are a net benefit in helping to meet the U.S. growing water demand and that appropriate measures should take advantage of the changes in the patterns of increased precipitation delivery.

Page ES-2, lines 7-9

Whether or not the observed increase in the recent rate of sea level rise represents a sustained change in the trend or a short-term variation is far from being well-established (e.g., Holgate, 2007; Wunsch et al., 2007; Willis et al., 2008). Further, Kolker and Hameed (2007) have identified a meteorological effect on the rates of sea level rise along the eastern seaboard, which has acted to increase the apparent rate of rise in recent decades.

The TSD is not based upon the most recent available science, which is counter to its assertions about sea-level rise.

Page ES-3, lines 4-6

The latest research results invalidate such strong language on tropical cyclones (e.g. Knutson et al., 2008; Vecchi and Soden 2007a, 2007b; Vecchi et al., 2008). The first reference projects a decrease in intensity so small that it will likely not be detectable, but with a major reduction in the number of storms making landfall in the U.S. Statement.

This very important point in the TSD has simply been invalidated by subsequent research; increases in tropical cyclone impacts can no longer be used to support limitations on carbon dioxide.

The rate of sea level rise is closely tied to the rate of global temperature rise, and the observed rate ,~0.17ºC/decade, has been well-established. This is near the low end of the IPCC range of projections, **implying a sea level rise also near the low end of the IPCC projected range, that is, a sea level rise closer to 9 inches than to 19 inches—or one which is similar to the rate of sea level rise that we have adapted to during the past century.**

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page ES-3, lines 33-37

TSD is wrong about heat-related mortality. EPA knows that the sensitivity to extreme heat events has been declining in major cities across the U.S. This result is only apparent when you correctly standardize the population for changing size and age-distribution. Without properly standardizing, it is impossible to compare the climate impacts from one period to another (or one place to another). This standardization also applies to the future. You just can't say, "well, we'll have more elderly in the future, and elderly are more sensitive to extreme heat, thus more people will die in the future because of extreme heat." While, in its simplest sense, this is true, it has nothing to do with the heat—as it depends only on the changes in the population. Temperatures could decline in the future

and still more people would die of the heat because there are more people vulnerable. Without standardizing the population for age and number, you cannot make useful determinations of the future effect of climate change on U.S. heat-related mortality. And once you do standardize, you find that extreme heat is having a lessening impact as we better adapt to its occurrence.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required.

Page ES-3, lines 39-45

How on earth can the EPA possibly support an IPCC claim that declining air quality is a "virtual certainty" in the U.S., when, partly because of their own efforts, air quality has been improving in the U.S. in a warming climate?

The IPCC report is just wrong. The TSD's use of something so obviously incorrect certainly weakens any arguments for an "endangerment" finding from carbon dioxide.

Page ES-3 (lines 47-51) and ES-4 (lines 1-7)

For the past 30, 50, or 100 years, agricultural yields have been increasing in the U.S., despite climate fluctuations and trends. This has occurred as farmers adopt the new technology, fertilizer, tillage regimes, crop varieties, irrigation, etc. that allows them to produce higher yields under prevailing conditions. Imported food crops (such as soybeans) are grown across the U.S. in a variety of local and regional climates. Advances in farming techniques and practices as well as improvements in crop varieties (including those genetically modified to resist pests and weeds) will add new tools from which to choose to best adapt to climate conditions. Assuredly, American's farmers will be able to keep up with the challenges that a changing climate may present them—they more than did so during the 20th century, and we should expect continued success in the future.

The TSD is wrong. A more sophisticated understanding of the adaptation response of agriculture to changing conditions (natural or otherwise) is required, which would obviate its use as a basis for a finding of endangerment.

Page ES-4, lines 9-12

Wildfire patterns across the U.S. involve complex interactions with local and regional climate related to sea surface patterns in both the Pacific and Atlantic Oceans that are primarily driven by natural oscillations such as the PDO and the AMO (McCabe et al., 2004). Thus, future patterns of wildfire are not as simple as temperatures go up, and wildfire occurrence responds, but instead involve the interplay between forest management practices, natural oscillations, and anthropogenic climate change.

The TSD is overly simplistic concerning the relationship between forest fires and climate change, and a more complete discussion would probably not support any finding of endangerment from carbon dioxide-caused warming with respect to forest fires in the U.S.

Page ES-4, lines 14-21

Despite the climate changes that EPA has identified that have occurred across the U.S. over the course of the 20th century—temperature increases, precipitation increases, sea level rise, etc…—**damages to coastal communities from tropical storms, once properly standardized to account for changes in coastal population, the wealth of the population, and inflation, display no long-term trend (Pielke et al., 2008).** True, the raw damage totals have increased dramatically and will likely continue to do so in the future, but this increase in not related to climate or climate change.

Table ES-3

Health sector

Why did EPA only choose to include the parenthetical "(but may be modulated by public health measures)" after the "expanded range of vector-borne and tick-borne disease"? It applies equally to all of the other bullets in this section. In fact, science has conclusively demonstrated that public health measures have successfully reduced our sensitivity to heat waves (Davis et al., 2003a; 2003b). Another clear example can be found from the low mortality impacts of Hurricane Ike in Houston-Galveston. Clearly, the U.S. populace has adapted over time to such phenomena by improving forecast technologies, building codes, evacuation strategies, etc. All of these adaptive measures are not going to suddenly vanish in the future. It goes without saying that "Without investments in countermeasures, hot temperatures and extreme weather are likely to cause increased adverse health impacts from heat-related mortality, pollution, storm-related fatalities and injuries, and infectious diseases." But no one thinks these countermeasures won't be pursued, do they? Primarily these countermeasures are required to best live in our *climate*—rather than climate change. All you have to do is roll back the clock a hundred years or so to see what kind of destruction our climate can bring upon us if we are unprepared or ill-adapted (e.g., Galveston hurricane 1900 (8,000+ deaths); Johnstown flood of 1889 (2000+ deaths), or the heat waves of 1901 (9,000+ deaths) or 1936 (4,000+ deaths). So, assuredly, "investments in countermeasures" will be pursued as they are an integral part of our evolving society.

The TSD completely misleading concerning adaptation to both climate and climate change.

Air Quality

Again, how can the EPA possibly claim that declining air quality is a “virtual certainty” when, partly because of EPA regulatory programs, air quality has been improving in the U.S. in a warming climate? The simple correction here is to replace "Declining" with “Improving” and “due to” with “despite.”

The TSD is completely wrong here, even denying the beneficial effects of previous EPA regulation.

Food Production and Agriculture

TSD displays a remarkable ability to overlook on-going trends that run counter to its projected negative impacts despite the fact that the trends have been on-going in concert with a changing climate. This is certainly the case with U.S. agriculture where technological advances result in continued increasing productivity as global temperature increased.

The TSD ignores the reality that American farmers are perfectly capable of adapting to a wide range of climate, and therefore to climate change.

Forestry

In general, U.S forests thrive as the length of the growing season increases and elevated levels of atmospheric CO2 concentration enhance photosynthesis and moisture-use efficiency. Arguing that increased forest biomass is bad because it provides more fuel for ultimate fires has a wonderful *reductio ad absurdam*, namely that we would obviously have no forest fires if we had no or just scrubby little trees, and that this would somehow be a good thing!

The response of forest fires and insect outbreaks is a complex climate interaction of involving temperature and precipitation patterns that are largely driven by changes in SST in the Pacific and Atlantic Oceans (e.g. McCabe et al., 2004). How these patterns are affected by future climate change and the downstream affect on U.S. forests is far from being firmly established, as climate models have clear difficulties in predicting the evolution of major oceanic teleconnection patterns (see comments on the Atlantic Multidecadal Oscillation, below).

Water Resources

What does “Climate change will constrain North American’s over-allocated water resources…” mean? If our water resources are over-allocated, this would imply that our current climate already doesn’t supply our needs. What does this have to do with climate *change*, epecially given that the U.S. total annual precipitation amount has been *increasing* over the past century as planetary surface temperature has warmed?

While many studies have shown that our annual precipitation increases have been accompanied by increases in heavy rainfall events (just as expected, e.g. Michaels et al., 2004), many other studies have shown that instead of being accompanied by more droughts, rather the timing of the increases have been responsible for increasing the streamflow during the lowest times of the year (Downton et al., 2005; Lins and Slack, 1999; Small et al., 2006; Small and Islam, 2007). These run counter to TSD's contention that we will experience longer periods of low flow. Also, there has not been any evidence that the U.S. has been experiencing more drought conditions. Again, the observed trends run counter to their expectations, despite occurring during a time with increasing human greenhouse gas emissions and corresponding climate change.

The impacts that EPA attributes to climate change simply are not identifiable, and the TSD is therefore an inappropriate support for any finding of endangerment.

Sea Level Rise and Coastal Resources

In this summary EPA refers to expectations of stronger coastal storms. The latest research results imply that this expectation should not apply to tropical cyclones (e.g. Knutson et al., 2008; Vecchi and Soden 2007a, 2007b; Vecchi et al., 2008). Further, it is less clear that a sustained acceleration of sea level rise has occurred in recent decades (e.g. Holgate 2007; Willis et al., 2008) or that high rates of sea level rise should be expected in the future (e.g. van de Wal et al., 2008; Joughin et al., 2008). In fact, the recent slowdown in the rate of global temperature rise will likely act to slow (at least temporarily) the rate of sea level rise, and pushes back the total rise for years to come. **As the rate of sea level rise is closely tied to the rate of global temperature rise, the observed rate of global temperature rise (~0.17ºC/decade) which has been well-established for three decades now and which lies along the low end of the IPCC range of projections, implies a sea level rise also near the low end of the IPCC projected range, that is, a sea level rise closer to 9 inches rather than to 19 inches—or one which is similar to the rate of sea level rise that we have adapted to during the past century.**

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

The case for increased "storm intensity" is a difficult one to make. Using the outdated IPCC AR4 as the basis for a hurricane argument is simply not keeping up with the current literature on the topic, as noted both above and below. With respect to non-tropical storms, there is simply no clear trend, as indicated by the AR4 (page 282, column 1).

An updated discussion of the relevant science is required in the TSD. As it stands now, it is insufficient for basing an endangerment finding.

Energy, Infrastructure and Settlements

EPA spends a lot of time worrying about the impacts of warming and coastal erosion on the native settlements in one of our least populous states. The native population of Alaska has been dealing with coastal erosion and periods of warming there not only during the past 100 years (e.g., McCarthy, 1953; Hume and Schalk, 1967; Hume et al., 1972), but undoubtedly for many thousands of years (e.g. MacDonald et al., 2000, Hu et al., 2001; Kaufman et al., 2004). As they reduce their nomadic lifestyles and move into more permanent settlements, their abilities to respond to the climate and climate variations, are greatly altered. Thus, most of the problems that they are facing are rooted in the transition from the "old" ways to new ones, rather than in climate *change*.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Section 4. Radiative Forcing and Observed Climate Change

Page 22, lines 11-13

Several recent studies continue to show that the local impact (a warming influence) of human activity has not been sufficiently removed from the global surface land record (e.g., McKitrick and Michaels, 2007; de Laat and Maurellis, 2006)

The TSD is not based upon the best and more recently available science here. An updated discussion of the relevant science is required.

Page 22, Figure 4.2 "Annual global mean temperatures with linear fits to the data"

If would be hard to draw up a more biased illustration of global temperature trends. Clearly the goal has been to make it seem like the warming rate has been increasing. **In fact, the warming rate from about 1910 to the early 1940 is precisely the same as the warming rate during the past 30 years. And in the intervening 30 years (mid-1940s to mid-1970s), current data sets indicate that the global temperature changed little.** This division is a much fairer representation of the global temperature record than is Figure 4.2.

Further, recent findings by Thompson et al. (2008) indicate that the temperature decline in the decades immediately following WWII is likely a result of changing observation practices. Efforts to remedy this situation seem to indicate that the temperatures during these post-WWII decades (through the early 1960s) will likely have to be raised. This will have the effect of *reducing* the temperature trend for the past 50 years—a reduction that will be further enhanced by the continued slowdown of the rate of global warming recorded in the 21st century.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it an insufficient basis for an endangerment finding.

Page 23, lines 1-6

While the rates given are correct and the statement that the warming rate during the past 50 years in nearly double the warming rate of the past 100 years is a true statement, this is an unfair assessment of the pattern of global temperature changes over the course of the past 100 years (1908-2007**). During this time period, global temperatures rose during the first third, cooled (or remained relatively steady) during the middle third, and rose again during the most recent third.** The rate of warming during the first third (from 1908-1942) was 0.16ºC/decade, the same rate as that during the most recent third (1974-2007). Comparing the rate during the last 50 years, with that during the past 100 years, is done to make it seem like the rate of temperature rise has been increasing recently compared with past changes. This demonstration relies on a judicious selection of start dates rather than on a fair assessment of global temperature patterns. **In fact, the rate of warming in recent decades is no different than that observed during an equal length period in the early 20th century.**

This biased presentation by in the TSD must be revised to better reflect the true nature of observed changes.

Page 23, lines 6-10

What is the point of this discussion about temperature rankings? You've already established that temperatures have been increasing over the past 100 years. It would seem odd if the end of the record didn't have the highest temperatures. If you rewrite this paragraph 10 years from now, you'll probably have to change the ranking of the warmest years around to reflect a continued rise in global temperature. Once you've established that temperatures have been rising over the period of record, there is little gained from saying the warmest years are at the end. If the warmest years weren't at the end, *that* perhaps this would be worth mentioning, otherwise you are stating the obvious—an unremarkable situation.

Correction requested: Remove this section as it is irrelevant and out-dated.

Page 23, lines 14-15

Several recent research papers have found economic signals in the land-based temperature records which suggest that the warming is over-represented is these datasets (e.g. de Laat and Maurellis, 2006; McKitrick and Michaels, 2007; Pielke et al., 2007). Correcting for these "non-climatic" signals will reduce the difference between land and ocean temperatures.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 23, lines 19-21

Arctic temperatures are known to vary to a much greater degree than global temperatures and thus have a propensity to exhibit trends of a larger magnitude. Throughout the past 100 years, the temperatures trends in the Arctic (both during warming periods and cooling periods) have exceeded the global values. Thus, the statement about Arctic temperatures (lines 19-21) is a commentary more about general climate conditions rather than climate *change*.

Correction requested: The TSD should be modified to reflect the nature of climate.

Page 23, lines 33-36. "Antarctica…[has] not warmed in recent decades, but it is virtually certain that there has been strong warming over the last 50 years in the Antarctica [sic] Peninsula area."

The TSD should note that this comprises roughly 2% of the Antarctic continental area. Why is this especially noteworthy? Readers who know Antarctic geography will sense this as an attempt to mislead others who do not. **The TSD should acknowledge how small the area is and explain why it is important to emphasize this in the face of different behavior of 98% of the continental region.**

The TSD is not scientifically complete here. An updated and more thorough discussion of the relevant facts is required. As it stands now, it is inappropriate for basing an endangerment finding.

Page 23, lines 35-36

What's the big deal about the 50-yr warming rate over the tiny Antarctic Peninsula such that it merits a specific mention in the section describing the warming rates since 1979? **Why not mention that although *Alaska* has apparently been warming rapidly during the past 50 years, that during the past 25 years, the warming there has been virtually nonexistent?** (see this site from the Alaska Climate Research Center at the University of Alaska-Fairbanks, http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html).

The TSD is misleading here. An updated and more thorough discussion of the relevant facts is required.

Page 23, lines 38-40

Although the TSD claims that the pattern of Antarctic temperature change has been successfully modeled in Baldwin (2007), this is a misleading statement. The primary citation in Baldwin (2007) is Gillett and Thompson (2003) from *Science*. It is very clear from Figure 3 in that article that Antarctic Peninsula warming is *underestimated by a factor of 3*. That is hardly a "strong" warming in the simulation—in fact it is between 0.01 and 0.15°C per decade, while the observed value given in the article is in excess of

0.31°C. Using the midpoint for the projected range implies an underestimation of *at least* 2.5 times what has been observed.

Correction requested: Note that Antarctic Peninsula warming has been substantially underestimated in the (erroneous) Baldwin citation (which should be Gillett and Thompson). As it stands now, it is inappropriate for basing an endangerment finding.

Page 23, lines 38-40

Much of the modeling of the Antarctic temperature (and attribution to global warming) relies on the changes to the SAM (Southern Annular Mode) index. Models suggest the global warming should lead to a positive trend in the SAM index—something that has been observed from the late 1960s through the late 1990s. However, the SAM record is not of sufficient length to judge these changes against natural variations. Recent work on reconstructing the SAM index into the 1800s reveals historic swings in the SAM index that are not unlike those of recent decades (Jones and Widmann, 2004). Further, the positive mode of the SAM seemed to have peaked in the late 1990s, and has trended back downward towards zero ever since (see http://polarmet.mps.ohio-state.edu/acd/sam/sam_recon.html). Thus, the behavior of the SAM index does not suggest that anthropogenic global warming has had much to do with the lack of warming in Antarctica in recent decades. **This is further supported by recent research by Karpechko et al. (2008)—which concludes that ozone depletion, rather than greenhouse gases are the primary contributor to recent SAM increases. This finding was published subsequent to the IPCC AR4 and Baldwin et al. (2007) and thus was not included in this assessment.**

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 24, lines 1-6, specifically "both the surface and troposphere have warmed, while the stratosphere has cooled. These changes are in accord with our understanding of the effects of radiative forcing agents and with the results from model simulations."

Douglass et al., (2007) demonstrated that there is a significant difference between temperature changes projected by the suite of IPCC models and those observed in the upper troposphere. This difference is important (because of lapse-rate considerations) and *not* in accord with model simulations. Very recently, Santer et al. (2008) have attempted a refutation of Douglass et al., but their work used much different (and more unconventional) statistical analyses and simply disregarded the now-corrected University of Alabama-Huntsville microwave data lieu of one much less cited, but authored by one of the papers' 17 "coauthors."

Correction requested: Note the controversy and cite both Douglass et al. and Santer et al., leaving the readers the very clear impression that this discrepancy is far from

settled. Updated science, rather than the TSD, will not support a finding of endangerment, as there is a clear discrepancy between the models and observations.

Page 24, line 25

What does "qualitative agreement" mean. In science, "agreement" means *quantitative* agreement. If they are merely of the same sign, but different magnitudes via standard statistical differencing techniques, then they are not in agreement.

Correction requested: Use more precise, scientific language. If the magnitudes are indeed different, there is very little scientific support for an endangerment finding.

Page 24, lines 24-26

EPA mentions the per decade rate of stratospheric cooling, but fails to mention that this rate has slowed greatly during the past decade and a half. In fact, there has virtually been no temperature trend in the satellite datasets of stratospheric temperatures since the mid-1990s (see http://www.ssmi.com/msu/msu_data_description.html#msu_amsu_time_series, or the IPCC AR4, Figure 3.17). If the stratospheric temperature trend since 1979 is worth mentioning, then so is the recent behavior. More important, how does this support an endangerment finding?

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 26, Section 4(c)

In the U.S., the recent temperature behavior is much more complex what the short summary here makes it out to be. A big jump occurred with the 1998 El Niño which has the impact of increasing the apparent rate of warming over the past 30 years, despite the fact that over the past 10 years (since the step up from the 1998 El Niño) the temperatures have not risen at all. In fact, they show signs of recovering to their pre-1998 levels. Given time, such a recovery will significantly lessen the apparent rate of temperature rise in the U.S**. Statements about the last 30 years that do not note this are incomplete; the observed behavior is hardly alarming and isn't sufficient to met the criteria for endangerment.**

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 27, Figure 4.4

The EPA keeps showing trends since 1901 which include a warming period into the early 1940s, a cooling from the mid-1940s to the mid-1970s, and another warming trend thereafter. Is the behavior of the first two periods related to anthropogenic climate change in the U.S.? If not, why include these data in the trend analysis? The behavior during the first two periods does go to show that predominately natural variability can be quite large over the U.S. It will be interesting to see over the next few years whether the national temperature continues it decline towards its pre-1998 level. If so, it will say a lot about the role that natural variability (such as the 1998 El Niño and the 1976-77 PDO shift) plays in the U.S. average temperature.

The TSD is quite misleading here and needs to be modified to discriminate between natural and anthropogenic warming.

Page 28, bullet 1

This analysis starts in 1900. Were the changes that occurred prior to the past 30-50 years attributable to anthropogenic climate change? If not, why include that period in the trend analysis. If so, is EPA suggesting that the Dust Bowl was a result of the U.S. greenhouse gas emissions?

Here is an idea. Present the trends for some 30-year periods during 1900 though, say, 1975 as representing predominantly "natural" variability, and then compare them with the trends during the past 30 years. In this way, we can see whether the trends during the "anthropogenic" era depart from the "natural" patterns and magnitudes of precipitation variability. Lacking such an analysis, EPA is representing all changes that have occurred as a result of anthropogenic climate change—which is simply not the case.

I have done this analysis for you for the U.S. The histogram below (Figure 1) shows the distribution of 30-yr trends in U.S. precipitation (data from NCDC) starting in the period 1895-1924, then 1896-1925, 1897-1926, …and ending in 1978-2007. The trend during the most recent 30 years is a negative 0.014 inches/year which falls near the middle of the distribution (within 1 standard deviation of the mean). **There is absolutely no indication that U.S. greenhouse gas emissions have produced an unusual situation when it comes to U.S. annual precipitation totals and/or trends. The TSD is not correctly representing the secular behavior of precipitation.**

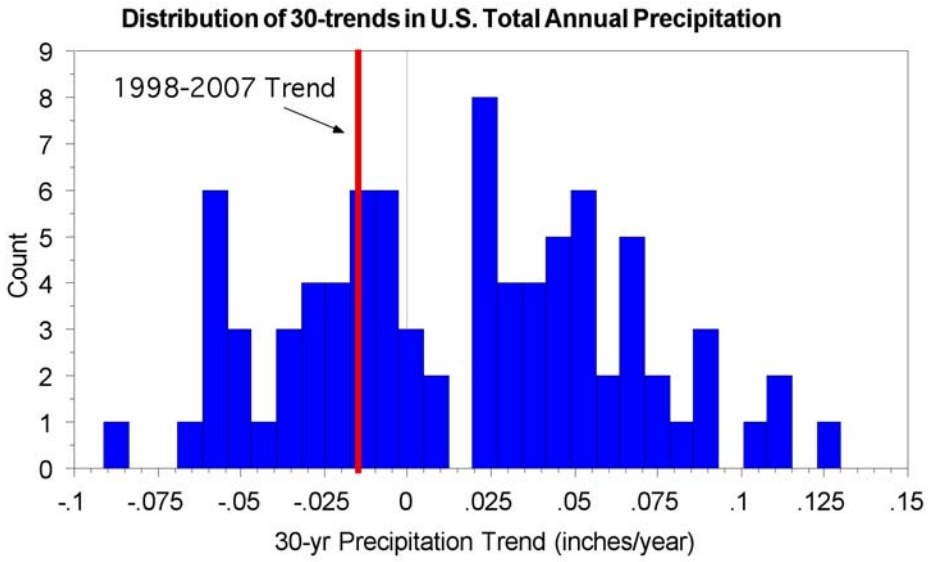


Figure 1. Distribution of 30-yr trends in U.S. annual precipitation, 1895-2007 (data source, NCDC).

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 28, lines 7-8 "More intense and long droughts have been observed…"

More intense and longer than what? In the Southern and Southwestern U.S. (subtropics), overwhelming paleo-evidence shows that droughts occurred in the past 1,000 years that have dwarfed any modern droughts both in terms of intensity and duration (see http://www.ncdc.noaa.gov/paleo/drought/drght_home.html for example, or Seager et al., 2008). **So your general statement is wildly incorrect.**

The TSD is scientifically incorrect here. An updated discussion of the appropriate science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 28, lines 14-17

The places where precipitation has been decreasing and yet the increases in heavy precipitation events have been increasing are few and far between—and in most of those regions, the result is not a robust one. After all, how long can such a situation exist? You can't get more heavy rain events and less total rain for very long—it is a physically/mathematically non-sustainable situation. In the U.S. our research has shown that while heavy rainfall has been increasing, it is increasing just as it is expected to do so under a situation where annual precipitation is increasing (a situation found throughout much of the U.S.). There is nothing whatsoever that is "disproportionate" about these changes. We find that while the *total* rainfall falling in heavy events has increased, the percentage of annual rainfall falling from such events is unchanged. See Michaels et al.,

2004—an appropriate and applicable published result for some reason ignored by the IPCC AR4.

Further, recent work by Small and Islam (2007) found that U.S. precipitation increases were found primarily in the fall season in the central U.S. and that they resulted from changes in the number of events, not in the intensity of the events and that any trends were related to decadal variability rather than slowly evolving changes in one direction. This result was also not included in the IPCC AR4.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 29, lines 35-37

"…which is indicative of significant variability in regional precipitation patterns in time and space" as is naturally, and always has been, the case.

Correction requested: The TSD should be modified to include a description of natural climate.

Page 30, lines 4-5

The reason for the uncertainty in whether the recent rate of sea level rise is "a reflection of short-term variability or an increase in the long-term trend" is that similar rates of sea level rise have been observed at various times during the 20th century, interspersed with times of relatively low sea level rise. In fact, Holgate (2007) concluded that:

> "Extending the sea level record back over the entire century suggests that the high variability in the rates of sea level change observed over the past 20 years were not particularly unusual. The rate of sea level change was found to be larger in the early part of last century (2.03 ± 0.35 mm/yr 1904–1953), in comparison with the latter part (1.45 ± 0.34 mm/yr 1954–2003)."

This finding by Holgate (2007) was published too late for inclusion in the IPCC AR4 report and thus was not considered in your Bindhoff et al., 2007 reference, which weakens support for a finding of endangerment.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 30, lines 18-25

In another paper that was published subsequent to the Bindhoff et al., 2007 references, Willis et al. (2008) find that in recent years (mid-2003-mid-2007), thermal expansion (-0.5mm/yr +\- 0.5mm/yr) and ice melt (0.8mm/yr +/- 0.8mm/yr) together contribute far less than the sea level rise rate measured by satellites (3.6 mm/yr +/- 0.8mm/yr) over the same period. The authors suggest that the discrepancy may be related to errors in any of the three measurement systems, and that it is important to get that figured out in order to get a good handle on the rate of recent sea level rise. The point being, that satellite altimeters are, as of yet, far from determined to be the end all and be all for data on the rate of sea level rise. They, too, may include sources of errors derived from how they are processed (and changes in how they are processed).

The bottom line is that an acceleration in sea level rise in recent decades is hardly an established fact.

The TSD is not based upon the complete available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 30, lines 40-43

Two studies published after the IPCC AR4 report was released indicate that processes other than a global sea level rise from increasing temperatures are manifest in the tide gauge readings along the Atlantic coast of the U.S. Wöpplmann et al. (2007) used a better model of crustal movement (based on GPS benchmarks) and find that the apparent sea level rise should be reduced for many U.S. coastal locations, and Kolker and Hameed (2007) concluded that atmospheric circulation variations (associated with the fluctuation in the North Atlantic Oscillation) were responsible for a significant portion of the observed sea level rise in the North Atlantic. In fact, when they accounted for the effects of changing patterns of circulation they found that the residual sea level rise in the North Atlantic (since 1900) is less than 1 mm/yr, and these patterns of atmospheric variations also explain why the observed rate of sea level rise is greater in the Western Atlantic than the Eastern.

Such results, not included in the IPCC AR4, need to be considered when trying to assess the impact of anthropogenic global warming on the rate of sea level rise, as they indicate less of an impact than is currently reflected in this Technical Support Document.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 32, lines 1-2

"Antarctic sea ice extent, however, shows no statistically significant average trends (IPCC 2007d)."

This statement is completely false. There is a statistically significant upward trend in the monthly sea-ice anomaly data for the southern hemisphere (Figure 2). Data from the National Snow and Ice Data Center (http://nsidc.org./data/seaice_index), from November 1978 through October 2008 has a statistically significant (p<.0001) upward trend of 12,000 square kilometers per year.

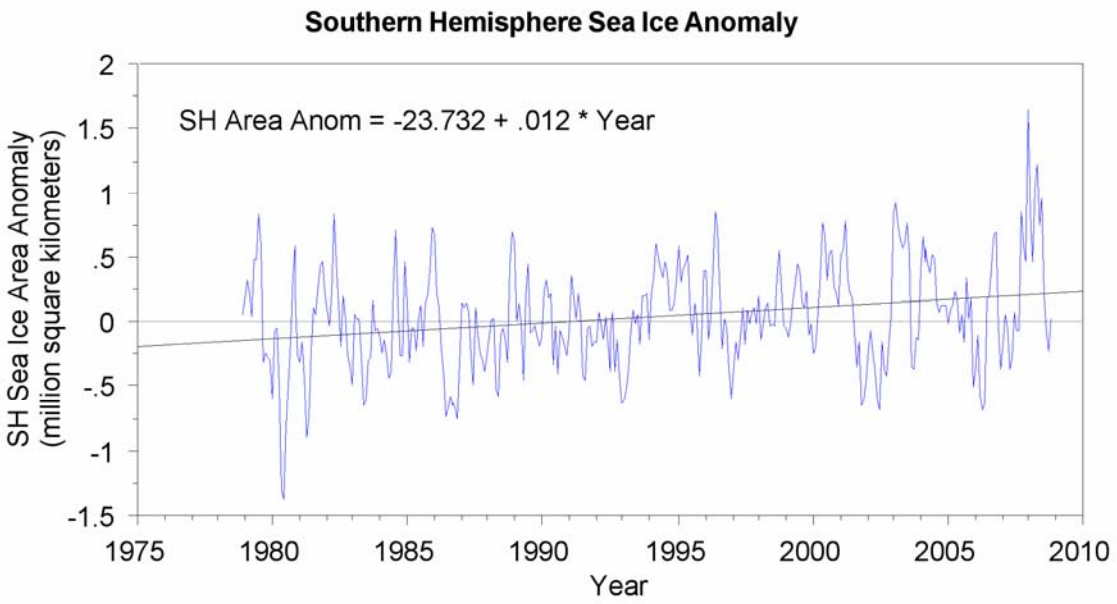


Figure 2. Monthly Southern Hemisphere sea ice anomalies and best-fit linear trend line (data source: NSIDC).

This blatant error demonstrates the weakness of the TSD synthesis as a basis for the ANPR.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 32, lines 12-14

The melting of glaciers in Alaska has much of its grounding in the recovery from the cold conditions (and expanded glacial extent) of the 19th century as well as in the periodic climate cycles related to the Pacific Decadal Oscillation. The positive PDO phase which began in 1976-77 has been linked to higher temperatures across Alaska (Figure 3) (Hartman and Wendler, 2005), as well as to temperature and moisture patterns in the northwestern U.S. (including those causing glacial recession at Glacier National Park; see Pederson et al., 2006). Yes, glaciers have been melting in Alaska and the U.S. northwest, but no, anthropogenic climate change is not the sole contributor—nor is it clear what the true level of that contribution is.

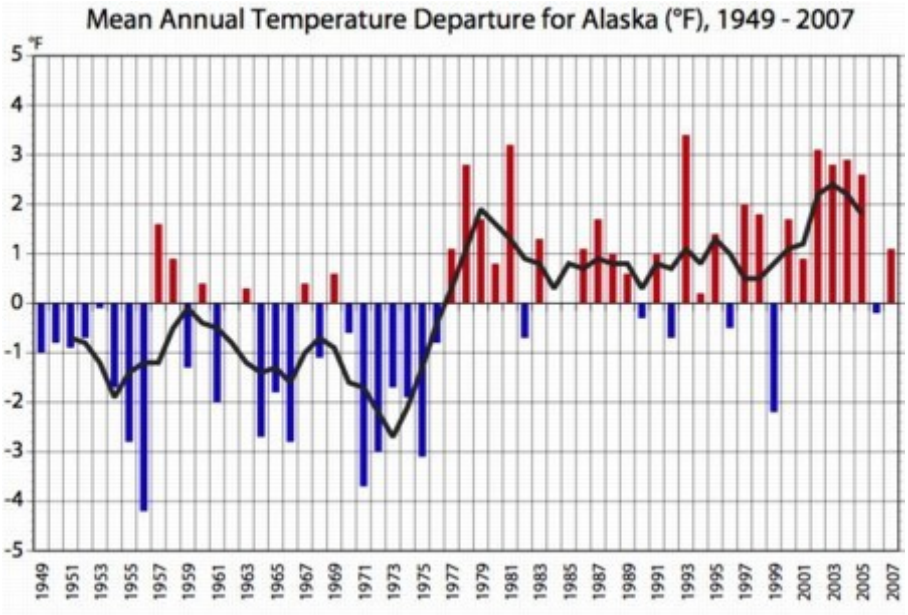


Figure 3. Mean annual temperature anomalies (ºF) for Alaska (figure from the Alaska Climate Research Center, University of Alaska-Fairbanks, http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html)

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 32, line 23

Again, concerning permafrost melting in Alaska. See the above chart on the Alaska temperature history, bearing in mind that the 1976-77 step up in temperature is contemporaneous with the shift in the PDO from negative to positive phase—i.e. not anthropogenic global warming.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Pages 32-34, Sections 4(h) and 4(i)

There is little contention that the global temperatures, on average, has been rising for the past century including the past several decades. Therefore, we should expect changes in physical and biological systems. When it warms, ice melts, and plants and animals redefine their ranges and habitats. If the biosphere didn't respond this way, it is likely that most life on earth would have taken a completely different form from the one we know.

Thus, it is good news to observe the biotic response that you describe because it is indication that despite all our activities that have resulted in large and small-scale range fragmentations, plants and animals are still finding the best ways to adapt to climate

changes, be they largely natural (as in the first 2/3rds of the 20th century) or largely anthropogenic (recent changes). How does this serve to support a finding of endangerment?

Correction requested: The TSD should be modified to reflect the on-going positive, adaptive responses to climate change throughout the biosphere.

Page 35, line 2

As mentioned previously (see comment, Page 28, lines 14-17) the places where precipitation has been decreasing and yet the frequency in heavy precipitation events has been increasing are few and far between and represent the exception not the norm—after all, how long can such a situation exist—you can't get more heavy rain events and less total rain for very long—it is a physically/mathematically non-sustainable situation. These results are not robust and should not be repeatedly mentioned. According to IPCC AR4 Figure 3.39 (bottom), there are also places where trends in extreme precipitation are less than trends in overall precipitation ("minus" signs on AR4 Figure 3.39)—which, I guess would indicate a less extreme precipitation climate, yet such occurrences are never mentioned (and I would bet that there are more places with a moderating precipitation climate than places that have a robust signal of more extremes and less total precipitation). And for the places that are receiving more precipitation and more extreme events—this is generally how precipitation increases are manifest—more total rain is most accompanied by more extreme rain—thus the changes are not disproportionate to expectations (see Michaels et al., 2004 for a more thorough examination of the common misapplication of the term "disproportionate" as applied to precipitation changes).

The IPCC AR4 is unsettled on this issue. Delete as credible support for the TSD in this area.

Page 35, lines 11-13

The statement about the increases in the potential destructiveness of hurricanes is not nearly qualified enough. There are as many papers disputing the global existence and/or magnitude of such a trend (e.g., Landsea, 2005; Landsea et al., 2006; Klotzbach, 2006; Landsea et al., 2006; Swanson, 2007) as there are those suggesting the existence of one. Further, new papers have been published showing that even if an increasing trend did exist it is not of the character as that projected by climate models to accompany anthropogenic greenhouse gas increases (Knutson et al., 2008).

In fact, the topic of global warming and hurricane patterns is an active and fast evolving field, and many of the most recent findings do not draw as close a linkage between anthropogenic climate changes and increasing hurricane frequency and/or intensity as those which served as the basis for your "Trenberth et al., 2007" reference. These include, but are not limited to:

Briggs, W.M. 2008. On the changes in the number and intensity of North Atlantic tropical cyclones. *Journal of Climate*, **21**, 1387-1402.

Knutson, T.R., et al., 2008. Simulated reduction in Atlantic hurricane frequency under twenty-first-century warming conditions. *Nature Geosciences*, doi:10.1038/ngeo202

Wang, C., & Lee, S.K. (2008). Global warming and United States landfalling hurricanes. *Geophysical Research Letters, 35*(1), L02708.

Kossin, J.P., & Vimont, D.J. (2007). A more general framework for understanding Atlantic hurricane variability and trends. *Bulletin of the American Meteorological Society,88*(11), 1767-1781.

Landsea, C.W. (2007). Counting Atlantic tropical cyclones back to 1900. *EOS: Transactions of the American Geophysical Union, 88,*

Latif, M., Keenlyside, N., & Bader, J. (2007). Tropical sea surface temperature, vertical wind shear, and hurricane development. *Geophysical Research Letters, 34*(1), L01710.

Nyberg, J., Malmgren, B.A., Winter, A., Jury, M.R., Kilbourne, K.H., & Quinn, T.M. (2007). Low hurricane activity in the 1970s and 1980s compared to the past 270 years. *Nature, 447*(7145), 698-701.

Vecchi, G.A., & Soden, B.J. (2007). Effect of remote sea surface temperature change on potential tropical cyclone intensity. *Nature, 450*(7172), 1066-1070.

Vecchi, G.A., & Soden, B.J. (2007). Increased tropical Atlantic wind shear in model projections of global warming. *Geophysical Research Letters, 34*(8), L08702.

Vecchi, G. A., 2008. Whither Hurricane Activity? *Science*, **322**, 687-689.

Any reliance on "Trenberth et al., 2007" to base any endangerment finding is grossly unwarranted and out-of-date.

The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 35, lines 38-43

This analysis does not give a complete picture of the history of precipitation changes across the U.S. In research that we performed (Michaels et al., 2004) we showed that while precipitation on the wettest day of the year for stations across the U.S. (roughly the wettest few percents of rain events) was increasing, we showed that it was not increasing faster than the total annual precipitation (Figure 4). In other words, greater annual precipitation totals are accompanied by greater annual precipitation from the wettest days

of the year—but not disproportionately so. There was no general indication of a trend towards a more extreme precipitation climate in the U.S., allowing for the fact that precipitation itself has increased.

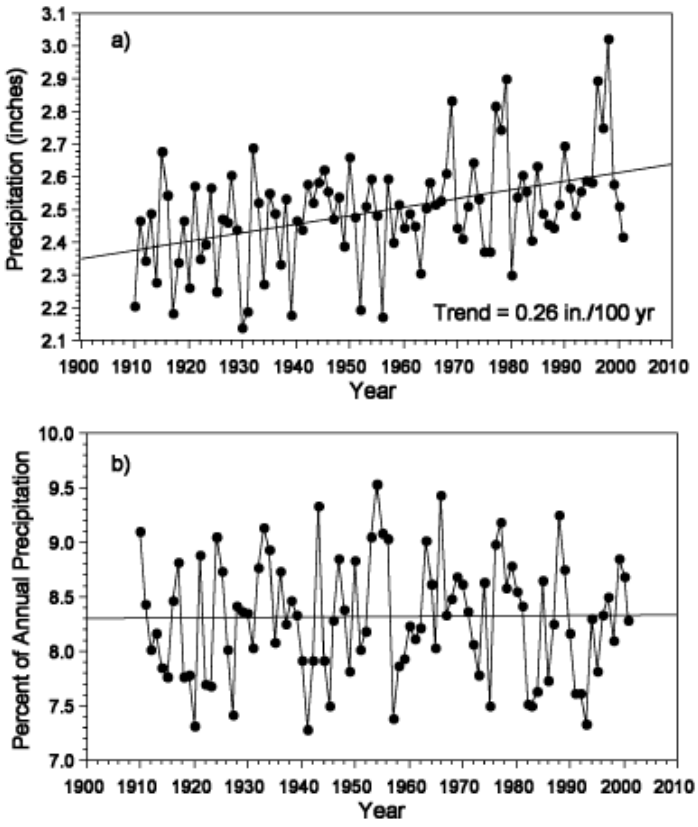


Figure 4. Top: Trend in the average precipitation falling in the wettest day of the year (averaged across the U.S.). Bottom: Trend in the average annual percentage of precipitation falling on the wettest day of the year. (source: Michaels, et al., 2004).

Also, note the recently published results of David Small and colleagues (Small et al., 2006; Small and Islam, 2007) which show that changes in recent U.S. precipitation patterns are related to decadal variations rather than long-term changes, that they are primarily confined to the fall season over large portions of the country, and that they are driven by changes in frequency rather than in intensity.

Somehow these results were not included by your "Trenberth et al., 2007" reference. **Thus "Trenberth et al., 2007" does not represent the true state of the science on the issue of the nature of precipitation changes across the U.S.**

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 35, line 48-49

"Multidecadal fluctuations" have been demonstrated to have been occurring long before anthropogenic global warming could have caused them—meaning that they are (including the recent ones) largely (if not entirely) of natural origin. This fact should be included in this summary.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 36, line 1-5

As mentioned previously, the state-of-the-science on this issue is rapidly evolving such that the general findings from your reference "IPCC, 2007d" have largely been superceded. SSTs are not the only factor in determining tropical cyclone intensity. Recent research by Vecchi and Soden (2007a, 2007b) illuminate how changes in the *patterns* of SSTs can influence wind shear in the main development region of Atlantic tropical cyclones—and suggest that future changes may act to reduce tropical cyclone intensity. Knutson et al. (2008) recently concluded that (1) the recent increase in tropical cyclone activity in the Atlantic Ocean does not appear to be consistent with assumptions that anthropogenic global warming was a primary factor; (2) future anthropogenic warming is anticipated to *reduce* the frequency of tropical storms and hurricanes throughout the Atlantic basin; and (3) the decrease in hurricane and tropical storm occurrence is greater in the western half of the Atlantic basin (rather than the eastern half). Consequently, the simulated number of hurricanes making landfall decreases by a greater percentage than the number of hurricanes itself. Knutson and colleagues summarized their results this way: "Our results do not support the notion of large increasing trends in either tropical storm or hurricane frequency driven by increases in atmospheric greenhouse-gas concentrations."

And more recently, Vecchi et al. (2008) describe how it is unclear whether Atlantic tropical cyclone activity patterns are related to absolute changes in the SST of the tropical Atlantic, or rather to relative changes in the SST of the tropical Atlantic compared to the tropical oceans as a whole. This distinction has great import for current attribution and future projections of Atlantic tropical cyclone activity. If the former is the case (the situation described in the TSD) anthropogenic emissions of greenhouse gases could be expected to increase Atlantic tropical cyclone activity, if the latter is the case, then there won't be much of an impact at all as a result of anthropogenic greenhouse gas emissions, because climate models do not project Atlantic SSTs to increase relative to other tropical ocean basins (see Figure 5). Vecchi et al. (2008) go on to present physical-based arguments as well as dynamically driven hurricane models (of smaller spatial and temporal scales than GCMs) to lend support to the latter relationship.

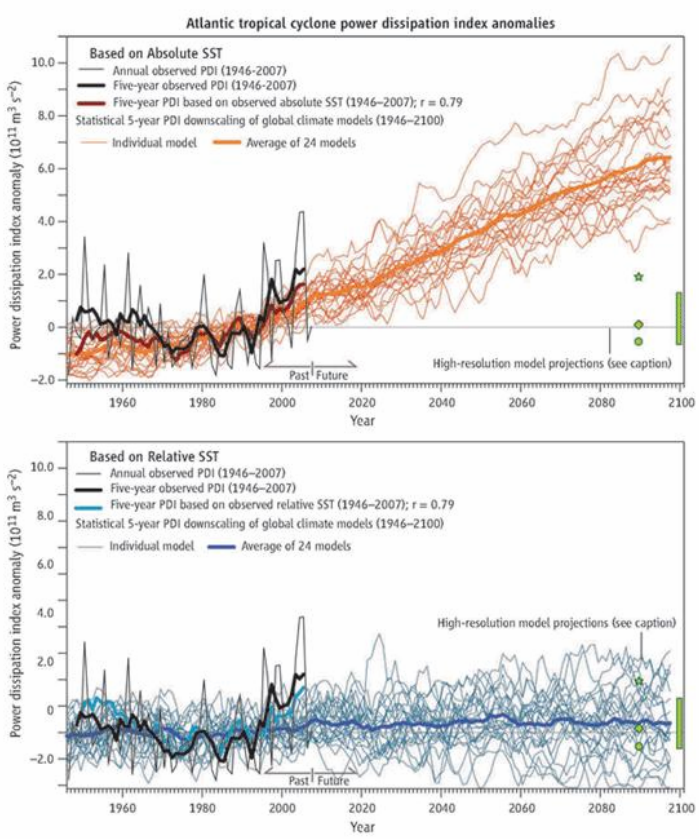


Figure 5. Observed tropical cyclone activity in the Atlantic basin, 1946-2007 (black lines) and the fit to absolute tropical Atlantic SST (thick brown line, top) and relative tropical Atlantic SST (thick light blue line, bottom). Climate model projections to the year 2100 based upon the observed tropical cyclone/absolute SST relationship (orange lines, top) and observed tropical cyclone/relative SST relationship (blue lines, bottom). The projections made by high resolution dynamic hurricane models are indicated by the green symbols on the right of each chart (see Vecchi et al., 2008 for additional details).

It is imperative that these post-IPCC AR4 results be taken into consideration in the Endangerment finding, for without doing so, and in relying primarily on the IPCC AR4 results, you will have based a decision on out-dated science which does not fully or accurately describe our best current understanding of U.S. greenhouse gas emissions and their potential impact on Atlantic tropical cyclone activity.

The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Section 5. Attribution of Observed Climate Change to Anthropogenic Greenhouse Gas Emissions at the Global and Continental Scale

Page 39, Figure 5.2 and Page 40, lines 6-8

On subglobal scales, such as North America, the models failure to capture the timing of internal natural variations that can effect the attribution of observed patterns of climate change.

Across many parts of western and northwestern North America, the switch from a negative phase of the PDO to the positive phase of the PDO in 1976-77 had a large impact on the region's temperature. This would clearly represent a known "natural" variation, but such a known signal does not show up in the blue portion of Figure 5.2, which is supposed to represent models output using natural forcings. Thus the true degree of natural variation, even at the decadal mean level depicted in the Figure, is larger than is reflected in climate model output. Further, the red portion of Figure 5.2 representing the combined effects of natural and anthropogenic forcings, is the result of tweaking the models such that the modelers produce the desired fit. This can be done by adjusting the degree and timing of the cooling influence of atmospheric aerosols such that it produces the desired fit on a model-by-model basis (see for example, Kiehl, 2007). Thus the red area can largely be tuned to whatever shape the modelers like and thus does not represent a true test of the models' hindcast abilities.

An updated discussion of the relevant science concerning the regional inabilities of climate models in replicating observed local and regional changes is required.

Page 40, lines 14-17

By mentioning the 2003 European heatwave (which is an event that is in everyone's mind as a result of the large loss of life accompanying it) the TSD creates a phenomenal mis-impression. **Increasing summer heat does not, in the long run, increase human mortality.** This has been demonstrated in the United States by papers by Davis et al. (2003a, b). And the population's rapid adaptation to heat has been demonstrated after the 1995 Chicago heat wave by Palecki et al. (2001) and across Europe after the 2003 event (Fouillet et al., 2008). So, it is one thing the state that summer extreme temperatures may be related to global warming, it is another to relate them to individual events (with a negative connotation) without then elaborating more fully. **This incomplete analysis in the TSD weakens it as the basis for an endangerment finding.**

Page 40, lines 27-29

In research published subsequent to "Hegerl et al. 2007" (in IPCC AR4) an analysis of the climate models incorporated into the IPCC AR4 shows that the modeled behavior of the Southern Annual Mode is far more influenced by ozone depletion than by global warming—which is shown to have little impact on the trends (Karpechko et al., 2008). Thus, Hegerl et al., 2007 does not represent the true and current state of the science.

The TSD is not based upon the best available science here as the IPCC AR4 is out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 40, lines 31-32

In research published subsequent to "Hegerl et al., 2007" (in IPCC AR4) Gerten et al. (2008) shows that precipitation over the global land area as been highly variable of the 20th century, characterized by a large degree of interdecadal fluctuation (Figure 6). Thus, it is extremely difficult to assign a cause to recent land precipitation trends when trends of similar magnitude and duration have occurred throughout the 20th century. In fact, virtually the entirety of the 20th century trend is dominated by fluctuations that occurred prior to the 1980s (i.e., not largely impacts by greenhouse gas emissions).

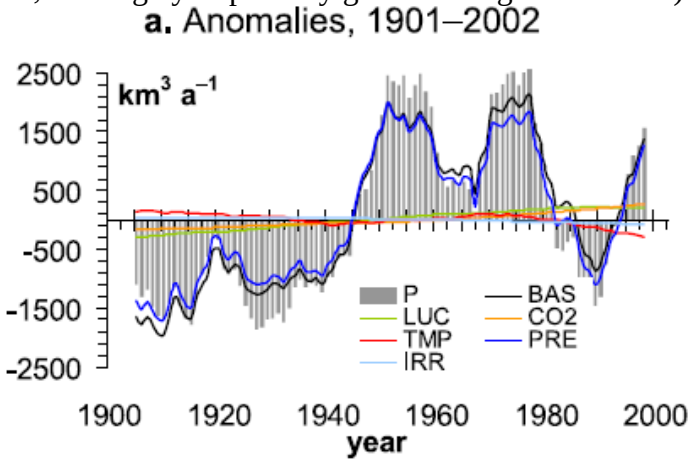


Figure 6. Fluctuation in river discharge (grey bars) have largely been influenced by fluctuations in precipitation (blue lines) over the 20th century. Interdecadal variability is large and dominates the overall trend (figure from Gerten et al., 2008).

And, while Zhang et al. (2007), concluded that they had detected an anthropogenic influence on the sign of overall latitudinal patterns of precipitation trends (although not in the magnitude of the trends), in the latitude band that included the majority of the United States, a mismatch between model projections and precipitation trends was found (model predicted a downwards trend while observations show and upwards trend) (Figure 7).

Figure 7. Latitude bands of the earth where observed precipitation trends (1925-1999) are of the same sign as model predicted trends. Green shading means increases in both observations and models, yellow shading means decreases in both observations and models, white areas are regions with insufficient observations, and gray area (which include most of the U.S.) are areas in which the observed trends and the modeled trends were of opposite signs (figure from Zhang et al., 2007).

Thus, it is unclear how well, if at all, the summary contained in lines 31-32 applies to the United States. **Again, the TSD is behind the times and shows itself to be an inadequate support for an endangerment finding.**

The TSD is not based upon the best available science here as the IPCC AR4 is out-of-date on this topic.

Page 40, lines 34-39

The caveats in lines 35-39 are well placed, however the statement in lines 34-35 that "It is more likely than not that anthropogenic influence has contributed to increases in the frequency of the most intense tropical cyclones" is no longer supportable based upon scientific findings that have appeared subsequent to "Hegerl et al., 2007" (in the IPCC AR4).

Primary among these references is Knutson et al., 2008, which concluded that:

> Our results using the ensemble-mean global model projections are inconsistent with the notion of large, upward trends in tropical storm and hurricane frequency over the twentieth century, driven by greenhouse warming.

A collection of the literature published subsequent to Hegerl et al. 2007 would produce a much different conclusion and lessen the likelihood to which recent increases in the frequency of extreme tropical cyclones (if there is a global increase at all; see for example Klotzbach (2006) for contrary evidence) reflect an influence from anthropogenic climate warming.

Again, the TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 40-41, Section 5(b)

The general comments made on Pages 32-34, Sections 4(h) and 4(i) apply here as well.

There is little contention that the global temperature, on average, has been increasing for the past century including the past several decades. Therefore, we should expect changes in physical and biological systems. When it warms, ice melts, and plants and animals redefine their ranges and habits. If the biosphere didn't respond this way, it is likely that most life on earth would have taken a completely different form from the one we know.

Thus, it is good news to observe the biotic response that you describe because it is indication, that despite all our activities which have resulted in large and small-scale range fragmentations, plants and animals are still adapting to climate changes, be they largely natural (as in the first 2/3rds of the 20th century) or largely anthropogenic (recent changes).

The TSD should be modified to reflect both adaptive and negative responses to climate change throughout the biosphere.

Page 41, line 35-36

Why limit your discussion to "a few local" disappearances when surely there have been "local appearances" of species as well. Say for example, all the hype about robins in the Arctic? Another example is the expanding northward range of Caribbean reef corals into the Gulf of Mexico and southern U.S. Atlantic coasts (Precht and Aronson, 2004). Isn't this a sign that things are perhaps getting better for robins and probably some other species as well?

The TSD should be modified to reflect both adaptive and negative responses to climate change throughout the biosphere.

Section 6. Projected Future Greenhouse Gas Concentrations and Climate Change

Page 42, Lines 28-29, "The IPCC SRES scenarios do not explicitly account for implementation of the Kyoto Protocol"

An unsophisticated reader might come away with the impression that Kyoto would actually have done something about global warming, but it is clear from Wigley et al., (1998), that, if adhered to, the Protocol would have eliminated 0.07ºC of warming by 2050 (and 0.14 by 2100), amounts too small to measure accurately.

Change the sentence to read, "…Kyoto Protocol, because that instrument would have done nothing measurable about global warming on the century time-scale."

Page 44, Figure 6.2 and related text

The same comment on the Kyoto Protocol applies to lines 43-46, so the text should similarly state that the reason it was not in the scenarios is because it has no effect measurable effect on warming.

More generally, some notation needs to be made on the simplicity of approach. MiniCAM is clearly just a straight line. Is there any reason to believe this? If so, it should be stated, because that obviates the need for any expensive, complicated emission scenarios or modeling.

Figure 6.1, Page 42; Figure 6.5, Page 48.

Methane concentration increases began to slow in the 1980s and have actually declined in some recent years (Dlugokencky et al. (1998); Schiermeier (2006). Every SRES projection (though less-so for the highly improbable B1) made by the IPCC assumes methane increases at least the pre-1980 rate at least through 2040.

As of this writing, a new paper by Rigby et al., (2008) reports on an uptick in global methane emissions in 2007, but the cause of this uptick, and whether or not it represents the re-establishment of an upward trend, or simply interannual variability has not been established. Their observations of a simultaneous increase of similar magnitudes in both hemispheres remain unexplained.

The general state of the science invalidates lines 25-26, page ES-2. "The radiative forcing associated with the non-CO2 GHGs is still significant and <u>growing</u> (emphasis added) over time. This is clearly not the case for methane, and the text needs to be modified to accommodate this reality, which also has important policy implications. It should have said "some" non-CO2 GHGs are growing, and note that methane, and important heat trapping gas has stabilized or even declined in recent years. Obviously this makes the TSD appear to be inappropriate to support an endangerment finding.

Page 51, Figure 6-8 (also Page ES-2, lines 31-37)

The TSD projects various ranges of temperature change for the 21st century on page ES-2, lines 31-37. The basis for these is given in Figure 6.8 (page 51). Figure 6.8 actually derives from the multi-model standard deviations for the (usually) 21 models input to each SRES scenario. These are given in particular for scenarios B1, A1B, and A2 in the IPCC AR4 page 763. The midrange scenario, A1B, is reproduced below (Figure 8).

***IPCC no longer has a defensible model basis because of recent and projected global temperature anomalies*. If IPCC has none, then the CCSP report, heavily cited as source material, has none, and therefore the TSD has none.**

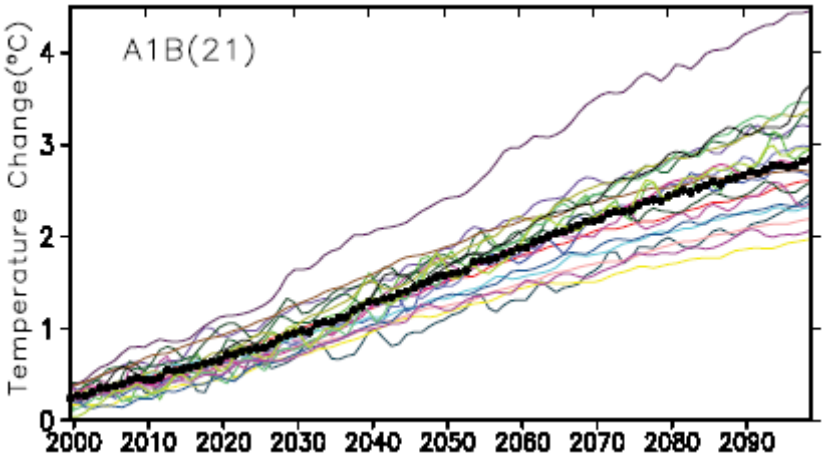


Figure 8. Warming projected by various models and the average (black line), IPCC midrange emission scenario (Figure source: IPCC AR4).

2008 marks the eleventh year without a net warming trend. While this is in part because of the large El Niño in 1998, IPCC claims that its models produce ENSO cycles. Consequently, the secular behavior of the IPCC models should be comparable to the secular behavior of climate.

However, even removing 1998 and the subsequent La Niña response (1999 and 2000), still yields no warming for eight years. One has to go back prior to the beginning of the second warming of the 20th century (pre-1975) to find such a string.

There is an additional problem originating in Keenlyside et al. (2008), which indicates there may be no additional warming until the middle or the latter part of the next decade. Note that *not one* of the models used in the midrange scenario in AR4 (or in the other two that the IPCC illustrated) has a 15-20 year period with no net warming.

The implications of course, are manifold. First, the models must, in general, be predicting too much warming. Holding temperatures constant for 1-2 decades obviously delays any oceanic water vapor feed back for even longer. While Schlesinger and Zhang give an oceanic thermal lag of around 60 years, even a lag of less than half that value

means that a 15-year hiatus in warming has devastating consequences for 21st century models that have no such a pause.

The TSD has to date refused to acknowledge this serious problem. If there is no defensible model, then there is no defensible scientific basis for an endangerment finding or a quantitative limitation on carbon dioxide emissions.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 52, Box 6.3

The secular climate history calls for a re-assessment of the likely equilibrium sensitivity to a carbon dioxide doubling. As of now, the range is give as 2.0 to 4.5°C, with the most likely value of 3.0°C. The true likely value is highly dependent upon how much one ascribes the (0.5°C) warming of the early 20th century to carbon dioxide. There is no appropriate discussion of this here, and one is in order. It is generally thought that much of the early 20th century warming was largely driven by natural variations including changes of the sun's output. If that is true, then the equilibrium warming for a doubling is quite low, as only the warming from 1976-1998 is ascribable largely to CO2. Given a combination of residence time and equilibration time in the range of 60 years (still a reasonable assumption), and the linearities of the midrange SRES (and other) scenarios, this points towards a warming of about 1.6°C in this century, which roughly translates to an equilibrium sensitivity of a doubling to around 2.1°C. The troubling lack of warming since 1998 (which cannot be discounted simply because of the El Nino because the IPCC GCMs claim to simulate them), implies a further delay for the water vapor amplification (which itself may be overestimated; see, for example, Spencer et al., 2007; Spencer and Braswell, 2008).

Note that on page 53, lines 1-4, the TSD states that the observed temperature patterns are *Consistent with observations during the latter part of the 20th century*, rather than to the 1910-45 warming. This again points to a likely value *below* 3.0°C. This raises a serious question about the utility of the TSD for the ANPR.

The final sentence that "abrupt climate change processes cannot be predicted with confidence and the thresholds linked to risks for social systems are at least as uncertain" is true.

The TSD is inconsistent here, and is insufficient for basing an endangerment finding.

Page 53, lines 1-4: "Geographic patterns of projected warming show greatest temperature increases over land…consistent with observations"

McKitrick and Michaels (2007) and Michaels (2008) showed that as much as 50% of the land-only warming can be ascribed to "non-climatic" causes from 1979 to the present.

These include land-use changes and measures of prospective data quality. In general, we found that the poorer the region, the more warming was overestimated. The quality of the US data, however, is very good, but there are quality questions about the other land data as shown in those papers. The TSD statement may be based upon questionable land data.

Page 53, lines 23-24: Glaciers, ice caps and the Greenland Ice Sheet are also projected to add to sea level."

This is extremely vague and, lacking quantitative guidance, somewhat misleading.

Please add "The midpoint values for 21st-century sea-level rise in the IPCC's midrange (AIB) scenario are 0.12 meters (4.7 inches) for glaciers and ice caps, and 0.04 meters (1.6 inches) from Greenland." These rates clearly do not justify an endangerment finding.

Page 53, line 35

Correction requested: Please add the statement "MacDonald (2000) found that July temperatures for much of the Eurasian and Scandinavian arctic were as much as 2.5-7°C greater than modern for several millennia in the early Holocene. There is no evidence for a concurrent catastrophic loss of Greenland ice." Without this important note, the TSD is seriously misleading

Page 53, line 42

In portions of the U.S., mainly the Gulf Coast, the sea-level rose much more in the last century (because of subsidence) than the median prospective global-warming driven rise in the next century. For example, the rate of sea-level rise at Grand Isle, LA is 3.03 ft/century, at Galveston is 2.24 ft, and on the Atlantic, at the mouth of the Chesapeake Bay, is 1.98 ft. Grand Isle records date back to 1947, and Galveston to 1957. (Source: Http://co-ops.nos.noaa.gov/sltrends/sltrends.html)

If this amount of rise was adapted to, how will a smaller one lead to endangerment?

The TSD is not based upon complete science here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 54, Figure 6.9 (Alaska), 1950-2000 temperatures.

Hartmann and Wendler (2005) noted that Alaskan temperature trends (with the exception of a very few stations) are flat on either side of the 1976-77 Pacific climate shift. The record in Christiansen (2007) (IPCC AR4) is clearly at variance with this. Note that Hartmann's record contains much more data than the IPCC gridded set used by Christiansen. The problem is further complicated by Christiansen's selection of "Alaska",

which in reality includes more of Canada than Alaska. At the least, the figure should be labeled "Alaska and Northwestern Canada", but if it is to remain "Alaska" the climate record from *Alaska* should be used, which is quite different than the one shown. Given that this report is about the United States, Alaska-only temperatures are appropriate.

The TSD is not scientifically accurate here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding*.*

Page 54, lines 4-5: "winter over northern parts of Alaska…as shown in Figure 5.12".

I don't have a Figure 5.12. If the text is referring to Figure 6.9 on the same page, then the phrase "reaching 10°C" is misleading, because that's the highest value from the highest carbon dioxide input scenario. Text and Figures need to match, and it is not appropriate to mention only one value (from one scenario?) when each scenario gives a range. Doing what was done here (reaching, or saying "as much as") makes the report seem biased and weakens it as support for the ANPR.

Page 56, lines 6-9: "For North American coasts, emissions scenario A1B shows sea level rise values close to the global mean…"

Contrary to the statement, the rate of sea level rise observed in the last 100 years at locations noted above along the U.S. Atlantic and Gulf Coasts (trends based upon samples of differing length) exceeds the mean A1B global value of 13.5 inches. (see http://co-ops.nos.noaa.gov/sltrends/sltrends.html). TSD is incomplete here and therefore misleading.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding*.*

Page 56, lines 9-10: "Vertical land motion from geologic processes may decrease (uplift) or increase (subsidence) the relative sea level rise at any site (Nicholls et al., 2007)."

This sentence refers to the U.S. South Atlantic and Gulf Coasts, where subsidence has been much greater than climate-related sea-level rise. Consequently, climate is a relatively unimportant factor here.

Page 57, lines 23-24. "Possible implications for human health of these projected changes in temperature extremes are discussed in Section 7(b)."

This statement is misleading. The information in 7b contains no reference to the highly-documented phenomenon of adaptation to increasing heat-wave frequency.

The EPA might substitute "some" implications, which is a more honest word meaning that also weakens the TSD as support for endangerment.

Page57, lines 34-36 and associated references

This section references the IPCC WG1 Chapter 10. Figure 10.12 from that chapter gives multi-model projected precipitation changes. The greatest negative changes are indeed in the Southwest, at around 0.2mm/day. This works out to slightly less than three inches per year *over the course of 100 years*. However, in general, there has been a secular increase in precipitation over this region (see Figure 9).

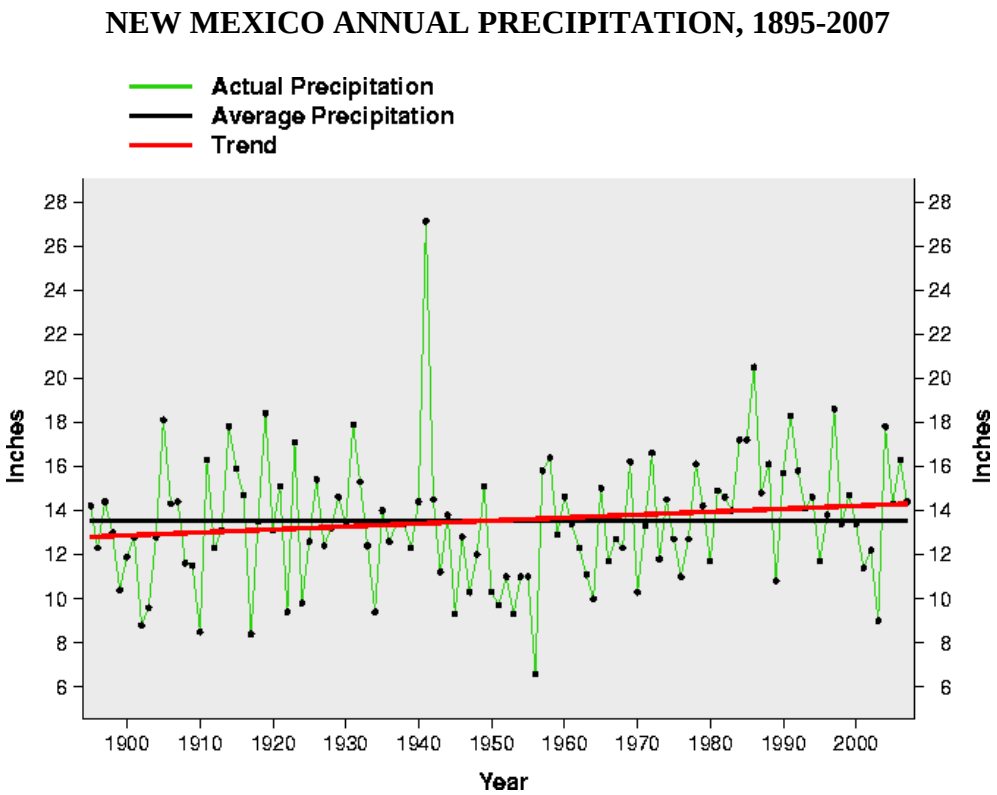


Figure 9. Statewide average total annual precipitation, New Mexico, 1985-2007 (source: NCDC).

So, what is being projected here is a decline of less than 1.5 inches below where precipitation averaged in the early 20th century. It is hard to believe that this represents a significant endangerment, given the other massive pressures on the water supply, such as immigration and regional population growth.

Modify the text so that it specifically refers to the U.S., and then put the IPCC figures in historical perspective, which in fact provides little support for endangerment.

Page 57, line 43: "… find widespread increases in extreme precipitation events under SRES A2..."

Why pick only the most extreme scenario? IPCC tended to give A2, A1B and B2 in its text, and the TSD would do well to do the same. As it stands, this the TSD exaggerates for effect, and is an inappropriate reference.

The TSD is not scientifically complete here. An updated and more thorough discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Page 57, lines 46-48: "Based on a range of models, it is likely that tropical cyclones...will become more intense....

The real numbers should have been provided. Knutson and Tuleya (2004) show a 6% increase in mean windspeed in what works out as *year 80* in their simulation. In 2008, Knutson's revised model predicted a 30 percent reduction in the frequency of landfalling hurricanes in North America. The mean windspeed in the reduced number of storms is now 2%, a number that is clearly too small to measure. The TSD is not sufficiently current with the tropical cyclone literature to be able to find support for any endangerment finding. The TSD should be revised to be less misleading in this area, giving magnitude and relative frequency changes.

The TSD is not scientifically complete here. An updated and more thorough discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Page 58, lines 34-37. "Scientific data show that abrupt changes...during the last glacial period…likely up to 16°C within decades over Greenland…"

The citation, Jansen et al., 2007, which is the IPCC's Fourth Assessment chapter, "Paleoclimate" states:

> High-latitude records show that ice age abrupt temperature events were larger and more widespread than were those of the Holocene. The most dramatic of these abrupt climate changes were the Dansgaard-Oeschger (D-O) events, characterized by a warming in Greenland of 8°C to 16°C within a few decades(see Severinghaus and Brook, 1999; Masson-Delmotte et al., 2005a for a review) followed by much slower cooling over centuries.

This is an ice age phenomenon, and hence not relevant to the current climate. Further, the IPCC gives a range of temperature, not "likely up to," which is a rhetorical device rather than a scientific statement. The TSD has no application to "endangerment".

Correction requested: The TSD is not scientifically relevant here. An updated and discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Page 59, lines 26-29

This sentence notes that the "complete" disintegration of the Greenland Ice Sheet would take "many hundreds of years to complete", according to models. The citation is Meehl et al., 2007 (the IPCC AR4).

Shown below is the Greenland simulation from that report. Note that it is not complete at 1760 years, and further, that this assumes that atmospheric carbon dioxide concentrations quadruple from the background and *stay at that concentration* for millennia (Figure 10).

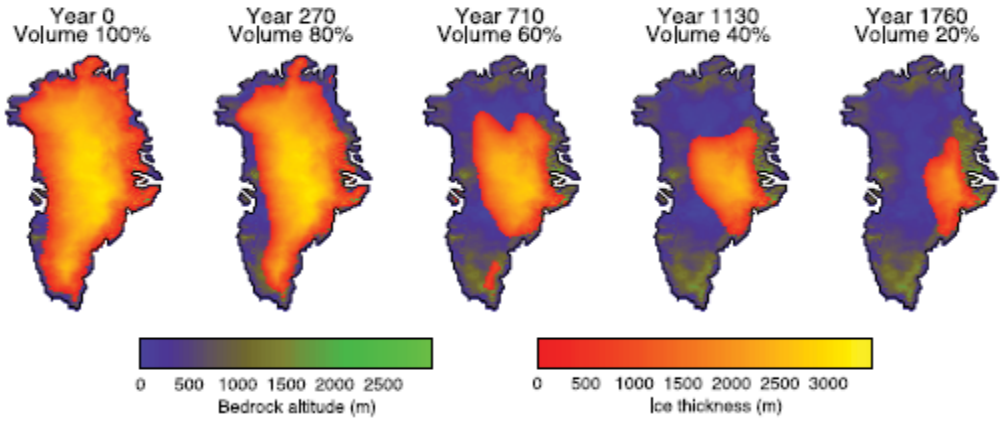


Figure 10. Modeled evolution of Greenland surface elevation and ice sheet volume over time under a quadrupling of atmospheric CO2 concentrations (source: IPCC AR4).

It is dubious, indeed, to believe that we will be powered by fossil fuels for the next two millennia, sufficient to maintain an atmospheric concentration of carbon dioxide of over 1100ppm. In fact, none of the IPCC SRES reach this value by 2100. The TSD did not note this. Further, the TSD clearly leaves the reader with an impression (complete loss on the "hundreds of years" scale) that is not what is in the latest IPCC AR4 WGI Chapter 10 report. It is inappropriate to use the TSD's statement on Greenland as scientific support for endangerment.

The TSD is not scientifically relevant here. An updated and discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Pages 59-60, lines 31-40, 1-4

TSD neglects here to point out that Antarctica was warmer for millennia after the end of the ice age and the West Antarctic Ice Sheet (WAIS) clearly did not collapse. In a study of 11 long-term Antarctic ice core records, Masson et al. (2000) state, "…the

classical temperature interpretation suggests a warming by 0.2 to 2.5°C, depending upon site" for the period from 11,500 to 9,000 years ago. By neglecting this data, the TSD is misleading on an important historical test of the stability of the WAIS.

Page 61, lines 12-17

This paragraph claims that the observed surface warming of the Antarctic Peninsula and cooling in the interior are explained by modeling experiments cited in Baldwin et al., (2007). As per my previous comment (*Page 61, lines 12-17*), the primary citation in Baldwin et al. is Gillett and Thompson (2003) from *Science*. It is very clear from Figure 3 in that article that Antarctic Peninsula warming is *underestimated by a factor of 3*. **It is therefore scientifically false for the TSD to say that models explain the Peninsular warming.**

Section 7: Human Health

Page 64, Lines 7-8: "Depending on progress in health care, infrastructure, technology and access, climate change could increase the risk of heat wave deaths."

This statement is profoundly misleading. Heat wave deaths decline as frequency of heat waves increases, as shown for North American cities by Davis et al. (2003a, 2003b). A major adaptation to heat waves occurred in France as a result of the 2003 disaster. In that even, deaths substantially exceed model projections but when a heat wave of similar magnitude struck in 2006, far fewer deaths occurred than were predicted (Fouillet et al., 2008). The difference was a direct result of adaptation and the lessons learned from the earlier event. The words "depending upon progress in health care…" are an attempt to influence the reader into thinking that people are unlikely to adapt, when in fact there is incontrovertible evidence that they already have.

This also applies to lines 21 and 22 on the same page, which are simply wrong because of observed adaptation.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 65, lines 1-36

EPA has muddled some concepts about mortality demographics and thus have reached an incorrect conclusion. Once a person reaches his or her teens, their risk of dying increases on a daily basis as he or she ages. As the Baby Boomer demographic gets older, there will be a broad mortality spike. Current U.S. life expectancy at birth is just over 78 years. This was 77.2 years in 2001 and only 75.4 in 1990, not really that long ago. As there are more elderly living as a percentage of the population, they will certainly make up an increasing percentage of the people who die.

But is their *risk* of dying greater? **If so, then life expectancy could not be increasing.** It is true that elderly do not have the same ability to thermo-regulate as do younger people, but they are clearly living longer despite increased exposure to heat, as you note in citations on this topic. EPA can correct this problem by simply deleting lines 16-36. It's a really hard argument to make that future heat wave mortality will increase despite the fact that historically the trend has been exactly the opposite even with increasing heat and humidity.

The fact that more of the populace tends to live in cities actually weakens EPA's argument for CO_2 impacts. EPA correctly notes that cities are generally warmer owing to the urban heat island, so urban residents routinely are exposed to higher temperatures than are their rural counterparts. And despite this increased heat exposure, age-adjusted death rates have not increased over time**. The TSD is too incomplete to serve as backing for endangerment.**

Page 65, lines 15-22, "There is some indication that populations in the U.S. became less sensitive to high temperatures over the period 1964 to 1988, in part, due to these factors (Davis et al., 2002; Davis et al., 2003; Davis et al., 2004 in Confalonieri et al, 2007). On the other hand, growing numbers of older adults will increase the size of the population at risk because of a decreased ability to thermo-regulate is a normal part of the aging process (Confalonieri et al, 2007). In addition, almost all the growth in population in the next 50 years is expected to occur in cities (Cohen, 2003 in Confalonieri et al, 2007) where temperatures tend to be higher due to the urban heat island effect increasing the total number of people at risk of adverse health outcomes from heat."

This is a remarkable misreading of the Davis papers, which show that heat-related mortality is *lowest* in U.S. cities that have the oldest population distributions (Tampa and Phoenix). These are inherently very hot locations; the heat island in Phoenix is one of the world's largest. The one American city that shows an increase in heat-related mortality is Seattle, the coldest large city in the summer and one with a very young population distribution. The TSD is simply wrong here, and therefore misleading.

Page 65, lines 1-46 (Increased heat exposure)

EPA is making a mistake and getting confused by projected population structure changes and trying to tie them into climate *change*, when really the issue has to do with the prevailing climate. It is true that as the population ages, the number of individuals susceptible to extreme heat effect increases, but this has nothing to do with climate *change*. The effects of population age structure are accounted for in the series of work by Davis et al., that you all cite, and the results overwhelmingly show that the overall (standardized) population's sensitivity to extreme heat has been declining across the U.S. from the 1960s through the 1990s (1998 not 1988 as is indicated on Page 65, line 16). This declining sensitivity includes the elderly. There are two way to deduce this, the first, is that elderly mortality makes up the majority of all heat-related mortality, and so declines in standardized mortality rates imply declines in elderly mortality rates, and the

second, is that in Davis et al., 2002, changes in elderly mortality rates were examined directly for several cities in the eastern U.S., and declines in mortality during extreme heat events were documented. *And further, these declines occurred during a period of increasing summer temperatures*. Thus, there is plenty of evidence that there is a declining sensitivity to extreme heat events in the United States, even in the elderly population.

Projections of future mortality rates inaccurately reflect the impact of climate (much less climate change) if they do not take into account the changing age-structure of the population—pure and simple. A raw increase in the number of people who die in extreme heat events because there is a greater number of susceptible individuals is neither related to climate nor climate change. Thus, the arguments made on Page 65, lines 17-22 are simply wrong. More people may die in the future during heat events, but it will likely not have anything to do with climate change—as the series of studies by Davis et al. clearly indicate—Americans (including the elderly) have become better adapted to extreme temperature events and mortality rates during extreme temperature events, when properly standardized for population age-structure changes, are declining. No matter how the population age structure changes, these standardized declines will likely continue as better medical practices and community awareness programs help alleviate the impacts of extreme heat events.

In fact, quoting from the EPA's Excessive Heat Events Guidebook (page 7):

> Despite the history of adverse health impacts, there is consensus that most of these outcomes are preventable (CDC, 2004a). Lessening future adverse health outcomes from EHEs will require improving the awareness of public health officials and the general public about the health risks of EHEs while continuing to develop and implement effective EHE notification and response programs.

Somehow it seems that EPA has now given up on the belief in its own Program's effectiveness as laid out in the Excessive Heat Events Guidebook?

Again, the TSD repeats this ill-founded claim on Page 65-lines 24-29. In order to accurately compare the response to excessive heat events across time **one must first control for population changes**. This is standard practice (see http://www.cdc.gov/nchs/data/nvsr/nvsr56/nvsr56_16.pdf for example). As the population of the U.S. grows, the number of total people who die each year increases, but the mortality *rate* does not (in fact, it has been declining)! Thus, in discussion of heat-related mortality in the future, you must discuss mortality *rates* not overall mortality. And, as has been demonstrated by Davis et al., heat-related mortality *rates* are declining across the U.S., including in the elderly population. Without a discussion of mortality rates, your entire line of reasoning in lines 24-29 is improper and misleading.

Here is what Gosling et al. (2008) "Associations between elevated atmospheric temperature and human mortality: a critical review of the literature"—another post-IPCC AR4 paper—has to say about age-standardization:

> In a long time-series, the changing age–structure of the population should be accounted for, because an aging population will be more vulnerable and may bias temporal comparisons (Calado et al. 2005; Davis et al. 2003a). This is commonly achieved by the direct standardisation method (Anderson and Rosenberg 1998), or by only examining a restricted age group. This method is commonly applied in the epidemiological and synoptic climatological approaches.

The TSD is obviously an inappropriate backing for endangerment on this matter.

Page 65, line 35-36

If the incidence of heat waves in Chicago increases in the future, all signs point to a decline in the heat-related mortality there as a result. See the series of papers by Davis et al. (2003a, 2003b). Davis et al. found 1) that the sensitivity to extreme heat in the populations of cities in the Midwest has been declining over the past 3-4 decades, despite rising summertime heat, and 2) that in locations where heat waves are more common (along the southern tier of the U.S.), the population is even better adapted to them and thus less sensitive. Therefore, if heat waves become more common events in the future, Chicago should become much better prepared and adapted to them.

Further, there has been direct evidence of Chicago's ability to adapt and respond to heat events. As has been published in the scientific literature (Palecki et al., 2001), a heat wave of nearly similar intensity struck Chicago during the summer of 1999 and the mortality was much less than in 1995 because the city was better prepared. Chicago didn't redesign its city between 1995 and 1999, but it did redesign its response measures to heat waves. And it was quite successful. Clearly, if heat wave intensity and/or frequency increase in the future, cities will better prepare for them. This improved response has been demonstrated to have taken place in France as well (Fouillet et al., 2008).

The bottom line is this: If heat wave frequency and/or intensity of extreme heat events increase in the future under anthropogenic global warming, the citizens of the U.S. will take appropriate measures to adapt to them—a response that is demonstrated to be on-going. The overall impact will be a declining sensitivity to extreme heat events and a decline in population-adjusted mortality (and no doubt, morbidity) rates. This whole section (Page 65, lines 1-36) fails to recognize this, but instead, uses improper methodology to contend that climate change will kill more people than climate does now. This is simply an ill-founded conclusion and needs to be corrected.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Pages 65 (lines 40-46) and 66 (lines 1-20)

The number of people who die from cold exposure is a small segment of total winter mortality, so warming of the cold air masses will have no real effect on mortality rates.

People who have not done biometeorological research on human mortality frequently make the mistake that you are making here—that because mortality is higher during heat waves then global warming will reduce cold mortality. But the response is quite different at the cold tail of the distribution, and I discussed this fact in great detail in my Davis et al., 2004 paper (which you also cite). I agree with your comments in general on lines 7-10; **but a careful read of my 2004 paper indicates that temperature does not have a major influence on monthly mortality rates in U.S. cities.**

Delete lines 15-20. The Kalkstein and Greene (1997) study did not 1) use age-adjusted population or 2) account for the observed declining trend in heat-related mortality. Either of these issues is reason alone to not use this paper as a basis for an endangerment finding.

The TSD is not based upon the best available science here. An updated discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Page 66, lines 12-20

Just because the IPCC doesn't cite any studies looking at cold vs. heat related deaths in the U.S. in the future doesn't mean that they don't exist. For instance, the subject of Davis et al. (2004) is precisely this topic. We concluded that whether or not a future temperature rise resulted in a net increase of decrease in the standardized population rates of temperature-related mortality depended on the seasonal distribution of the temperature change patterns. A winter-dominated warming produced lower overall mortality rates, while a summer-dominated warming produced an elevated mortality rate. However, most importantly, we found that the impact of temperature on mortality rates in the U.S. was minuscule, making up only a few hundredths of a percentage of the overall annual U.S. mortality rate. Ultimately we concluded that:

> Temperature currently does not have a major influence on monthly mortality rates in US cities. By the 1990s, there was little evidence of a net mortality benefit to be derived from one's place of residence. In all locations, warmer months have significantly lower mortality rates than colder months, undoubtedly related to the impact of influenza during the cold season, although influenza (and co-morbid influences) alone cannot account for the observed seasonality in mortality. The winter–summer

> mortality rate difference of residents in Phoenix or Los Angeles, for example, is similar to those of people in Minneapolis or Boston. This consistency across cites with largely different climate regimes can be partially attributed to adaptations (both biophysical and infrastructural) and influences of technology. The implication is that the seasonal mortality pattern in US cities is largely independent of the climate and thus insensitive to climate fluctuations, including changes related to increasing greenhouse gases.

By not noting this, the TSD is very incomplete and misleading.

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 66, lines 34-42

Despite projections of increased flooding in the U.S., no trends (actually a slight statistically insignificant decline) in flood-related damages—when properly accounting for increases in U.S. wealth and property at risk—have been detected (Downton et al., 2005), in the face of rising global temperatures (Figure 11). **The TSD does not note this and is therefore incomplete.**

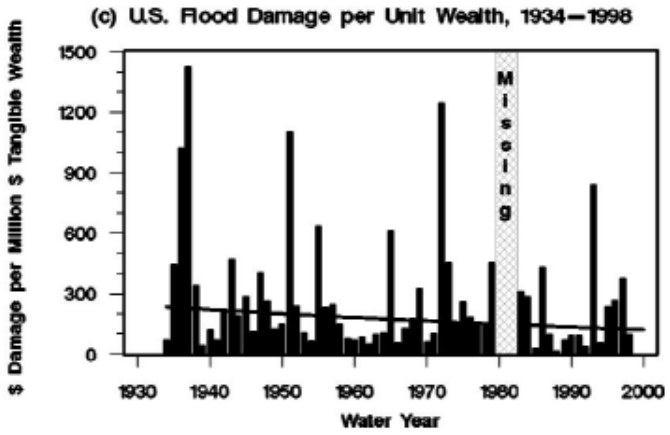


Figure 11. U.S. flood damage normalized for changes in tangible wealth (Downton et al., 2005).

Page 67, lines 1-13.

EPA's contention that the risk of deaths will increase from stronger storms once again is counter to observed trends. First, the argument that tropical cyclones will become more severe is not supported by a large number of hurricane experts (while some do agree with EPA, it should at least acknowledge that there is significant disagreement on this topic and that the issue has hardly been resolved). Certainly more people are placing themselves in harm's way by choosing to live along the coasts, but despite this preference, they are dying less often. And once again, the reason is adaptation: Stronger

structures, better warning systems, better forecasts, better evacuation procedures, better disaster preparedness and management. EPA's predictions seem to be based upon an "everything else being equal" scenario when that has not been the case over the last century. If storms do indeed get worse, which is questionable, we will continue to adapt to these conditions. EPA's use of Katrina as an "adaptation is inadequate" example completely misses the point. Louisiana was primed for this kind of disaster for decades and EPA knew it. Katrina was in no way a climatologically remarkable storm in New Orleans. The fact that this became a disaster is more of an indictment of government emergency response officials than a failure of our adaptive capacity.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 67, Lines 1-3

The IPCC 2007 results are outdated when it come to the association between current on future trends in tropical cyclones and human-caused global warming. The latest results are that the patterns of recent changes in tropical cyclone activity do not match those projected to have occurred from anthropogenic global warming (e.g. Knutson et al., 2008) and that future projections are not for a greater number or more intense storms (e.g. Vecchi and Soden, 2007a, 2007b; Bengtsson et al., 2006).

The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 67, lines 4-13

The U.S. vulnerability to hurricanes is primarily a climate and human choice issue, not a climate *change* issue. There has been monumental growth in tropical cyclone-related damages in the U.S. (Figure 12), but no growth in damages if they are properly standardized for population and wealth changes (Figure 13) (Pielke et al., 2008).

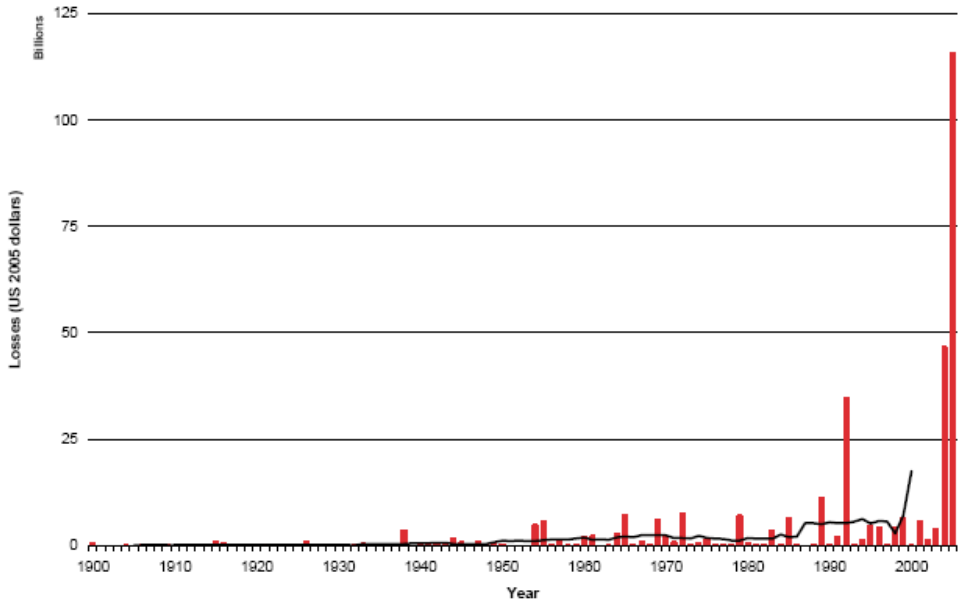


Figure 12. U.S. tropical cyclone damage (in 2005 dollars) when adjusted for inflation, 1900-2005 (from Pielke Jr., et al., 2008).

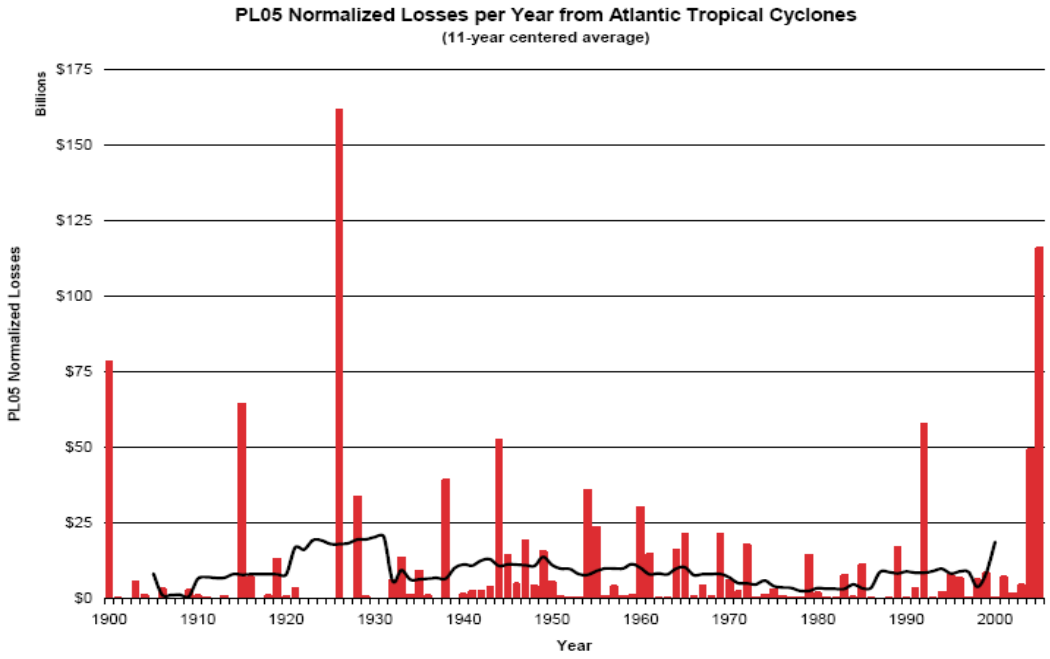


Figure 13. U.S. tropical cyclone damage (in 2005 dollars) when adjusted for inflation, population growth and wealth, 1900-2005 (from Pielke Jr., et al., 2008).

The situation with the U.S. vulnerability to future tropical cycles is well-summed up in a recent statement released by a collection of some of the world's leading hurricane researchers issued the following statement that reflects the current scientific thinking on hurricanes and their potential impact (http://wind.mit.edu/~emanuel/Hurricane_threat.htm) in coming years:

> [T]he possible influence of climate change on hurricane activity is receiving renewed attention. While the debate on this issue is of considerable scientific and societal interest and concern, it should in no event detract from the main hurricane problem facing the United States: the ever-growing concentration of population and wealth in vulnerable coastal regions. These demographic trends are setting us up for rapidly increasing human and economic losses from hurricane disasters, especially in this era of heightened activity. Scores of scientists and engineers had warned of the threat to New Orleans long before climate change was seriously considered, and a Katrina-like storm or worse was (and is) inevitable even in a stable climate.
>
> Rapidly escalating hurricane damage in recent decades owes much to government policies that serve to subsidize risk. State regulation of insurance is captive to political pressures that hold down premiums in risky coastal areas at the expense of higher premiums in less risky places. Federal flood insurance programs likewise undercharge property owners in vulnerable areas. Federal disaster policies, while providing obvious humanitarian benefits, also serve to promote risky behavior in the long run.
>
> We are optimistic that continued research will eventually resolve much of the current controversy over the effect of climate change on hurricanes. But the more urgent problem of our lemming-like march to the sea requires immediate and sustained attention. We call upon leaders of government and industry to undertake a comprehensive evaluation of building practices, and insurance, land use, and disaster relief policies that currently serve to promote an ever-increasing vulnerability to hurricanes.

It is not climate change, but climate/human behavior interactions that are leading to the U.S.'s vulnerability to hurricanes, past, present and future. **As it stands, the TSD is misleading on this issue and a correction must be made.**

Page 67, lines 15-21

There is no evidence that droughts are increasing across the U.S. since 1900, despite rising global temperatures (Figure 14). So, concerns of such changes in the future should be tempered by past observed behavior.

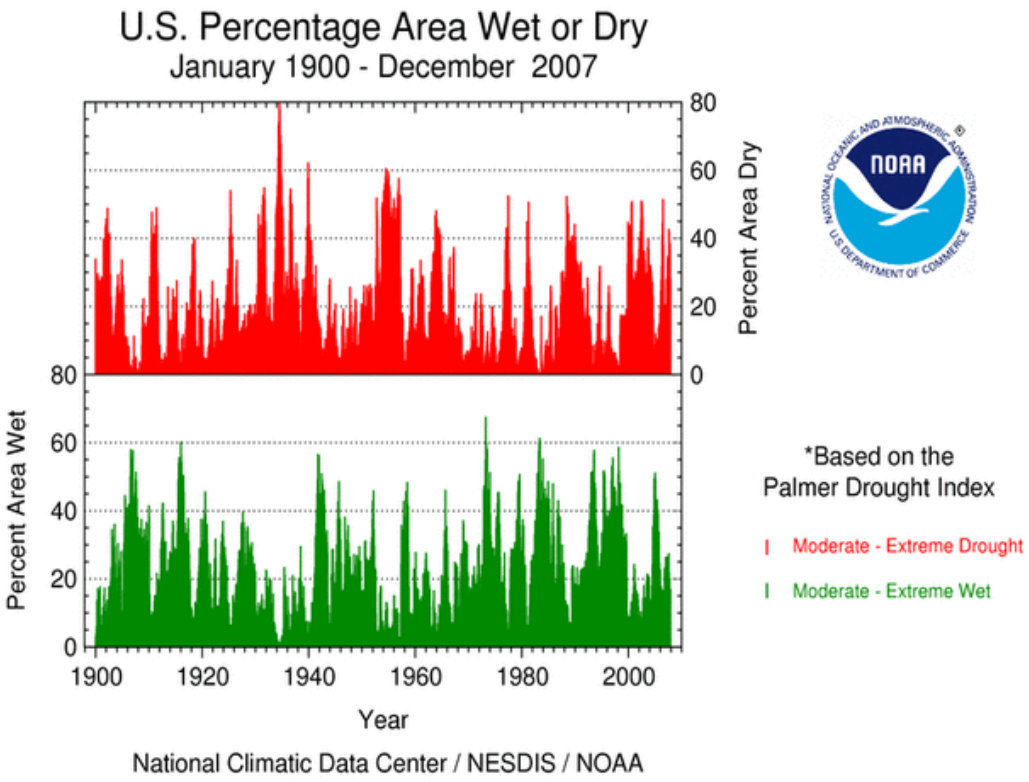


Figure 14. Percentage area of the U.S. experiencing moderate (or worse) drought (top) or wetness (bottom). There is no indication that the U.S. moisture climate has been impacted by global temperature increases during the past century (figure source: National Climatic Data Center, http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/drought-summary.html)

Page 67, Lines 23-31

In a paper published subsequent to the IPCC AR4, Kitzberger et al. (2007) examined the influences on historical wildfires (back to 1550AD) in the western United States and identified the influences of natural multi-annual and multi-decadal oscillations in sea surface temperatures both in the Pacific as well as the Atlantic Oceans.

They found that the ocean temperature variations created by the Atlantic Multidecadal Oscillation (AMO) are actually the dominant factors in whether wildfires were widespread across the West. They found that the warm phase of the AMO appears to be the critical factor in explaining the massive outbreaks of fire across the West—including the most recent ones. As the lead author noted, "The key issue is that the Atlantic Multi-Decadal Oscillation persists on time scales of 60 to 80 years, compared to just one year or a few years for El Niño."

Another recent paper (Zhang et al., 2008) offered further evidence that the AMO cycles are driven by natural, rather than anthropogenic, forces.

As we are currently in the warm phase of the AMO and, given the very long-term nature of this Atlantic ocean/climate phenomenon, we are likely to be in the AMO warm phase for quite some time to come. That suggests we will likely continue to see an increased frequency of western wildfires in the years to come**. However, this projection (and their occurrence) is not from the influences of anthropogenic global warming, but from the associations between wildfires and natural climate variations.**

Page 67, lines 44-46

The examples of Katrina and Rita demonstrate the perils of our climate, not climate *change*. As mentioned, research subsequent to the publication of the IPCC AR4 does not support a high likelihood that anthropogenic global warming will increase hurricane strength or severity (e.g. Knutson et al., 2008; Vecchi and Soden, 2007a 2007b; Vecchi et al., 2008; Bengtsson et al., 2007).

The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 68, lines 9-16

This whole paragraph is inaccurate. An examination of the patterns of outbreaks, along with a little background knowledge of the climate of the U.S. makes the claims made in this paragraph nonsensical.

The TSD is scientifically inaccurate. Remove this entire discussion.

Page 68 lines 9-16.

This entire section should be deleted because 1) you don't make any linkages between climate change in the U.S. and these conditions (for good reason, because the linkage doesn't exist) and 2) vector disease control activities can virtually eliminate the problem (as you state in lines 18-20). The TSD doesn't support endangerment in this area.

Page 68, line 10-11 (West Nile Virus)

West Nile Virus was introduced to the United States through the port of New York City (hardly the warmest place in the country) in 1999. It then rapidly spread throughout the country within a few years (Figure 15). This is not a sign that the U.S. is progressively warming. Rather, it is a sign that the existing environment is naturally primed for the virus.

The vector for West Nile is mosquitoes; wherever there is a suitable host mosquito population, an outpost for West Nile virus can be established. And it is not just *one* mosquito species that is involved. Instead, the disease has been isolated in over *40*

mosquito species found throughout the United States. So the simplistic argument that climate change is allowing a West Nile-carrying mosquito species to rapidly spread across the country is simply wrong. The already-resident mosquito populations of the U.S. are appropriate hosts for the West Nile virus.

Clearly, as is evident from the establishment of West Nile virus in every state in the contiguous U.S., climate has little, or nothing, to do with its spread. The annual average temperature from the southern part of the United States to the northern part spans a range of more than 40ºF, so clearly the virus exists in vastly different climates. In fact, West Nile virus was introduced in New York City and has spread westward and southward into both warmer and colder and wetter and drier climates. This didn't happen because climate changes allowed its spread, but because the virus was introduced to a place that was ripe for its existence—basically any location with a resident mosquito population (almost anywhere in the U.S).

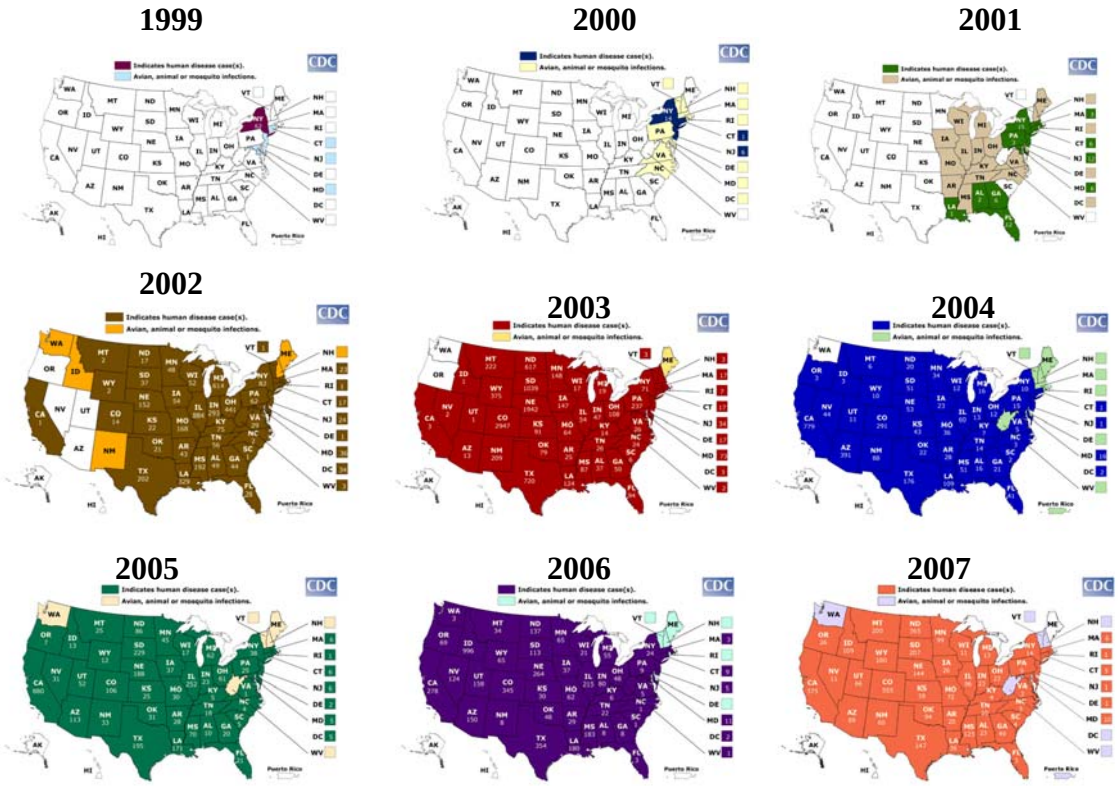


Figure 15. Spread of the occurrence of the West Nile Virus from its introduction to the United States in 1999 through 2007. By 2003, virtually every state in the country had reported the presence of virus. (source: http://www.cdc.gov/ncidod/dvbid/westnile/Mapsactivity/surv&control07Maps.htm).

West Nile virus now exists in the U.S. because the extant climate/ecology of the U.S. is one in which the virus can thrive. The reason that it was not found in the U.S. in the past was simply because it had not been introduced. Climate *change* in U.S. has absolutely nothing to do with it. By following the virus' progression from 1999 through 2007, one

clearly sees that the virus spread from NYC southward and westward, it did not invade slowly from the (warmer) south, as one would have expected if warmer temperatures was the driver.

Since the disease spreads in a wide range of both temperature and climatic regimes, one could raise or lower the average annual temperature in the U.S. by many, many degrees or vastly change the precipitation regime and not make one bit of difference in the aggression of the West Nile Virus. Claims that climate change has something to do with it ignore these basic facts. **The TSD is wrong on this issue.**

Page 68, lines 11-13 (St. Louis encephalitis)

"Saint Louis encephalitis has a tendency to appear during hot, dry La Niña years"

Are hot, dry La Niña years supposed to increase in the future? According to the supposed source for the TSD, the IPCC AR4 (Chapter 10, page 751):

"Based on various assessments of the current multi-model data set, in which present-day El Niño events are now much better simulated than in the TAR, there is no consistent indication at this time of discernible changes in projected ENSO amplitude or frequency in the 21st century."

So the association with La Niña seems to have little relevance for the future.

How about any association with St. Louis encephalitis outbreaks and the climate?

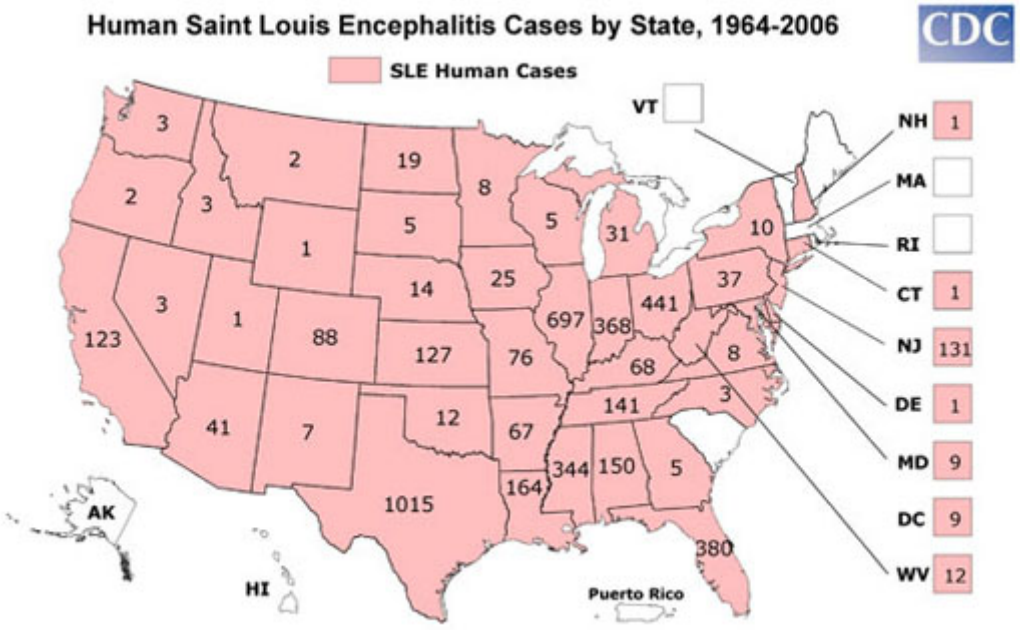


Figure 16. St. Louis encephalitis cases reported by states, 1964-2006 (figure source: Centers for Disease Control, http://www.cdc.gov/ncidod/dvbid/sle/MapsActivity/Sle_SurvControl.html)

Figure 16 shows there is no obvious association. Five cases in Georgia, 10 in New York, 697 in Illinois, 164 in Louisiana, 7 in New Mexico, 19 in North Dakota, 68 in Kentucky, 441 in Ohio. Temperature and precipitation normals vary greatly among these states—probably more so than the variability of individual states. There is no clear pattern.

So surely, if EPA is concerned about St. Louis encephalitis outbreaks, they must be on the rise as the U.S has warmed?

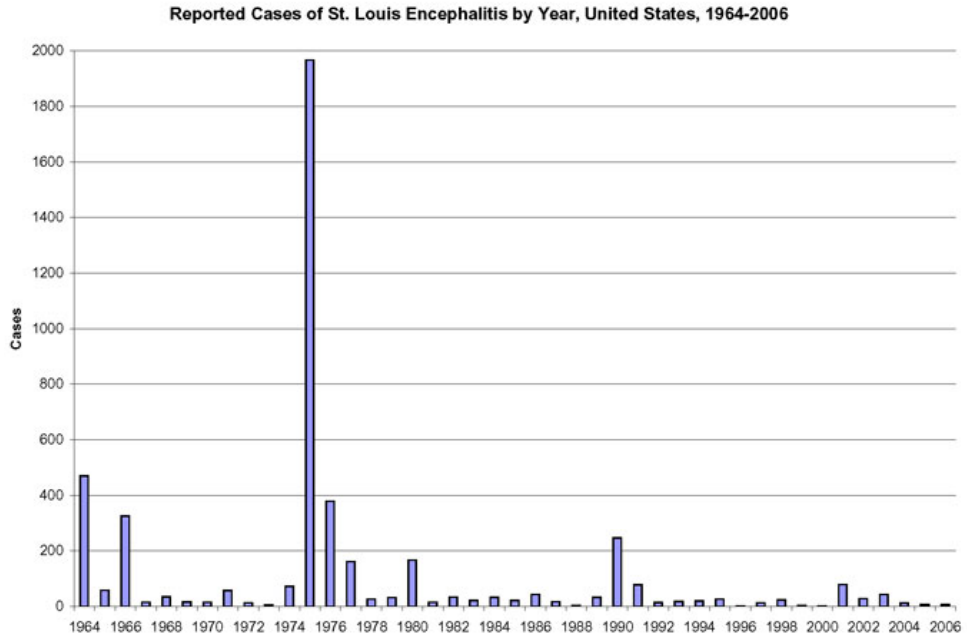


Figure 17. Reported cases of St. Louis encephalitis by year, United States, 1964-2006 (figure source: Centers for Disease Control, http://www.cdc.gov/ncidod/dvbid/sle/Sle_ByYear.html)

As Figure 17 shows, this has most decidedly *not* been the case, even when examining total reported cases not adjusted for population increases.

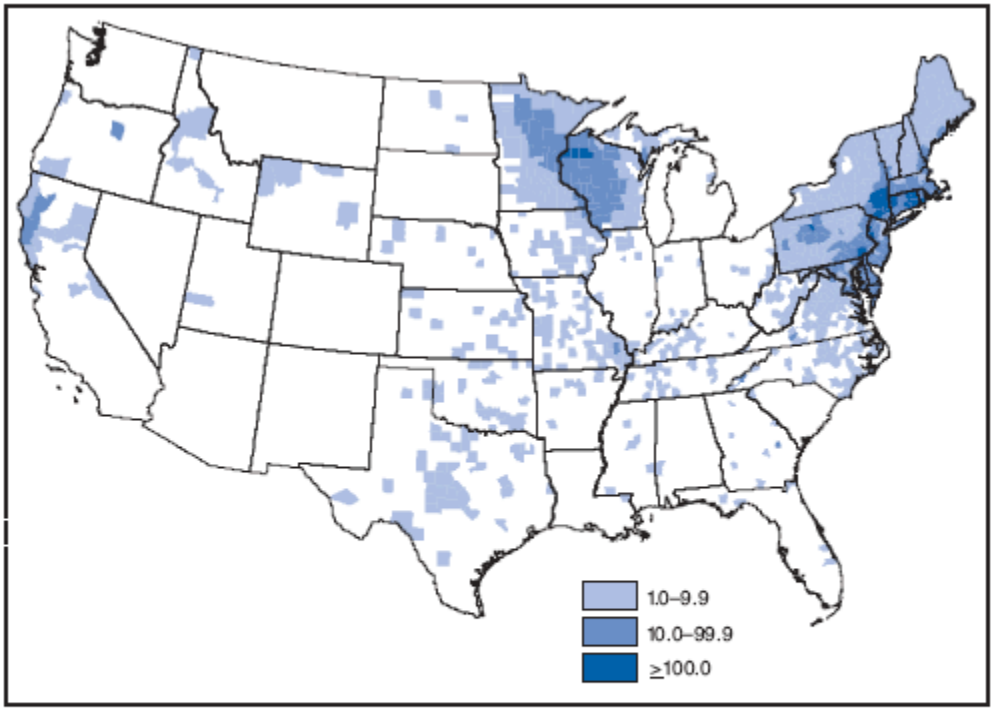
Basically, there is absolutely nothing whatsoever to link climate change to St. Louis encephalitis outbreaks in the U.S.—now or in the future. **Any use of the TSD to support the endangerment is clearly inappropriate here.**

Page 68, lines 13-16 (Lyme disease)

EPA states "A study found that the northern range limit of *Ixodes scapularis*, the tick that carries Lyme disease, could shift north by 200 km by the 2020s and 1000 km by the 2080s."

If the range of the tick associate with Lyme disease increased its range northward as a result of anthropogenic climate change, exactly how would this impact the U.S.? The

highest density of occurrence is already found in our most northerly states (of the lower 48). Since the map below (Figure 18) shows less incidence in warmer locations, perhaps a warming would lower the incidence in the U.S.? **In any case, in no way does the map below indicate the Lyme disease is currently limited by cool temperatures in the U.S., in fact, it seems just the opposite; it flourishes in cooler climates!**



* Per 100,000 population.
† County of residence was available for 98.1% of cases reported during 1992–2006.
§ During 2003, Pennsylvania reported 4,722 confirmed cases and 1,008 suspected cases.

Figure 18. Average rate of Lyme disease (per 100,000 population) by county of residence, 1992-2006 (figure source: Centers for Disease Control, http://www.cdc.gov/mmwr/preview/mmwrhtml/ss5710a1.htm)

The TSD is not based upon the best available science here.

Page 68, lines 18-20

"Although large portions of the U.S. may be at potential risk for diseases such as malaria based on the distribution of competent disease vectors, locally acquired cases have been virtually eliminated, in part due to vector and disease control activities."

Correct. And to further illustrate this fact, malaria case were prevalent across the U.S. in the late 1800s (Figure 19) during a time when the climate was much cooler than it is now. This is a great example of how public health measures are far more effective at controlling disease outbreaks than attempting to control global climate. Such control will continue here, so **the TSD provides no support for endangerment in this area.**

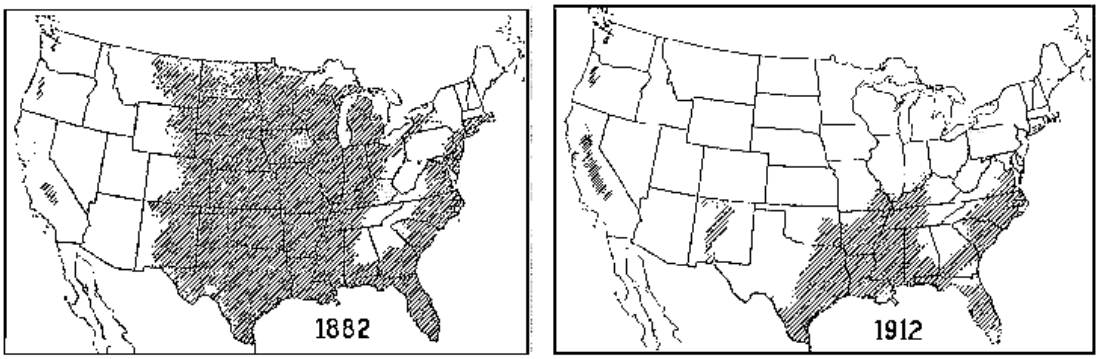


Figure 19. Shaded regions indicate locations where malaria was endemic in the United States in 1882 (left) and 1912 (right) (source: Zucker et al., 1996).

Page 68, lines 22-27

As the Figure 20—U.S. mortality trends and U.S. temperatures trends—reveals, we are greatly triumphing over climate change (or, perhaps a warmer climate is a healthier climate?). It would take a true pessimist to think this strong trend towards decreasing mortality rates will reverse itself in the future, no matter what the climate holds in store—primarily because the impact of climate on human health in the U.S. is minuscule (e.g., Davis et al., 2004).

U.S. Mortality Rate **U.S. Temperature History**

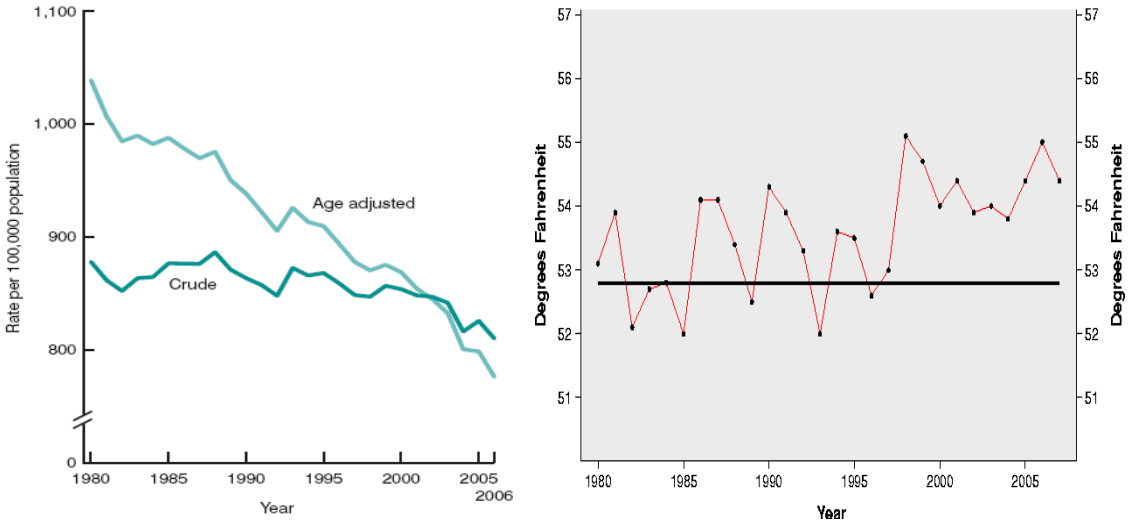


Figure 20. Left: Crude and Age-adjusted mortality rates (per 100,000 people) in the U.S. from 1980-2006 (figure source: Centers for Disease Control, http://www.cdc.gov/nchs/data/nvsr/nvsr56/nvsr56_16.pdf). Right: U.S. average annual temperature, 1980-2008 (figure source: National Climatic Data Center, http://climvis.ncdc.noaa.gov/cgi-bin/cag3/hr-display3.pl)

Correction requested: The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 68-69, Section 7(d) Aeroallergens

The vast majority of the world's plant species grow better when grown under conditions of higher concentrations of atmospheric carbon dioxide. Period.

This is true for plants the produce allergenic reactions, as well as plants that produce food. It is true for plants that cause skin rashes as well as plants that produce medicinal benefits. It is true for weeds and it is true to flowers. It is true for introduced species and it is true for natural inhabitants.

If plants growing better is a bad thing, then you've got something. Otherwise, cherry-picking from amongst all the world's plants only those whose increased health and vigor leads to negative reactions in some people is truly looking at the world through extremely dark glasses. But if you insist, perhaps here is something else you should add to the list:

• The number of people having heart attacks while mowing grass will increase as grass will grow thicker and stronger requiring a greater effort and a greater frequency to mow it. Plus, add on an aging U.S. population (older people are more susceptible to heart attacks) and the numbers could increase manifold.

• The same as above but for raking leaves.

You can see how this can quickly get ridiculous. CO2 makes plants grow better—which, as omnivores, is a good thing for humans, in net (increasing corn production greatly

trumps increasing poison ivy production). **The TSD is an inadequate support document for endangerment in this area.**

Page 68, line 38-46.

Yes, increasing CO2 will make many plants grow faster. I don't doubt that aeroallergens will increase. We are also spending a lot of time and effort planting trees to help mitigate the CO2 increase, and these trees produce pollens and some biogenic hydrocarbons that are involved in ozone formation. **So is the EPA suggesting a major deforestation effort to reduce pollen levels?** You can see that this argument becomes silly very quickly. Why don't you just admit that higher CO_2 levels will increase the growth rates of most plants-including not only weeds but trees and crops?

The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Section 8. Air Quality

Section 8(a) Tropospheric Ozone

Somehow the EPA managed to get through more than two full pages (pages 70-72) of ground-level ozone projections for the future under various and sundry anthropogenic climate change scenarios before getting to the one little paragraph that trumps everything that has come before:

> "The influence of climate change on air quality will play out against a backdrop of ongoing regulatory control of both ozone and particulate matter (PM) that will shift the baseline concentrations of these two important pollutants. However, most studies to date that have examined potential future climate change impacts on air quality isolate the climate effect by holding precursor air pollutant emissions constant over time."

This final statement, which is true, basically means that you should not rely on any of the results that just described as they are inappropriate for making real-world projections!

This paragraph (the one quoted above) should lead the section on tropospheric ozone and not come at the end of it and remove reference to studies which do not take into account on-going trends and impacts of regulatory measures as they do not accurately portray future occurrences.

The TSD fails to reflect the reality of the situation regarding ground-level ozone trends across the U.S—they are declining (during a time of increasing temperatures!). Studies which make projections which fail to incorporate the observed declines are produce meaningless results. Is the EPA planning on reducing its efforts (or know of other planned reductions) in controlling ground-level ozone? If not, then let the current trends be a guide to the future and disregard the projections from studies which do properly

account for known declines. And as you all surely know (since the data come from the EPA, (U.S. EPA, http://www.epa.gov/airtrends/ozone.html), trends in ground-level ozone concentrations across the U.S. are generally downward (Figure 21).

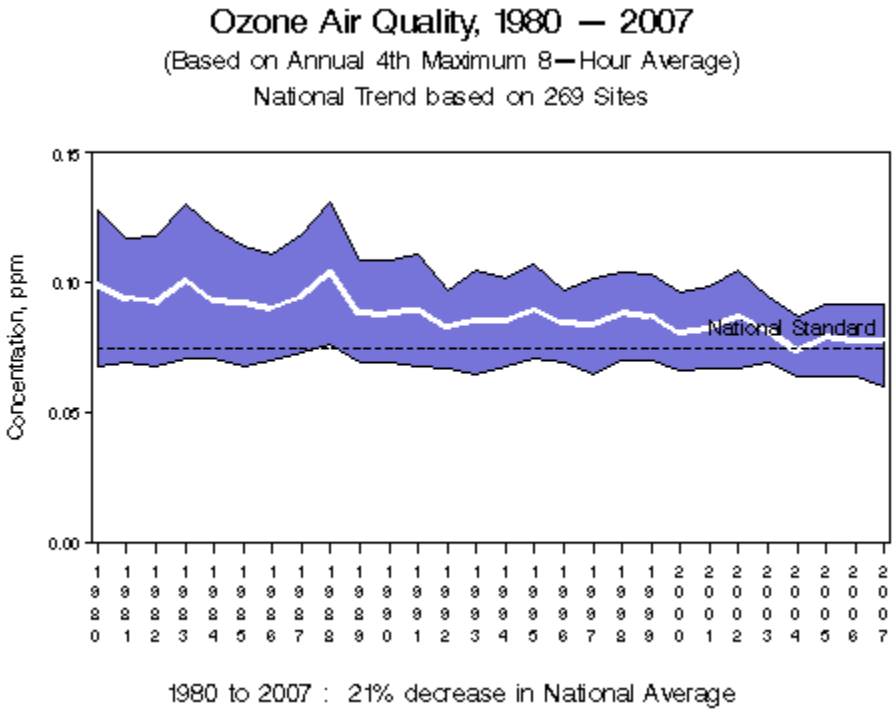


Figure 21. Trends in ozone air quality (source US EPA, U.S. EPA, http://www.epa.gov/airtrends/ozone.html).

This downward trend continues even if you don't control for the weather, that is, the weather (a product of a warming climate) does not induce ozone increases that exceed the non-weather related declines (Figure 22).

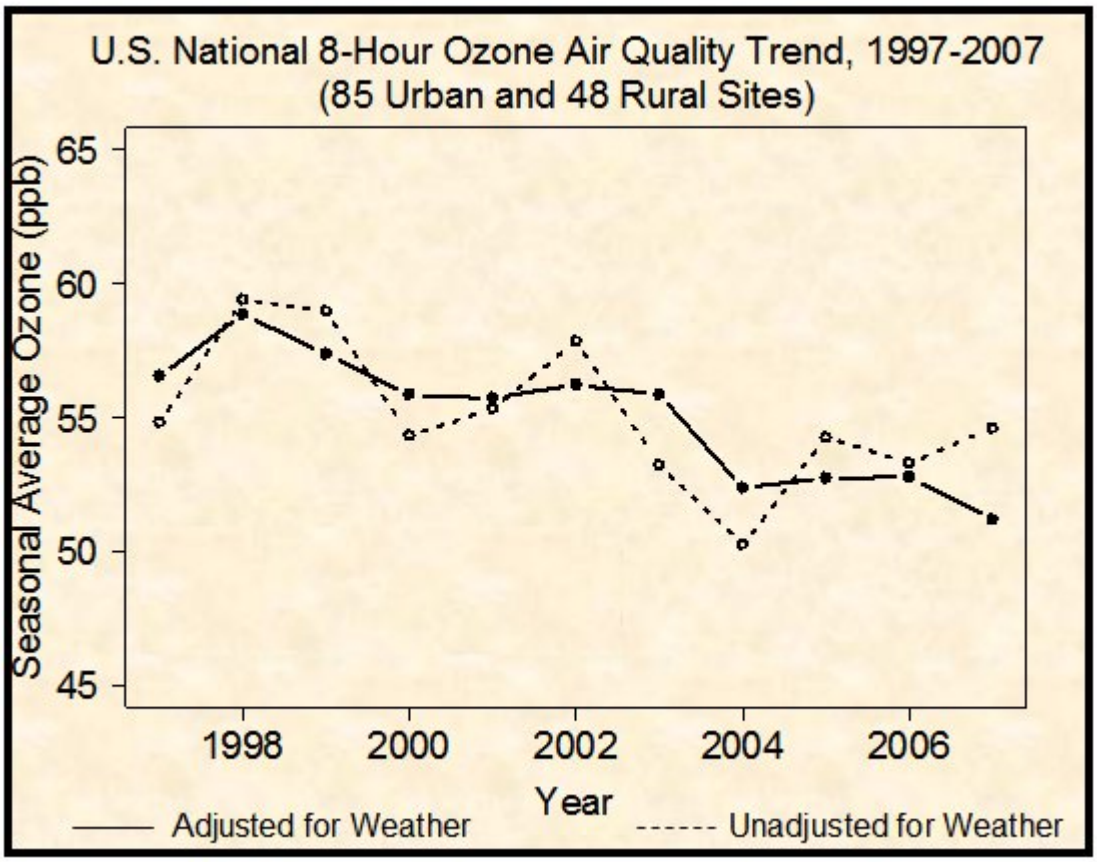


Figure 22. U.S. National 8-hour ozone air quality trend, 1997-2007, with and without adjustments for weather (source: US EPA, http://www.epa.gov/airtrends/weather.html).

Across the U.S., improving low level ozone air quality has been improving (including in our major cities like New York City which you mention in lines 35-37) despite rising air temperatures—observations which contrast with the pessimistic projections that are described (Figure 23).

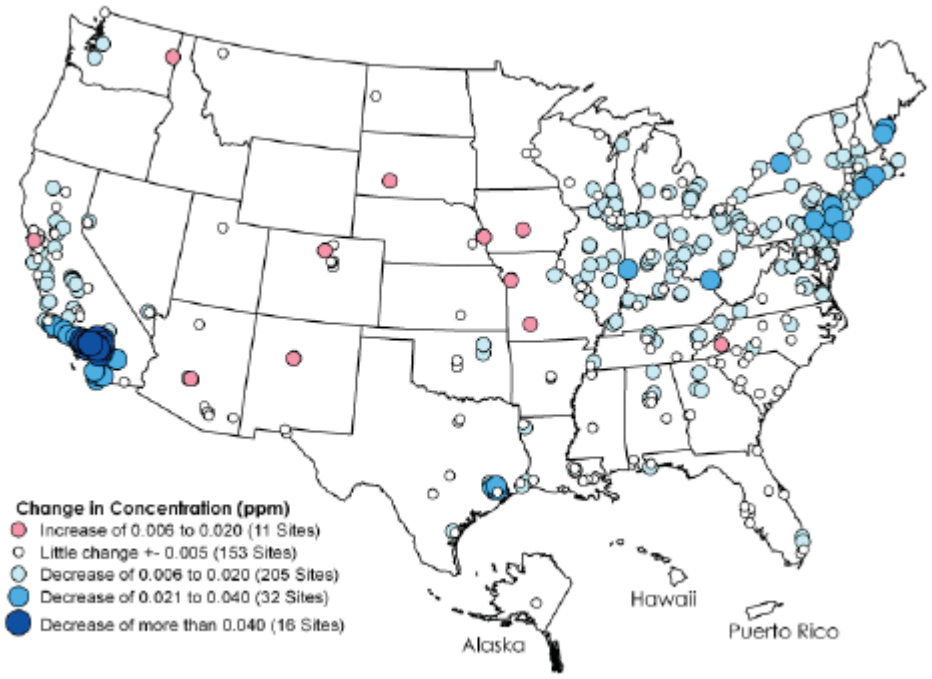


Figure 23. Change in ozone concentrations in ppm, 1990-1992 vs. 2004-2006 (3-year average of annual fourth highest daily maximum 8-hour concentrations) (source: http://www.epa.gov/air/airtrends/2007/report/groundlevelozone.pdf).

The bottom line here is that EPA's current description fails to capture the nature of observed trends in ground level ozone concentrations during a period of increasing temperatures across the U.S. and describes projections that seemingly are oblivious to the existence of such trends. As such they are of little use in addressing the potential future of low level ozone concentrations across the U.S. **Without recognizing this fact, EPA is making an endangerment finding that is not based upon the most relevant data.**

Page 71, lines 1-7

The observed warming of the surface relative to the mid-upper troposphere should decrease static stability. This is no doubt at least part of the reason for the increase in observed precipitation. A more unstable climate results in a higher mixing depth, weaker and/or more infrequent inversions and thus lower pollution concentrations. If the Brewer-Dobson circulation is indeed enhanced via increasing CO_2 levels, then the related high latitude ozone increases would a) mitigate against polar ozone depletion issues and the concomitant health impacts from surface UV and b) not necessarily result in more stratosphere-troposphere exchange. The latter occurs primarily in streamers linked to the coldest winter air masses near the polar jet—a warming of the coldest air masses, as is being observed over the Northern Hemisphere, could just as easily result in a decrease in tropospheric ozone levels.

Correction requested: The IPCC AR4 is unsettled on this issue. Delete as credible support for the TSD in this area.

Page 71, lines 41-48.

Despite the Mickley et al. *simulation*, observations show something different. Strong and Davis (2007) calculated a 0.6% increase (per decade) in the frequency of jet cores and increasing jet stream speed over much of central and eastern Canada over the period 1958-2004 in conjunction with a regional contraction of the 300 hPa circumpolar vortex. Given that there is no upward trend in pollution episodes over the last 30 years, these observations call into question the likelihood of future air quality degradation from climate change.

Correction requested: The TSD is not based on the best available science here and inappropriate for use.

Page 72, lines 1-5

Another factor to consider with respect to urban ozone levels is the possibility that precipitation will increase over urban areas because of the presence of the urban heat island coupled with a moister and more unstable atmosphere, resulting in more wet deposition. (See, for example, Shepherd, J.M., 2005).

This means less urban air pollution, thus the TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 73, lines 1-7

While it is true that elevated low level ozone concentrations have a net negative impact on most plants, EPA left out of the discussion that copious findings in the scientific literature demonstrate that elevated CO2 levels act to lessen (or even totally offset) the impact of elevated ozone levels. The mechanism is hypothesized to be that atmospheric CO2 enrichment tends to reduce stomatal conductance (which also increases the water use efficiency), which leads to less uptake of ozone into the plant which reduces the ozone damage to sensitive plant tissues. For an extensive listing of scientific papers that find that enhanced CO2 levels act to offset enhanced O3 levels please see: http://www.co2science.org/subject/o/subject_o.php

Correction requested: EPA will find substantial evidence that the TSD is remarkably incomplete concerning this subject. A full re-write is required.

Page 73, lines 46-47

Why will soils be drier given increasing precipitation and cloud cover (the latter of which mitigates against higher evaporation)? Because a model says so? When, in reality, over the last 100 years, precipitation increases have been far greater than evapotranspiration across the U.S.?

Correction requested: Relying on models that aren't working today is not a good basis for the ANPR. This section should be modified to reflect observed processes and trends.

Section 8(b) Particulate Matter

Here EPA describes current uncertainties in models and projections of particulate matter concentrations and their relationship to climate. We had a saying back in my weather forecasting days, that when the weather model uncertainty was too great to make heads or tails of the situation, we should "just look out the window." This seems to be the situation with current PM modeling. When we "just look out the window" we see that PM concentrations are trending downwards across the U.S. despite rising temperatures, largely as a result of EPA's regulatory success (Figure 24, Figure 25). Lacking any strong guidance that climate change will somehow act to more than offset the gains through regulatory actions into the future, the best indication of what is to come should be gained from what we have observed—that is, declines in PM concentrations despite increases in human greenhouse gas emissions, increasing temperatures, and increasing trends in wildfire activity. Clearly, the declining PM concentrations indicate that direct measures at controlling industrial sources of PM are far more effective at reducing PM concentrations than climate changes are at increasing them.

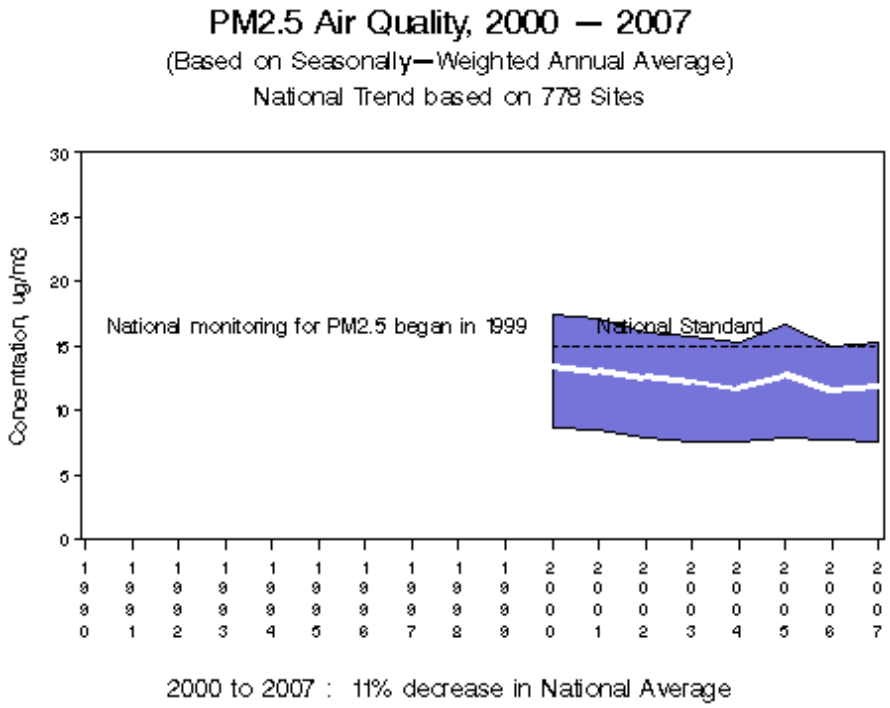


Figure 24. Trends in particulate matter (PM2.5) concentrations averaged across the U.S., 2000-2007 (source: http://www.epa.gov/air/airtrends/pm.html).

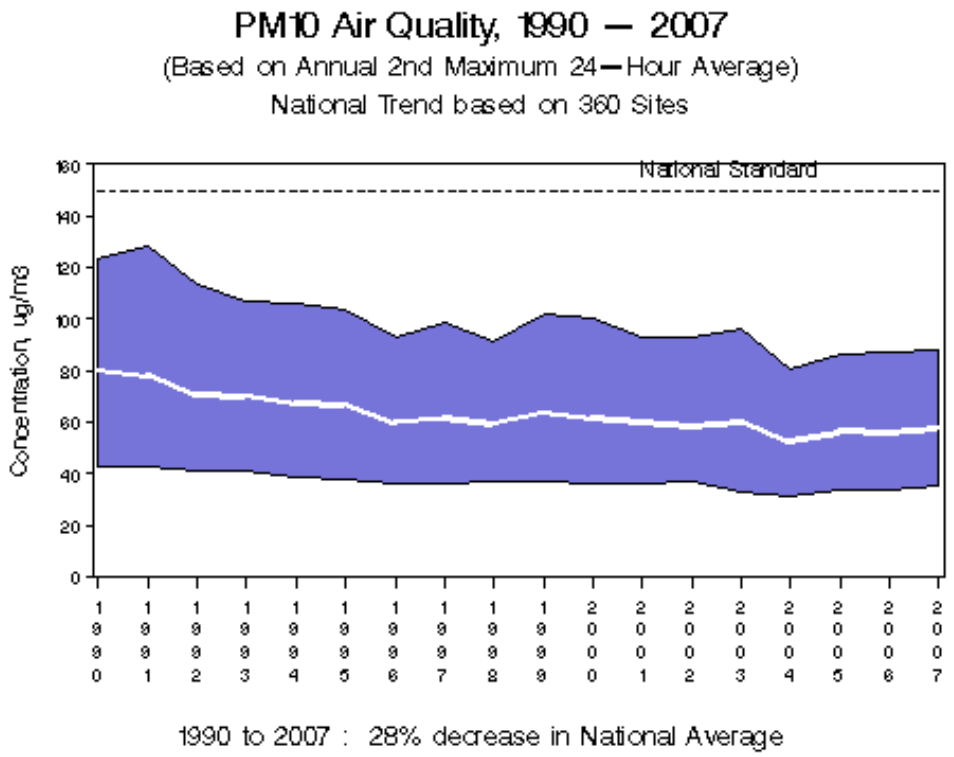


Figure 25. Trends in particulate matter (PM10) concentrations averaged across the U.S., 1990-2007 (source: http://www.epa.gov/air/airtrends/pm.html).

The EPA touts its successes at improving the nation's air quality (including ground level ozone and particulate matter) over the past several decades, even as the nation's GDP, vehicle miles traveled, and population has been steadily increasing (Figure 26). Also, the nation's energy consumption has been increasing during this time, and energy consumption equates to greenhouse gas emissions (Figure 26). So, EPA demonstrates in its own publications that air quality can be (and is being) improved despite increasing greenhouse gas emissions from U.S. and global sources and any climate changes that result, likely because increasing greenhouse gases and the resultant climate changes have exceedingly small impacts on air quality compared to direct pollution sources—see the figure above comparing the trends in ozone concentrations with and without adjustments for the weather. **This is the strongest and best guidance for the future, not the TSD.**

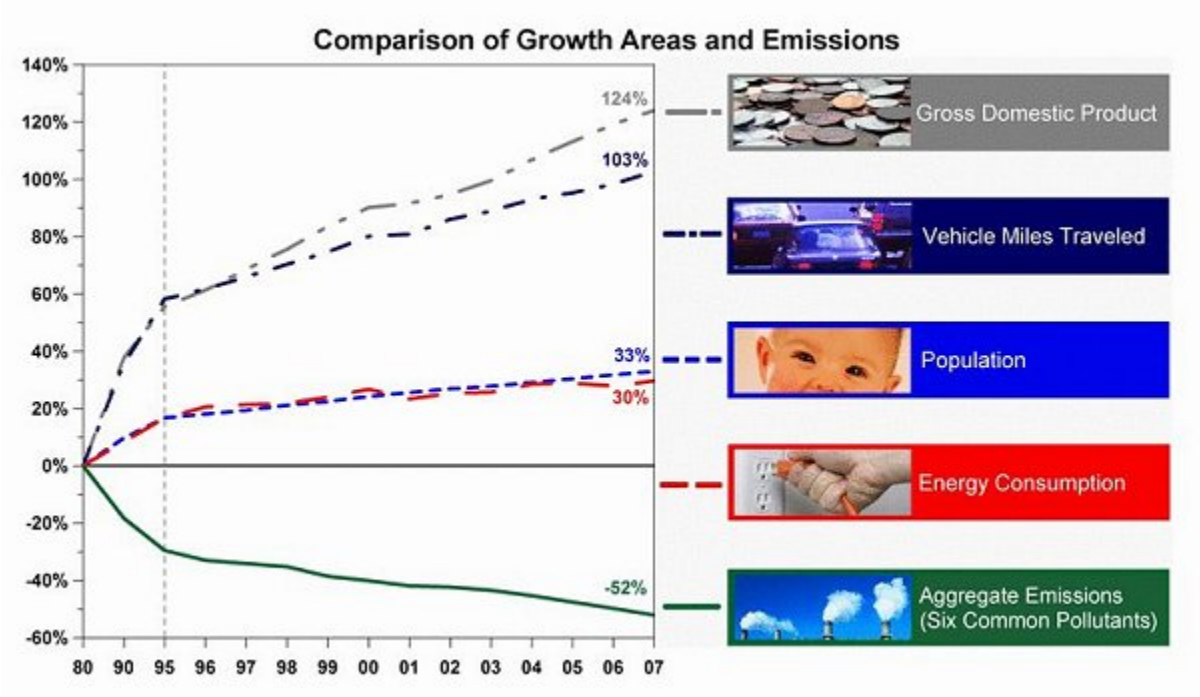


Figure 26. Air quality across the U.S. has been steadily improving even as our GDP, vehicle miles traveled, population, and energy consumption has steadily increased (source: EPA, http://www.epa.gov/air/airtrends/sixpoll.html).

Section 9. Food Production and Agriculture

Food Production and Agriculture

U.S. farmers have constantly changed their farming practices—tillage, crop varieties, irrigation, fertilizers, equipment—to maximize yield at minimized marginal cost. Most of these changes result from on-going technical advances including the development of new crop varieties that produce greater yields and are more resistant to environmental factors (including insects and weeds). These improvements are an on-going facet of modern farming and will undoubtedly continue into the future as the demands of a changing climate, changing landscape, changing food consumption patterns, etc. place a demand on the farmers of tomorrow. The best testimony to the on-going success of evolving farming practices is the continual and on-going upward trend in agricultural productivity per harvested acre (Figure 27). This increase in yield has occurred despite (or hand in hand with) the climate changes/fluctuations that have occurred across the U.S. during the past 100 years (Figure 27). Our major food crops are grown across a wide range of climate conditions—from the coastal Southeast to the plains of the Upper Midwest (Figure 28)—a range that far exceeds the changes projected from anthropogenic greenhouse gas enhancements. Our past response should well-reflect our future. Climate and weather may and do impact annual crop yields, but technological improvements continue to push our agriculture to ever higher yields. They surely will do so into the future**. What's in this to support an endangerment finding?**

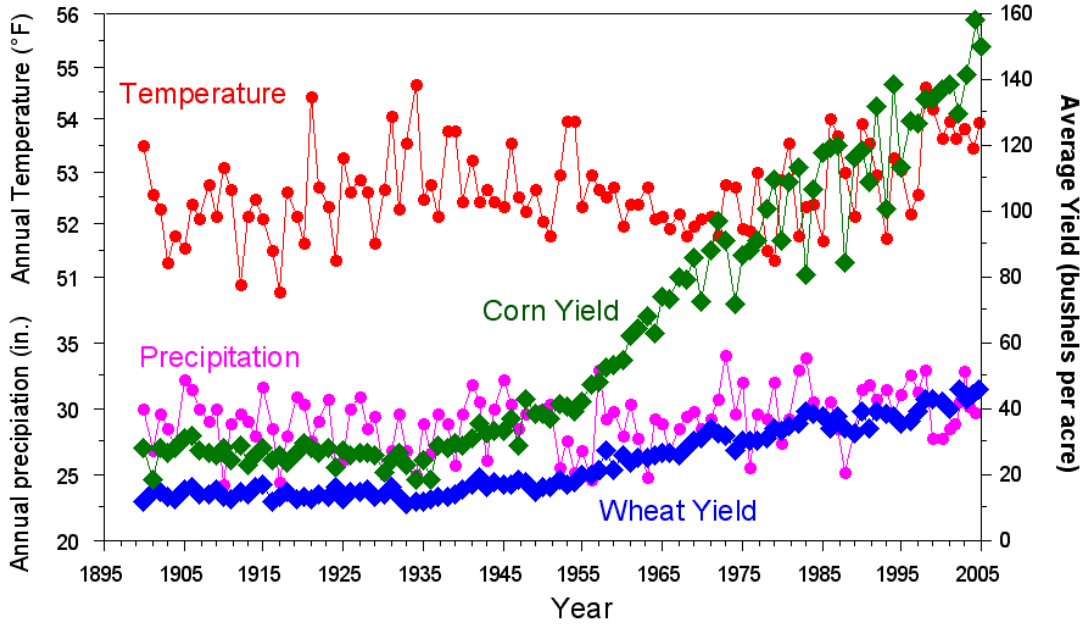


Figure 27. Yields of major cash crops such as corn and wheat show annual fluctuations as a result of weather conditions, but overall, they exhibit an upwards trends that far exceeds climate changes (data sources: NCDC, USDA).

Harvested Acreage of Major U.S. Food Crops

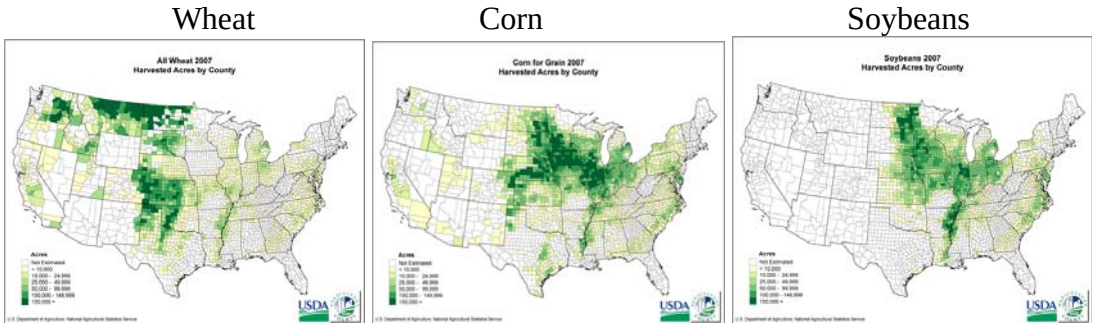


Figure 28. Our major cash crops are grown across a large range of climate conditions, an indication that climate changes can be readily adapted to (data source: USDA).

The TSD is not scientifically thorough or complete here. An updated discussion of the relevant observations and crop science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 77, lines 26-33

These "bullet" items do not represent a fair assessment of the climate changes on crop production across the U.S. The first one only concerns moisture excesses (no discussion of moisture deficiencies) and the last two only concern California.

Revise to include an accurate and more complete assessment.

Page 77, lines 40-42

EPA notes a possible tougher time for California wine grapes, but make no mention as to how Oregon wine grapes may fare. Will the same higher temperatures that may limit California crops enhance them further north along the coast?

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 78, lines 24-31

EPA left out of this discussion the positive impact that CO2 has on plants' water use efficiency. Plants grown under elevated CO2 conditions consistently show significantly improved water use efficiency meaning that their production/water demand ratio is lessened. This will have an impact of future irrigation requirements.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required that examines the topic of water use efficiency. As it stands now, it is insufficient for basing an endangerment finding.

Page 78, Section (c), Climate variability and extreme events

Obviously, any future scenario that increases the frequency and/or intensity of deleterious (climate) events will have negative impacts. The problem comes in assessing whether or not such scenarios represent likely outcomes as well as determining natural climate variability from that forced by anthropogenic emissions of greenhouse gases. For instance, where is the observational evidence that anthropogenic greenhouse modifications are leading to an enhanced frequency or deleterious climate events in the United States? Without such evidence to corroborate climate model projections, the model projections cannot be relied upon as trustworthy purveyors of future expectations.

Let's take the example of projections of increased variability of drought and floods. Where is the corroborating evidence? Downton et al. (2005), find that after accounting for changes in population demographics and wealth, there is no detectable increase in flood losses (they actually found a slight statistically insignificant decline)—in the face of rising temperatures. Small et al. (2006) find no trends in annual highest stream flow (that associated with flood events) despite increases in precipitation across the Eastern U.S.—primarily because precipitation has increased during the fall (they time of lowest annual stream flow) but has not done so during the spring (the time of the year with highest

stream flow). Thus the trend toward increasing precipitation has resulted in higher low and mean stream flow (beneficial flow rates) and not an increase in high stream flow (potential for flooding). In a subsequent analysis, Small and Islam (2007) found that the increase in fall season precipitation was caused by an increase in the number of events, not in the magnitude of events (another apparently beneficial trend). None of these studies was included in the IPCC AR4 WGI report and yet they all are directly applicable to U.S. climate change and impacts and should be included in this EPA document.

McCabe et al., (2004) specifically examined the trends in the variability of drought frequency (as captured by the Palmer Drought Severity Index) in across the United States and found that the majority of the long-term variability is explained by atmospheric/ocean cycles over the Pacific and Atlantic Oceans (specifically, the PDO and the AMO). They found less than one-quarter of the long-term variability of PDSI across the U.S. was related to large-scale upward trends in temperature, and that the pattern of drought across the U.S. related to the long-term warming trend was "a complex pattern of positive and negative trends in U.S. drought frequency over the 20th Century"—not one which clearly indicated a propensity of increased drought frequency, in net (Figure 29).

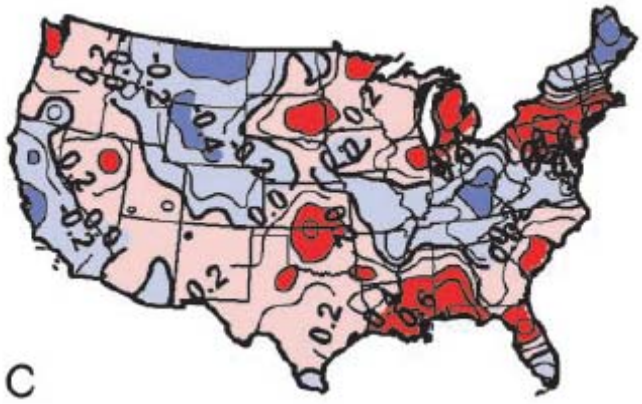


Figure 29. Pattern of PDSI trends associated with trends in Northern Hemispheric temperatures. Red areas indicate regions of the U.S. in which drought frequency increases as NH temperatures increase, while blue areas indicate regions where drought frequency decreases as NH temperature rise. There is no clear tendency for a net increase in drought frequency averaged across the U.S. as NH temperatures rise (source McCabe et al., 2004).

New research (published subsequent to the IPCC AR4) on the origins of the AMO (Zhang, 2007; Zhang, 2008) concludes that the AMO is largely a natural phenomenon not related to human atmospheric emissions as erroneously concluded by Mann and Emanuel (2006) (see Zhang, 2008). This serves as further confirmation that the PDSI variability attributed to the AMO by McCabe et al. (2004) is not related to human greenhouse emissions).

Seager et al. (2008) report that recent droughts in the southeastern U.S. do not show any indication of anthropogenic influences, and that future model scenario present no useful guidance for the directions of future trends.

Overall, the IPCC AR4 Chapter 3 (Trenberth et al., 2007) reported that the last quarter of the 20th century in the U.S. was a relatively wet one (despite the drought in the southwestern U.S.):

> A multi-decadal period of relative wetness characterised the latter portion of the 20th century in the continental USA, in terms of precipitation (Mauget, 2003a), streamflow (Groisman et al., 2004) and annual moisture surplus (precipitation minus potential evapotranspiration; McCabe and Wolock, 2002).

The bottom line is that there is little evidence to suggest that projections of increasing extreme weather events (specifically drought and floods) are in line with the observations during the recent period or rising temperatures in the U.S. Thus statements like (Page 78, lines 44-46) "The adverse effects on crop yields due to droughts and other extreme events may offset the beneficial direct effects of elevated CO2, moderate temperature increases over the near term, and longer growing seasons" is hypothetical and without grounds in the observational scientific literature—either that included in the IPCC AR4, or published thereafter. **There is no support for the ANPR endangerment finding in this area when broader science is considered.**

The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date and not scientifically complete on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 79, lines 1-7

EPA describes the risks associated with climate here, not climate *change*.

Update the TSD to appropriately differentiate between expectations and magnitudes of risk related to our climate versus those related to climate change.

Page 79, lines 10-12

As we have discussed, the projected trends do not square with the observed trends in either flooding (Downton et al., 2004; Small et al., 2006) or droughts via precipitation variations modulated by anthropogenic greenhouse gas emissions (Small and Islam, 2007; Seager et al., 2008).

The IPCC AR4 document does not adequately review the topic of precipitation variability and its causes across the U.S. and thus should not be relied upon to underlay EPA's conclusions on this topic.

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required.

Page 79, Section 9(d), Pests and Weeds

This section entirely ignores a discussion about the development and incorporation of herbicide and insect resistant crop varieties that improve the farmers' ability to control for insects and weeds impacting their crops.

In the U.S. the adoption of crops engineered for these attributes is steadily rising and should continue to do so into the future (Figure 30). Thus, as in all aspects of agriculture, technological advances continue to improve farmers' ability to grow productive, healthy food crops in ample supply.

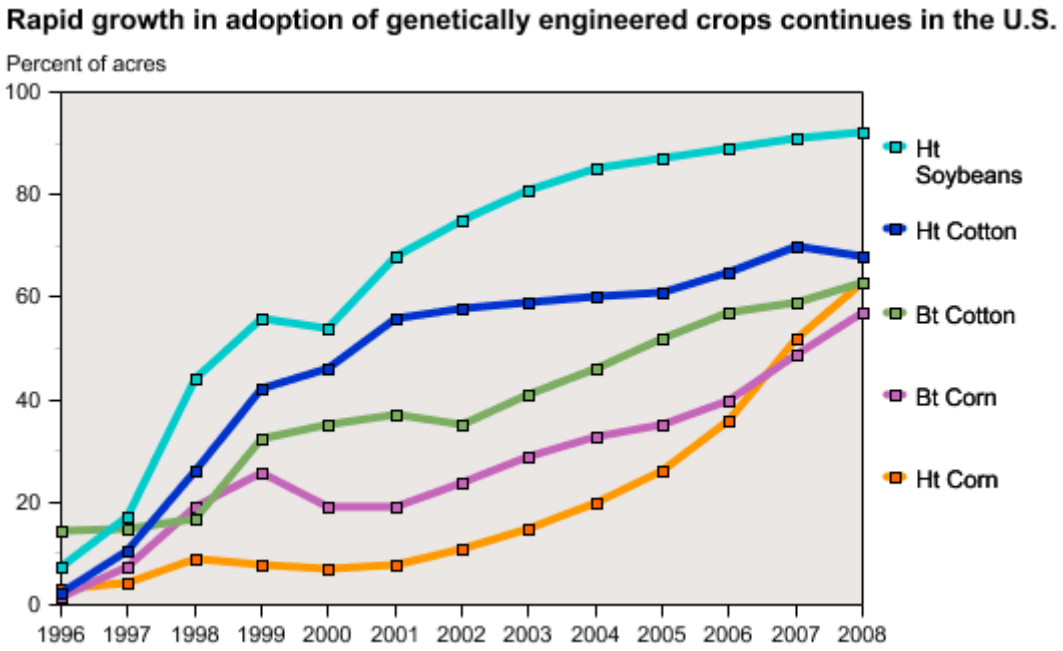


Figure 30. Trends in adoption of genetically engineered herbicide resistant (Ht) and Insect resistant (Bt) crops in the U.S. (source: Economic Research Service of the USDA, http://www.ers.usda.gov/data/biotechcrops/).

Ignoring technological improvements in farming practices will insure that you greatly underestimate farmers' abilities to respond to changing environmental conditions, and as a result, will produce a far more too pessimistic outlook for future crop production. This seems to be this case in this section.

Correction requested: The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 79-80, Section 9(e), Livestock

Again, no analysis of trends in livestock production is done to assess how U.S. livestock productivity has fared during the past decades of increasing atmospheric CO2 and

increasing temperatures in the U.S. ***Why? Because, such an analysis would surely have shown that the production of livestock products has been increasing***, and it is projected to continue to increase into the future (see http://www.ers.usda.gov/Briefing/baseline/livestock.htm). One near-term concern in livestock production is the slowdown that is anticipated to occur as a result of livestock feed being diverted to ethanol production!

According to the USDA ("Agricultural Baseline Projections: U.S. Livestock, 2008-2017", http://www.ers.usda.gov/Briefing/baseline/livestock.htm):

> Projections for the livestock sector include production adjustments in response to high grain and soybean meal prices resulting from the expansion of corn-based ethanol production. Returns to U.S. meat and poultry production fall levels in recent years [sic], slowing increases in or reducing production of all meats over the next several years.

Thus, it is not climate change that is negatively impacting productivity, but our attempts at trying to alleviate it! We can only imagine that such a negative impact to America's livestock farmers will also follow any decision by the EPA to restrict greenhouse gas emissions under the Clean Air Act. ***There is certainly no support for the ANPR in the agricultural data.***

Page 80, lines 43-45, "Coral reefs, for example…"

This, and virtually all discussion on the impacts of climate change (and warmer oceans) on coral reefs, focuses on bleaching issues and completely ignores the potential for northerly range expansion to the Gulf Coast and up along the U.S. southeastern coast. There is documented evidence that this expansion has occurred during prior Holocene warm periods and furthermore, is occurring currently (Precht and Aronson, 2004). Precht and Aronson write:

> There is mounting evidence that coral species are responding to recent patterns of increased SSTs by expanding their latitudinal ranges…In Florida, numerous thickets of staghorn coral, some up to 700 m2 in area, are now established north of their previously known range. Detailed studies documenting the composition, structure, and reproductive viability of these populations have been conducted in seven of these thickets. Elkhorn coral has also been observed colonizing shallow reef areas north of extant populations. The two Acropora species have expanded more than 50 km northward in just the last few decades.

This on-going range expansion could continue into the future as ocean temperatures continue to rise. This could have the effect of expanding the possibilities of reef-based fisheries into more northerly U.S waters. Yet this very real possibility is not included in the EPA discussion—but it should be. How else can a proper determination of the role

that coral reefs and reef-based fisheries may play in any future "endangerment" from the impact of greenhouse gas emissions?

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Section 10. Forestry

Page 81, lines 25-26: "Climate change has very likely increased the size and number of forest fires….in the interior West, the Southwest, and Alaska…"

The CCSP document, which is referenced as the basis for this, cites Westerling et al. (2006) as the source. Westerling et al. believed that earlier snowmelt is the cause, noting that wildfire frequency since 1986 is four times above the average from 1970 through 1986. In fact, in the same paper, Westerling et al. give the timing of spring snowmelt, and it is obvious that there is no trend towards earlier melt (Figure 31)

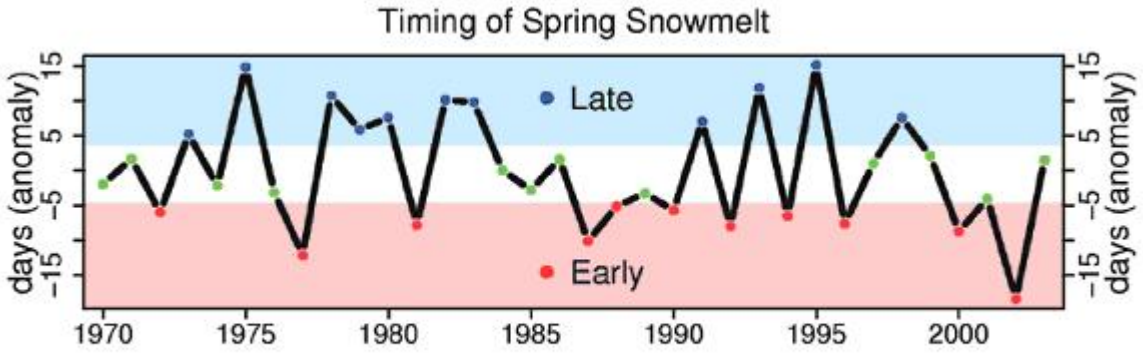


Figure 31 Timing of spring snowmelt in the western U.S. (the more negative the value, the earlier in the year the spring snowmelt occurred) (From Westerling et al., 2006)

In addition, there is no correlation between global surface temperature and the Palmer Drought Severity Index (a common measure of aridity) in the western U.S. Figure 32 demonstrates this, using data from the IPCC and the National Climatic Data Center.

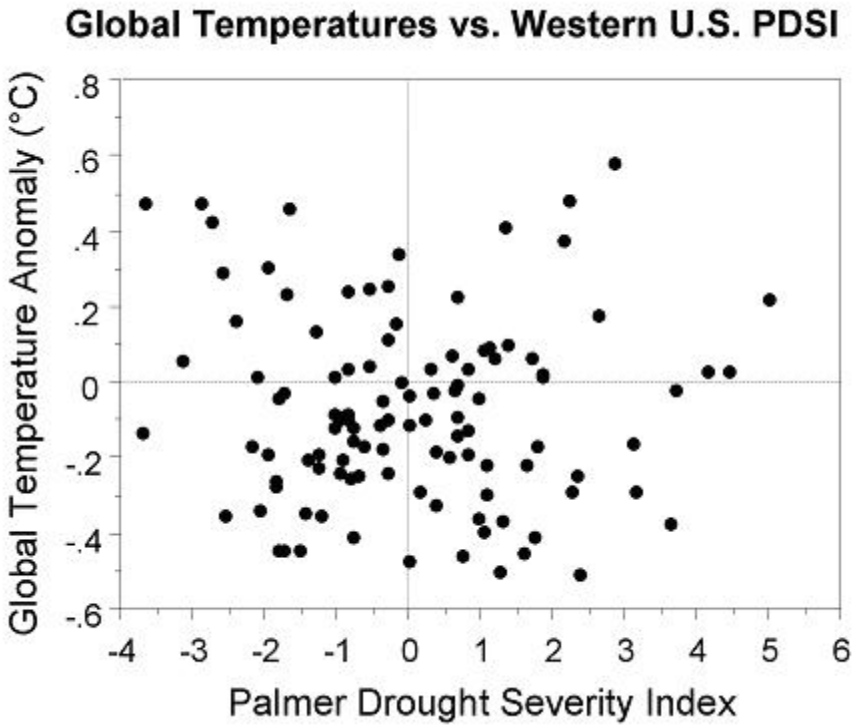


Figure 32. Scatterplot of global average temperature anomaly vs. western PDSI for the period 1895-2005.

Further, Cook et al., (2004) reconstructed western drought history back to 800 A.D. and found that "Compared to earlier megadroughts that are reconstructed to have occurred around AD 936, 1034, 1150, and 1253, however, the current [western] drought does not stand out as an extreme event…" Given that CCSP accepts Mann's "hockey stick" representation of the climate of the last millennia, this means that a world of lower mean surface temperature was clearly capable of producing more extreme western drought.

The CCSP is therefore not a reliable source on the historical relationship between global temperature, drought, and western fires and should not be used as a source for the ANPR endangerment finding.

Page 82, lines 39-44

This paragraph speaks equivocally about the effects of climate change on forests, despite the availability of reliable data sensed from satellites. Interestingly, the multiply-cited IPCC WGII report section on forest products (Easterling, et al., 2007) somehow ignored the work of Nemani et al. (2003) showing an enhancement of *global* vegetation in the last two decades. They ascribed their findings to the direct effect of increasing atmospheric carbon dioxide, and to warming temperatures.

The omission of this both in this paragraph and in the general background reference (Easterling et al.) means that neither the CCSP nor the IPCC documents provide

complete information for the TSD, and therefore for the ANPR endangerment discussion. The TSD needs to be revised to include the appropriate science.

Page 83, lines 31-32: "Earlier spring snowmelt….where the increase in wildfire activity has been greatest".

That is from Westerling et al. (2006), shown above. It is clear that there is no trend whatsoever in their snowmelt data.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 83, line 33: "In the south-western U.S., fire activity is correlated with higher Palmer Drought Severity Indices".

This in fact is taken from TSD's primary citation of Field et al., 2007, which is the section "North America" from the IPCC WGII report, which also says "In the south-western U.S., fire activity is correlated with….higher Palmer Drought Severity Indices", *and no reference is given*. Note that "higher" values of this index are "wetter," so both the TSD and the IPCC are claiming that wetter conditions yield more fires, even though they are predicting drier conditions! However, McKenzie et al. (cited earlier in the IPCC sentence before the assertion about the Palmer index) say this (*Conservation Biology*, 2004): "In the American Southwest, for example, large fire years are associated with current-year drought but wetter-than-average conditions in the five previous years."

At the same time, the TSD predicts increasing frequency of *drought* in the south-west caused by lower rainfall and higher temperatures. This would argue for *fewer* large fire years, which is hardly evidence that supports the ANPR endangerment finding.

Correction requested: The TSD needs to note inconsistency between fire and climate projections. As it stands now, it is insufficient for basing an endangerment finding.

Page 83, lines 35-37

" …current modeling studies suggest….elevate[d] fire risk with increased aridity." But a higher Palmer Index means increased wetness, which the TSD claims means more fires. Again, there is an internal inconsistency here.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 84, lines 17-20: "Bioclimatic modeling…suggests that, over the next century, tree species richness will decrease in most parts of the coterminous U.S. even though long-term trends (millennia) ultimately favor increased richness in some locations".

On the whole, there is no quantitative analysis given here of the net change in richness, and therefore no support for the ANPR.

Section 11. Water Resources

Page 86, lines 11-17

This paragraph talks about "North America's over-allocated water resources," lowering the "seasonal availability of water" because of declining snowpack and increasing evaporation, higher demand from demographic and economic factors, and lower levels of the Great Lakes and major river systems. The primary citation is Field et al., 2007, which is the "North America" chapter from IPCC WG II.

Any inspection of satellite imagery will reveal that North America, of all continents, is particularly awash in fresh water, almost all of which isn't allocated to anything but its own lake or river. If water were in fact to be in sufficiently short supply, two things must happen. First, the price of water will rise. Then there will be incentive to capture and sell water from the massive supply available in northern North America. This is so obvious as to be completely fatal to the credibility this particular citation of the IPCC, and renders the core document for this chapter, as cited, as clearly an inadequate basis for the ANPR.

If water were so limiting, how could Clark County/Las Vegas, Nevada support a population of 1.5 million and be the fastest growing urban region in North America? Obviously there is plenty of room for adaptation and efficiency with regard to water.

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 86, lines 39-42, especially, "April snow water equivalents have declined 15-30% since 1950 in the western mountains of North America".

This is taken from Field et al., 2007, again, which is the "North America" chapter of IPCC WG II. It is also reproduced graphically in the lower left of Figure 14.1 in the TSD, page 106. *There is obviously no significant trend in this time series*. Picking on 1950 as a start data is equally absurd, as there are clearly lower snow water equivalents in the decades prior to this.

This is a prime example of the inadequacy of the IPCC review process, and undermines the credibility of the IPCC report as the basis for this section of the

TSD, which, in turn, undermines the credibility of the TSD as the basis for the ANPR endangerment finding.

Below (Figure 33) are snow water equivalent histories from Cayan (*Journal of Climate*, 1996). It is also apparent here that these values were very low in the early 20th century and that there is simply no net trend.

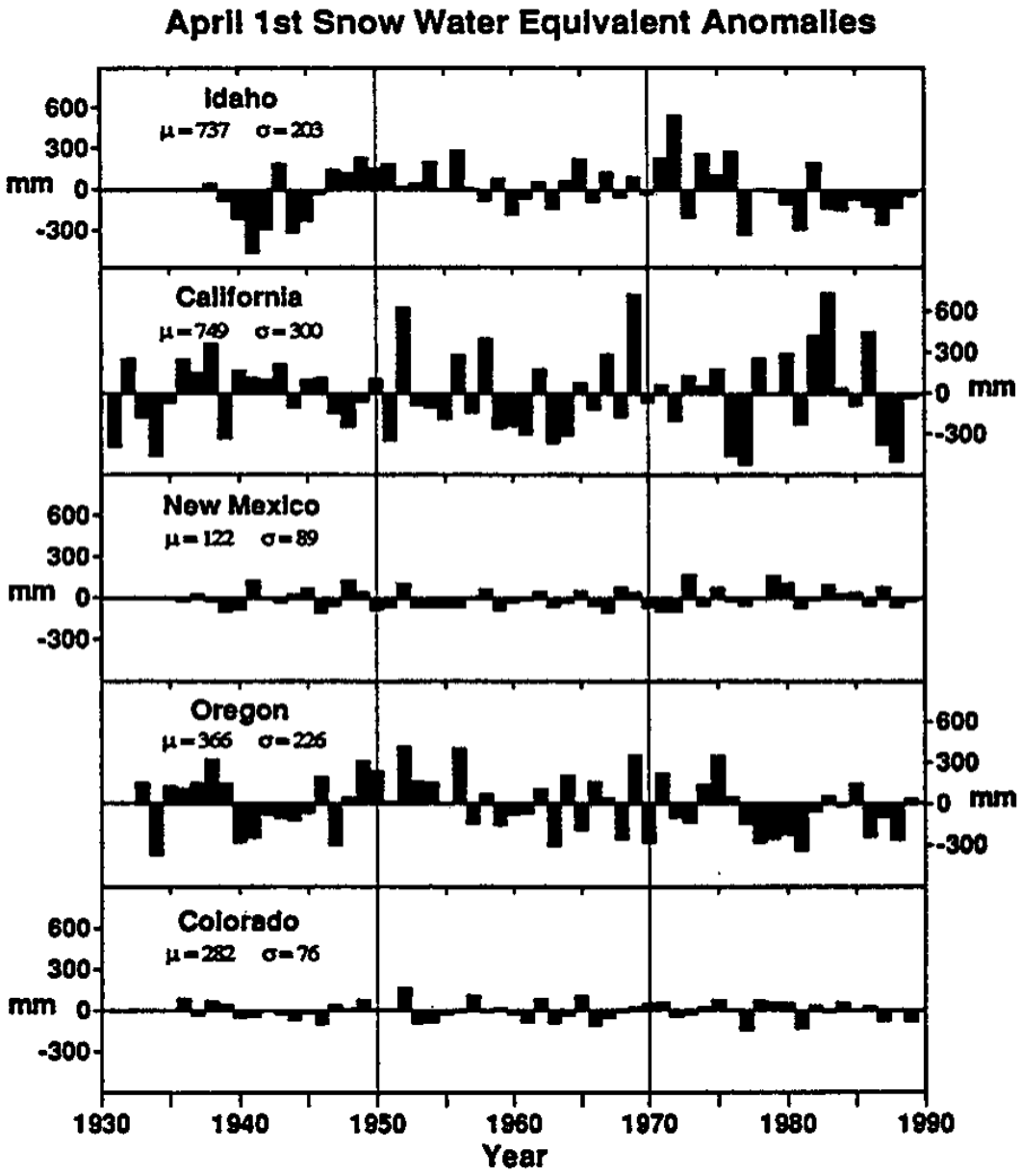


Figure 33. Average April 1st snow water equivalent anomalies in western U.S. (source: Cayan, 1996)

Correction requested: The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4

on this topic. A more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding*.*

Page 87, lines 1-2: "In the Arctic, precipitation has increased by about 8%..."

How can this serve as the basis for the ANPR endangerment finding? Canada is a huge net exporter of hydropower, and much of the water for that is ultimately of arctic origin. Hydropower itself, is often (erroneously) called a "renewable" resource, but it clearly does not increase emissions of carbon dioxide.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding*.*

Page 87, lines 7-9: "These changes to Arctic precipitation…act as climatic feedbacks (primarily by changing surface albedo), and have socio-economic impacts"

This is not the first time that the climate has this warm in the Holocene. Postglacial warming was Arctic-wide. Darrell Kaufman (2004) noted that for 2,000 years—from 9,000 to 11,000 years ago, Alaskan temperatures averaged 3°F higher than now. He found that there have been three similarly warm periods in Alaska: AD 0 to 300, 850–1200, and 1800 to the present. Thompson Webb III et al. (1998) found timings similar to MacDonald et al (2000): northwestern and northeastern North America were more than 4°F warmer than the baseline from 7,000–9,000 and 3,000–5,000 years ago, respectively.

The Inuit culture is thought to have migrated into North America about 8,500 years ago, or during a time that was warmer than present. Obviously it is perfectly capable of adapting to current temperatures. The lack of historical perspective in the TSD reduces its credibility as a basis for the ANPR.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding*.*

Page 87, lines 16-35

Again, this implies little or no infrastructural or technological adaptation. Given that societies clearly adapt to increasingly frequent urban heat waves (see comments above), why would they not adapt to more frequent shortages of water by increasing catchment or tapping the virtually limitless amount of fresh water from higher latitudes? These are projects that would evolve over decades, not years, but the fact that the TSD makes little mention of this.

The TSD is not scientifically complete here and must be revised. As it stands now, it is insufficient for basing an endangerment finding*.*

Page 89, lines 7-12

Citing the IPCC and the CCSP, TSD states that water quality is sensitive to increased water temperatures. The context is in terms of water systems for human and economic use.

People have maintained high-quality municipal water systems from the tropics to polar latitudes. Obviously the technologies are already in existence to cope with higher temperatures in the U.S. **Again, the TSD seems to ignore obvious adaptation. With regard to the ANPR, one must compare the enormous costs of substantial carbon dioxide limitations to the relatively small ones of natural adaptation to slowly changing conditions.**

The TSD is not scientifically complete here because the IPCC AR4 and the CCSP are not scientifically complete. Therefore the TSD should not rely on the IPCC AR4or the CCSP on this topic. A more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 90, lines 34-39

This paragraph comments on increased flash flooding and urban flooding. It ultimately is based upon Figure 10.12 in the IPCC WG I (page 769). In the coterminous U.S., the composite models vary in their daily runoff estimate from between -0.1 to + 0.1 mm/day, or an annual change of +/- 36.5 mm (1.43 inches). This is a truly insignificant amount, not even likely to be detected, and not exactly what creates a memorable flood! Further, as Figure 10.12 shows, the models reach the 80% same-sign criterion in only a very small portion of the southwestern coterminous U.S.

Therefore the TSD is using a citation that indeed shows very little change and does not support and endangerment finding here.

Page 91, lines 26-30

This paragraph talks about how climate-induced environmental change will affect "park tourism, winter sport activities, inland water sports and other recreational activities dependent upon precipitation."

The diversity of recreational activities across the U.S. is substantial, with some enhanced by snow and rainfall, while others are negatively affected. People will substitute—it should come as no surprise that many ski resorts have changed to "four season" destinations by adding lower-elevation golf courses. It's hard to see baseball and softball being negatively affected by warmer weather and slight changes in rainfall. This paragraph is so one-sided that it makes the TSD appear biased and therefore inappropriate backing for the ANPR.

Section 12. Sea Level Rise and Coastal Areas

Page 92, line 32, "…projected increases in storm intensity…"

The IPCC AR4 conclusion of projected increases in storm intensity has been supplanted, as I have discussed previously, by more recent findings which do not suggest such a change (e.g., Vecchi and Soden 2007a, Vecchi and Soden 2007b, Knutson et al., 2008; Vecchi et al. 2008), and **as such the IPCC AR4 should not be relied upon to support this aspect of the TSD on endangerment.**

Page 92, line 40, "Although climate change is impacting coastal systems, non-climatic human impacts have been more damaging over the past century."

What is the reference for the assertion that (presumably human-caused) climate changes have been damaging coastal systems (as you imply)? The sea level has been rising rather steadily (or at least without a clear human signal in its variability) for more than 100 years (e.g. Holgate, 2007), hurricanes go through natural cycles (e.g., Goldenberg et al., 2001; Zhang, 2008), as do sea levels along the East Coast (e.g., Kolker and Hameed, 2007), etc. Where is the robust evidence (that is, clearly discernible from the long list of other damages you describe) for damages from human-caused climate changes?

An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 93, lines 1-2

The "cumulative effect of these non-climate, anthropogenic impacts" increase the vulnerability to coastal systems much more to the impacts of climate than to climate *change*. For instance a hurricane hitting a coastal area today will have a much larger impact than one which hit the same area 50 years ago. This is an effect of climate, not climate change (Pielke Jr. et al., 2008). The impact of climate change, which perhaps may add a mile per hour or two to the hurricane winds has a much less effect than the climatological existence of the hurricane hitting the built-up coast does. In fact, the ratio of climate impacts as a result of demographic changes to climate change impacts is enormous. Pielke Jr (2007) analyzed the climate vs. climate change impacts of future tropical cyclones and concluded the following:

> This paper examines future economic damages from tropical cyclones under a range of assumptions about societal change, climate change and the relationship of climate change to damage in 2050. It finds in all cases that efforts to reduce vulnerability to losses, often called climate adaptation, have far greater potential effectiveness to reduce damage related to tropical cyclones than efforts to modulate the behaviour of storms through greenhouse gas emissions reduction policies, typically called climate mitigation and achieved through energy policies. The paper urges caution in using economic losses of tropical cyclones as justification

> for action on energy policies when far more potentially effective options are available.

Your comments about vulnerability are mis-focused and therefore cannot be used as support for the ANPR.

Page 93, lines 6-10

Again, you mis-focus on climate *change* when, in fact, climate processes themselves "affect sediment transport in complex ways" that are far from being well-understood.

Page 93, 21-22 "…an acceleration in the rate of sea-level rise may mean…"

It has not yet been firmly established that an acceleration of the long-term rate of sea level rise has been occurring, much less tied to anthropogenic climate change. Quoting this TSD from page 30 lines 3-4 "It is unclear whether the faster rate for 1993 to 2003 is a reflection of short-term variability or an increase in the longer-term trend." Also, as we previously discussed, several papers published subsequent to the IPCC AR4 confirm the uncertainty of a recent acceleration in the established rate of sea level rise, for example see Holgate (2007), Willis et al. (2008), or Wunsch et al. (2007) who concluded:

> At best, the determination and attribution of global mean sea level change lies at the very edge of knowledge and technology… It remains possible that the database is insufficient to compute mean sea level trends with the accuracy necessary to discuss the impact of global warming—as disappointing as this conclusion may be. The priority has to be to make such calculations possible in the future.

This is yet another example of where recent research adds more complexity and **tells a different story that that published in the IPCC AR4, and demonstrates why the IPCC AR4 should not be relied upon to make an Endangerment finding.**

Page 93, Lines 37-38 "According to the IPCC, most of the world's sandy shorelines retreated during the past century and climate change induced sea-level rise is one underlying cause."

You lead with this sentence and then proceed with a litany of erosion losses along the U.S shoreline. Yet you never quantify how much of the shoreline change is directly caused by anthropogenic climate changes. The on-going rate of sea level rise, established in the mid-1800s (long before human activities could have influenced it), is responsible for the 50% loss in salt marsh in the U.S.? The 90% of Louisiana that is eroding at 12m/yr?

The rerouting of the Mississippi River by the Army Corps of Engineers is responsible for much of the shoreline loss there. How much does the anthropogenic portion of observed climate change over the past 100 years contribute the rates of erosion? If you can't

quantify it or separate it from other well-known influences, you can't very well assess it or use it to support the ANPR.

The TSD is not scientifically thorough here. An updated discussion of the relevant science is required. As it stands now, it is inappropriate for basing an endangerment finding.

Pages 93-94, lines 43 (pg. 93) to line 2 (pg. 94)

So, no matter what climate change may bring to the Great Lakes, you've already set it up that it going to be bad—higher water levels are bad, lower water levels are bad…what if future variations in Lake levels are of similar timing and magnitude of past variations? Would you *a priori* blame them on anthropogenic climate change and thus claim a negative impact? How do you discern future climate *change*-related impacts from future climate-related impacts?

Below are the historical water levels from the Great Lakes according to NOAA's Great Lake Environmental Research Laboratory (Figures 34-38). Clearly, there is a lot of natural variability in the Lake system. So discerning an anthropogenic climate change signal is going to prove to be quite difficult.

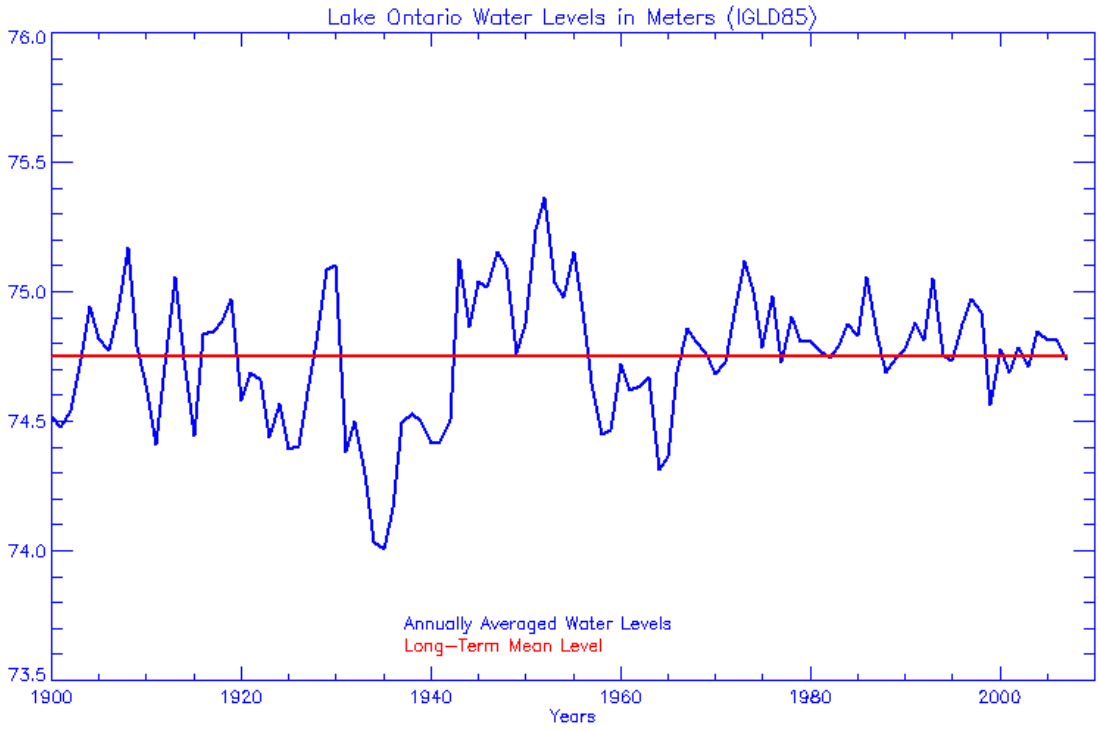


Figure 34. Lake Ontario water levels (source, NOAA GLERL, http://www.glerl.noaa.gov/data/now/wlevels/lowlevels/plot/Ontario.gif)

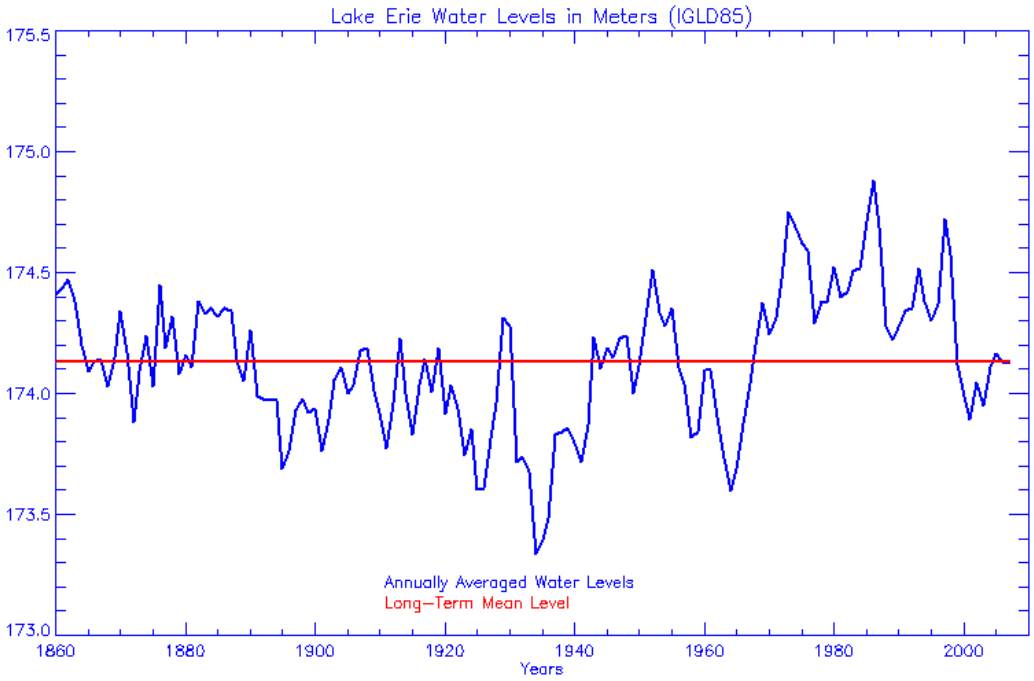


Figure 35. Lake Erie water levels (source, NOAA GLERL, http://www.glerl.noaa.gov/data/now/wlevels/lowlevels/plot/Erie.gif)

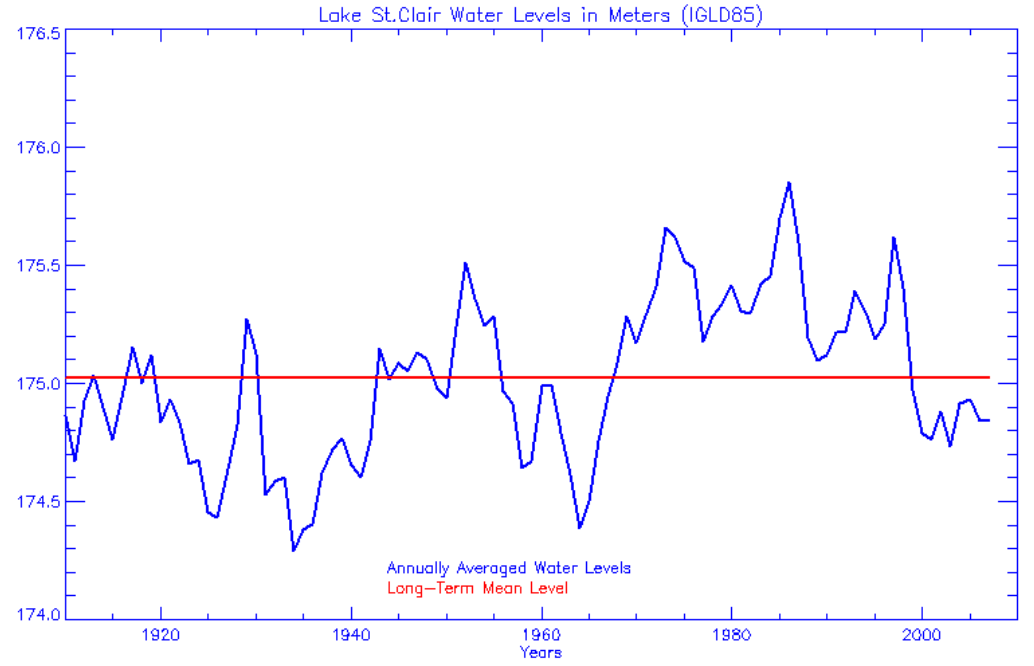


Figure 36. Lake St. Clair water levels (source, NOAA GLERL, http://www.glerl.noaa.gov/data/now/wlevels/lowlevels/plot/St.Clair.gif)

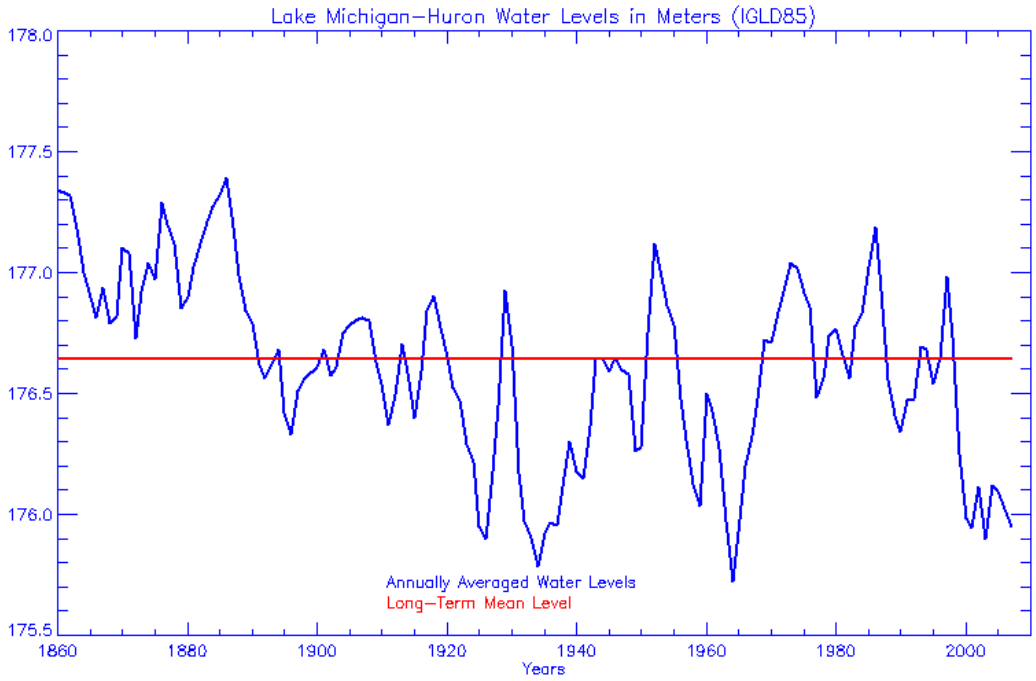


Figure 37. Lake Michigan/Huron water levels (source, NOAA GLERL, http://www.glerl.noaa.gov/data/now/wlevels/lowlevels/plot/Michigan-Huron.gif)

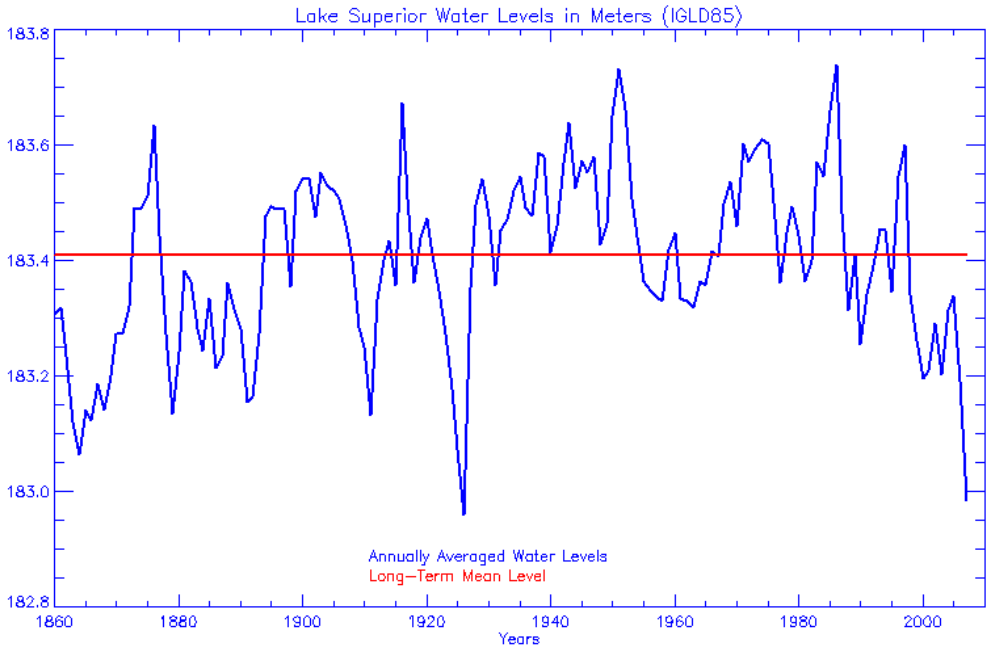


Figure 38. Lake Superior water levels (source, NOAA GLERL, http://www.glerl.noaa.gov/data/now/wlevels/lowlevels/plot/Superior.gif)

And Lake Superior has recovered in recent months (not included in Figure 38) to levels much nearer the long term mean (Figure 39).

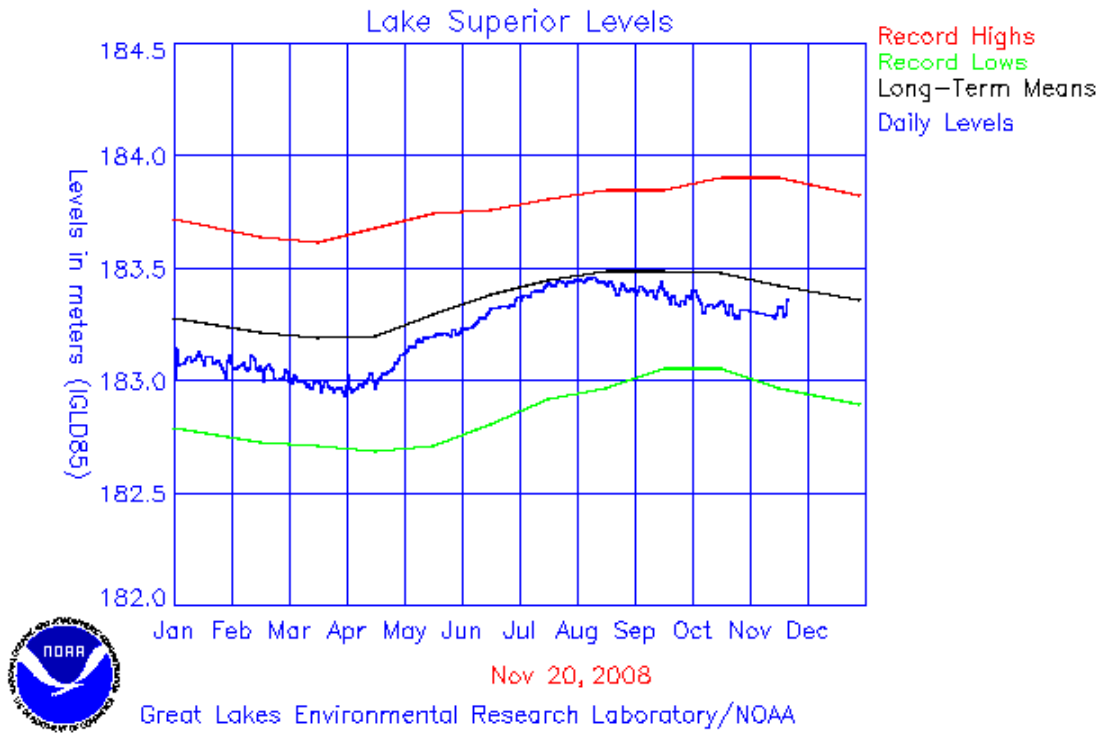


Figure 39. Lake Superior water levels, 2008 (source: NOAA's GLERL, http://www.glerl.noaa.gov/data/now/wlevels/sup_lvl.gif)

The TSD is not scientifically complete here in that it lacks a through discussion of natural variability. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 94, lines 6-7

Shoreline erosion has been documented to have been taking place in Alaska for many decades, oftentimes forcing the relocation of both native and non-native settlements. Numerous papers were published back in the 1960s and 1970s describing these processes. Here are examples of some of them:

Harper, J.R., 1978. Coastal erosion rates along the Chukchi Sea coast near Barrow, Alaska. *Arctic*, **31(4),** 428-433.

Hartwell, A.D., 1973. Classification and relief characteristics of northern Alaska's coastal zone. *Arctic*, **26(3),** 244-252.

Hume, J.D., and M. Schalk, 1967. Shoreline processes near Barrow Alaska: a comparison of the normal and the catastrophic. *Arctic*, **20(2),** 86-103.

Hume, J.D., et al., 1972. Short-term climate changes and coastal erosion, Barrow Alaska. *Arctic*, **25(4),** 272-278.

Lewellen, R., 1977. *A study of Beaufort Sea coastal erosion, northern Alaska. Environmental Assessment of the Alaskan Continental Shelf*, Annual Reports of the Principal Investigators, Vol. XV (Transport). National Oceanic and Atmospheric Administration, pp. 491-527.

MacCarthy, G.R., 1953. Recent changes in the shoreline near Point Barrow, Alaska. *Arctic*, **6(1),** 44-51.

Clearly, the shores of Alaska are relatively unstable and dynamic. This is nothing new. Suggesting that anthropogenic climate change is primarily responsible for this situation is unwarranted.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 94, line 29.

Spartina alterniflora's native range includes New England and extends as far north as Newfoundland (http://www.fs.fed.us/database/feis/plants/graminoid/spaalt/all.html). So, the concern that a "non-native" species is going to take over the salt marshes is misplaced.

The TSD needs to recognize the wide natural range of salt marsh species.

Page 94, lines 29-31 "The IPCC projects that many U.S. salt marshes in less developed areas can potentially keep pace with sea-level rise through vertical accretion."

So the problem is not anthropogenic climate change, but the sum total of all other anthropogenic changes. So, anthropogenic climate change in and of itself does not seem to "endanger" salt marshes, but instead, all of our other activities (primarily development in this case) has produced a situation where natural processes are unable to operate properly under a varying climate. CO2 emissions are not the problem.

The TSD needs to more prominently recognize that man's activities have largely effected ecosystem dynamics in many portions of the country such that natural responses to climate variability and/or climate change are disrupted.

Page 94, lines 36-37 "Reduced groundwater recharge associated with decreases in precipitation and increased evapotranspiration will exacerbate sea level rise effects on salinization rates. This effect could impose enormous costs on water treatment infrastructure…."

So, conversely, increases in precipitation, will mitigate sea level rise effects on salinization rates, correct? And ease the costs on water treatment infrastructure? Your Figure 4.5 shows that observed precipitation trends from 1901-2005 seem to trend

upward in most coastal areas. Are coastal regions of the U.S. projected, in net, to receive more or less precipitation in the future? Or is it too difficult to tell? Accordingly, isn't the effect of precipitation on salinization rates too difficult to accurately access?

The TSD is not scientifically complete here. An updated and more thorough discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 94, lines 42-44 "Saltwater intrusion into freshwater rivers has already been linked to the decline of bald cypress forests in Louisiana…"

The decline of bald cypress in Louisiana from saltwater intrusion is likely related to human activities—canal dredging and Mississippi River control—rather than those resulting in an enhancement of the world's greenhouse effect. Implying the latter rather than the former serves as an inadequate basis for the ANPR.

The TSD needs to more prominently recognize that man's activities have largely effected ecosystem dynamics in many portions of the country such that natural responses to climate variability and/or climate change are disrupted.

Page 95, lines 4-7

Native community response to the high rate of costal erosion along the Alaskan coast has been on-going, probably for centuries. See for instance, MacCarthy (1953) who describes abandoned Inuit settlements on the eroding shore near Point Barrow, AK. But back then, the Inuit were much more nomadic. But, to a large degree, these earlier times are now gone and the native Alaskan's are transitioning to a more modern lifestyle. But the transition has not come smoothly, which is often the case as a culture is faced with an uncertain, and oftentimes unwanted, upheaval. Establishing "permanent" settlements in a less than ideal setting is just one of the bumps along the road, but hardly a new mistake. For instance, consider the first English settlements at Roanoke Island, North Carolina or Jamestown, Virginia, or for that matter, look no further than establishment of the village of Barrow, Alaska back in the 1960s. Hume et al. (1972) include this photograph (Figure 40) with the caption: "Aerial view of the bluffs near the village recently settled. One building collapsed and one has been moved from the bluffs as a result of the 1968 storm. The beach formerly was 30 m. in width at this point. Photo taken in August 1969." The authors go on to add "The village will probably have to be moved sometime in the future; when depends chiefly on the weather…"

Figure 40. Photo from 1969 of the (then) recently established village of Barrow, Alaska showing the encroaching ocean (from Hume et al., 1972).

Erosion has been eating away at the Alaska coast for many, many decades and this fact has been known for equally as long. Wind and waves acting on soil held together by ice acts through a positive feedback to expose more frozen soil to the above-freezing temperatures of summer and the warm rays of sunshine, softening it for the next round of waves and wind. And so the process continues. A decline in near-shore ice cover helps to exacerbate the process. Ignoring these well-known environmental conditions has led to the unfortunate situation today where Inuit villages are facing an imminent pressure to relocate. This situation has less to do with anthropogenic climate change than it does to poor planning in the light of well-established environmental threats—threats that have existed for at least the better part of the 20th century.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 95, lines 10-12

I think it is a stretch to draw a comparison between Hawaii and the Chesapeake Bay's tiny Holland Island. There is a *world* of differences between the two islands—I don't think I need to begin to list them…you all know what they are.

Correction requested: This discussion should be removed as it is inappropriate for basing an endangerment finding.

Page 95, lines 23-25

Did the Maryland Geological Survey make an estimate of how much damage there would have been to shoreline structures had global temperatures not risen during the past 30 years? Or as to how much the damage would have been to coastal structures had Isabel struck 30 years prior? It would be quite useful in your analysis to be able to compare the impacts of climate change to the impacts of population changes. I wonder if the impact of climate change during the past 30 years is even discernible above the demographic signal. Whatever, **it cannot be demonstrated and shouldn't serve as the basis for the ANPR.**

Page 95, lines 34-39

As you mention, New Orleans is already 1.5-3 feet below sea level, and, as evidenced by Katrina, an unremarkable storm in New Orleans that caused huge damages (it *was* a remarkable storm along the Mississippi and Alabama coasts, though). Clearly, the prevailing climate is already perilous.

The TSD should recognize existing climate perils and compare them in magnitude to and new perils from climate change. Time and time again, the TSD fails to make this comparison. As such, it fails to provide an appropriate basis for an endangerment finding.

Page 95, lines 44-47

In research published subsequent to the IPCC AR4, the threat of a very rapid sea level rise from a rapid deglaciation of Greenland has been greatly diminshed. In its AR4, the IPCC didn't explicitly include loss from dynamic flow processes in Greenland and/or Antarctica in its sea level projections, because, in its words, "a basis in the published literature is lacking." In other words, there were no good models at the time of the AR4's publication of how glacier flow rates may vary in a changing climate.

However, since the publication of the IPCC report, several studies have appeared in the scientific literature suggesting that glacial flow rate changes in Greenland may not be great cause for concern. In these recent studies of flow dynamics, researchers have concluded that while Greenland's peripheral glaciers can move in fits and starts when melt water from the surface percolates downward through the glacier and lubricates the ice/bedrock interface, over the long term the average speed of the glacier is little effected (Joughin et al., 2008; van de Wal et al., 2008). In other words, warming should not lead to catastrophic ice discharge from increasing rates of glacial flow. Results such as these provide added support of the modest IPCC estimates of 21st century sea level rise and greatly weaken the argument for disastrous sea level rise.

As these results were published after the IPCC AR4, they have not been included in the TSD on endangerment, illustrating why the IPCC AR4 is not a reliable source on this and other climate change topics where the science is rapidly changing.

Page 95, lines 48-51

What if El Niño conditions slacken in the future? The IPCC AR4 projections indicate no clear tendency for El Niño changes (Figure 41). So a decrease in either frequency or intensity should be considered a credible possibility, no?

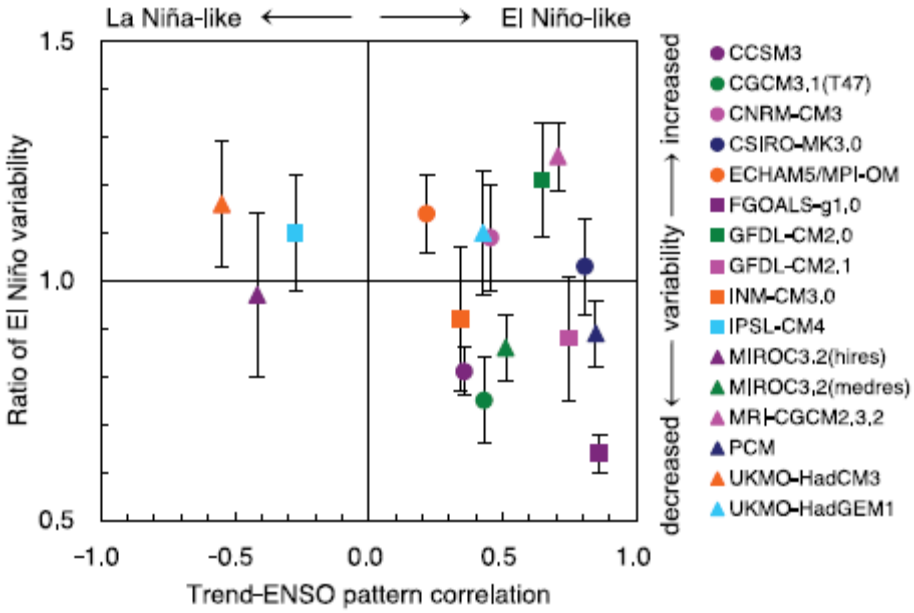


Figure 41. IPCC AR4 WGI Figure 10.16 illustrating that the projections of the future state of El Nino and its variability is largely uncertain.

Page 96, lines 3-13

This is a statement of *climate*, not climate change. As I have discussed prior on several occasions, population demographics greatly dominate over climate change when it comes to hurricane vulnerability (Pielke Jr., 2007)—a case made even more strong by recent studies, published subsequent to the IPCC AR4, which strongly suggest that global warming will not significantly increase the frequency of intensity of Atlantic tropical cyclones (e.g. Knutson et al., 2008, Vecchi et al., 2008).

You simply can *not* use the IPCC AR4 as a reference for expectations of future Atlantic tropical cyclones and their impacts.

Correction requested: The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Section 13. Energy, Infrastructure and Settlements

Page 97, lines 44-46.

This is a poor report from which to draw conclusions! Basically none of the underlying assumptions are going to be met. The population of California will not be the same at the end of the century as it is now, energy efficiency measures will surely be implemented (some requirements are on the books already and many more are being discussed), and it is quite unlikely that the high range temperature scenario will come to pass. Taken together, the great unlikelihood that any, much less all (recall that in combining percentages you multiple them, for instance if each of the underlying assumptions had a 25% chance of occurring (and that is being generous), the likelihood that all three would occur is 0.25x0.25x0.25=0.016 (1 in 64)), of the assumptions upon which the conclusion that the "annual electricity demand for air conditioning could increase by as much as 20% by the end of the century" is based will in fact occur.

Just remove this silly analysis. If you want a number representing California's likely energy demand for AC, then perform an analysis with more realistic assumptions. Oh, and be sure to compare it against the solution from a non-climate change scenario, i.e. business-as-usual—as you will undoubtedly find the demand for AC in California has been and will continue to go up on its own—especially if AC units and energy remain affordable.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Page 98, lines 39-40 "As currently designed, power plants require significant amounts of water, and they will be vulnerable to fluctuations in water supply."

Why did you use the future tense ("will be vulnerable") here instead of the present tense ("are vulnerable")? The variability in local climate in the U.S is already large. The impact from climate *change* will be small compared with the impact of an already variable natural climate.

The TSD should recognize existing climate perils and compare them in magnitude to and new perils from climate change. Time and time again, the TSD fails to make this comparison. As such, it fails to provide an appropriate basis for an endangerment finding.

Page 99, lines 36-40 "…projections of climate change suggest that extreme weather events are likely to become more intense…"

If EPA is talking about tropical cyclones, which by implication it is, this is incorrect and out-dated science that is based on the IPCC AR4 conclusions which have been supplanted in the post-IPCC AR4 literature, as I have discussed on many instances in these Comments. See Knutson et al. (2008) or Vecchi et al. (2008).

The TSD is not based upon the best available science here as the IPCC AR4 is vastly out-of-date on this topic. An updated discussion of the relevant science is required. As it stands now, it is insufficient for basing an endangerment finding.

Pages 99-100, line 47 (pg. 99) through line 2 (pg. 100)

Again, this is a climate vs. climate change issue. Flooding events by their very nature already disrupt the transportation network. And such disruptive events are a part of our climate past, present, and undoubtedly, future—with or without anthropogenic climate *change*. Downton et al. (2005) does not document any changes in the flood damages across the U.S. once demographic changes are taken into account. Small et al. (2006) and Lins and Slack (1999) do not find increases in the highest streamflow (the ones associated with flooding). So, there is evidence that our climate is not changing, despite increase in greenhouse gases, is such a way as to increase flood events.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 100, lines 28-29

No one doubts that adaptive measures are not required to handle climate and/or climate change. Adaptive measures are continually adopted as our understanding of the science and technological improvements continue to advance. These occur in the face of a static climate as well as a variable or changing one. Again, these measures are part of our past, present and future. To discount the role of adaptive measures in the future is to draw an ill-founded and illogical conclusion.

The TSD must recognize on-going society actions and adaptations to climate and climate change. In its failure to do so, the TSD is an inadequate and inappropriate basis for an endangerment finding.

Page 101, lines 4-8

The majority of the U.S. is built on non-permafrost soil, so I am confident that our building engineers can develop some way to build in Alaska in the future. If there is a will there is a way.

Page 101, line 14-19 "Climate change will affect transportation primarily through increase in several types of weather and climate extreme, such as very hot days; intense precipitation events; intense hurricanes; drought and rising sea levels, coupled with storm surges and land subsidence."

Climate change "will" affect transportation through increases in extreme events, or climate change "may" affect transportation through increases in extreme events?

In the past 30 years of "climate change" there are no clear indications that extreme climate or weather events have increased—a finding that is further contained in the CCSP 2008 "Weather and Climate Extremes in a Changing Climate." Roger Pielke Jr. summarized the CCSP's conclusions as follows:

> And the U.S. Climate Change Science Program recently issued a report ["Weather and Climate Extremes in a Changing Climate"] with the following conclusions:
>
> 1. Over the long-term U.S. hurricane landfalls have been declining.
>
> 2. Nationwide there have been no long-term increases in drought.
>
> 3. Despite increases in some measures of precipitation, there have not been corresponding increases in peak streamflows (high flows above 90th percentile).
>
> 4. There have been no observed changes in the occurrence of tornadoes or thunderstorms.
>
> 5. There have been no long-term increases in strong East Coast winter storms (ECWS), called Nor'easters.
>
> 6. There are no long-term trends in either heat waves or cold spells, though there are trends within shorter time periods in the overall record.

Pielke Jr.'s full review is located on his weblog (http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/index.html#001462).

Given the lack of a signal of increasing extremes related to climate change in the observed record, using the word "will" instead of "may" defies scientific sensibilities.

Also, recent research does not strongly support the idea of increased hurricane frequency or intensity in the future.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 101, lines 42-44

If you consult Hartmann and Wendler (2005) you will find that a big jump in temperatures in Alaska occurred 30 years ago (Figure 42) This temperature change is explained by the natural cycles of the Pacific Decadal Oscillation—not anthropogenic global warming. Temperatures in Alaska have not warmed in the past 30 years (Figure

43). So yes, higher temperatures do melt things in Alaska, but the primary cause of recent warming in Alaska is the PDO, not human greenhouse gas emissions.

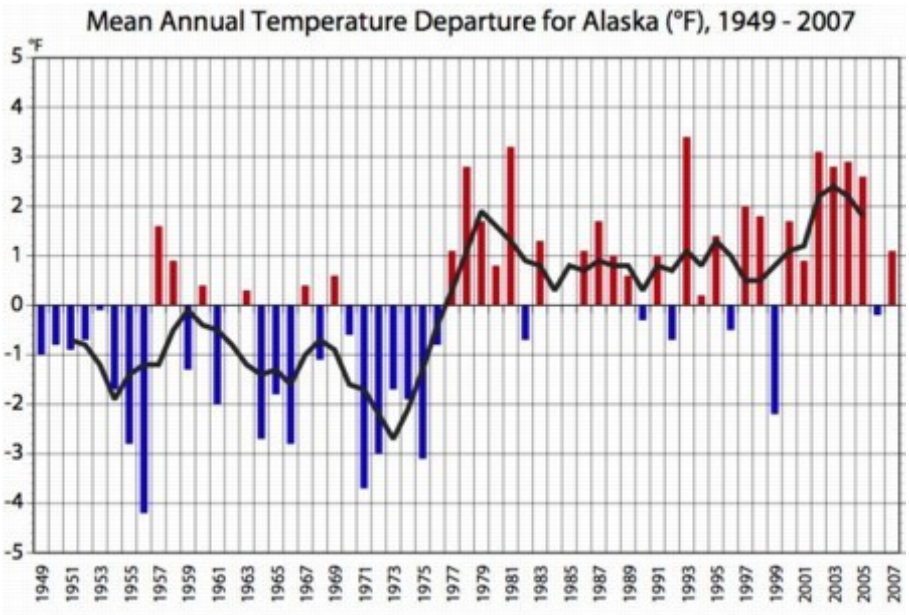


Figure 42. Statewide average temperature in Alaska, 1949-2007. Note the step up in 1977 (source: Alaska Climate Research Center, http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html)

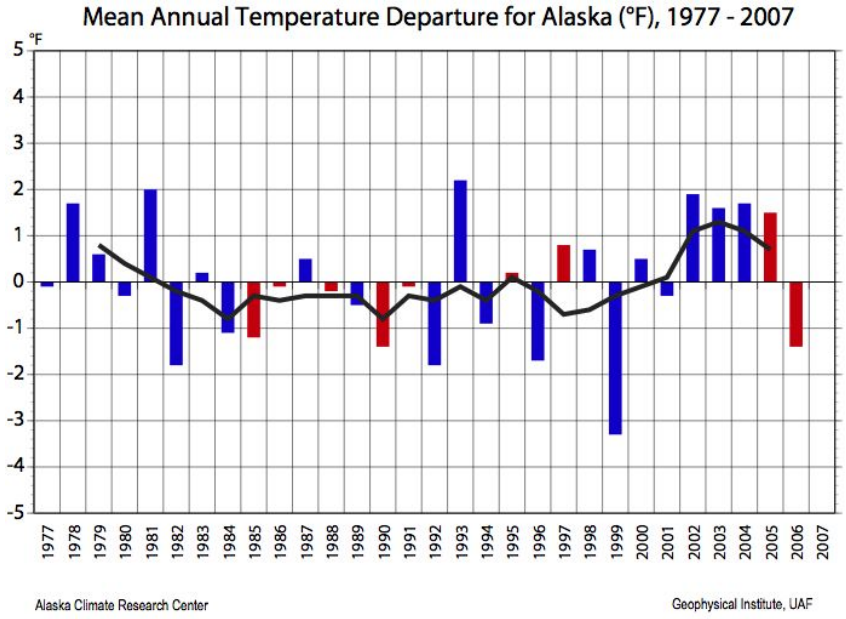


Figure 43. Statewide average temperatures in Alaska, 1977-2007. Notice that there has been no overall warming in the past 31 years there (source: Alaska Climate Research Center, http://climate.gi.alaska.edu/ClimTrends/Change/7707Change.html)

The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Section 14. Ecosystems and Wildlife

As one progresses through the TSD, it becomes increasingly repetitive. For example, I made a previous comment on tourism and recreation (*Page 91, lines 26-30*). But as one goes into the first paragraph of Section 14, there's another point, "impacts to tourism and recreation". It is noteworthy that much of this chapter will have been touched upon in earlier commentary.

Page 104, Lines 14-16:…"changes in climate will cause species to shift north….and fundamentally rearrange North American ecosystems".

A primary citation on this in Field et al. (2007; the North America chapter of IPCC WG II) is Root et al, 2003, *Nature*. The northward ecosystem migration in Root works out to 3.8 miles per *decade*, which would have the effect of moving the District of Columbia's natural biological community to Baltimore by 2102. Given that the rate of warming is largely projected to be constant by the ensemble of AIB models (see my comment concerning *Page 51, Figure 6-8*), this can be expected to continue. Another primary citation is Parmesan and Yohe, 2003, *Nature*, which calculates the observed rate of change in the onset of Spring. Her calculations indicate that spring 2103 would arrive in Washington (everything else being equal) about the time it arrives today in Raleigh, North Carolina, a distance of 232 miles. In other words, Washington will enjoy the growing season of Raleigh a century from now. It is hard to believe that this is an ecological catastrophe. Because the TSD does not quantify such changes as are in their primary citations, it is not appropriate to use as the basis for the ANPR. Quantitative rulemaking requires quantitative reasons.

The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 104, lines 30-32: "Between 1981 and 2000, global daily satellite data indicate earlier onset of spring "greenness" by 10-14 days…"

What's not to like about this? Here the TSD provides no support for endangerment.

Page 105, line 3. "Several frog species now initiate breeding calls 10-13 days earlier than a century ago".

The primary citation for this is Gibbs and Breisch (2001). Their study was confined to the area around Ithaca, New York. It compared records of the earliest calling dates during the first decade of the 20th century to the data collected in the 1990s. This is a prime example of ignoring important data. The figure below (Figure 44) gives the all the November-June average temperatures, from 1900 through 1999, and decadal averages (data from the National Climatic data center). The periods Gibbs and Breisch studied are highlighted. It is obvious that there are several decades that were quite similar to the 1990s long before anthropogenerated warming!

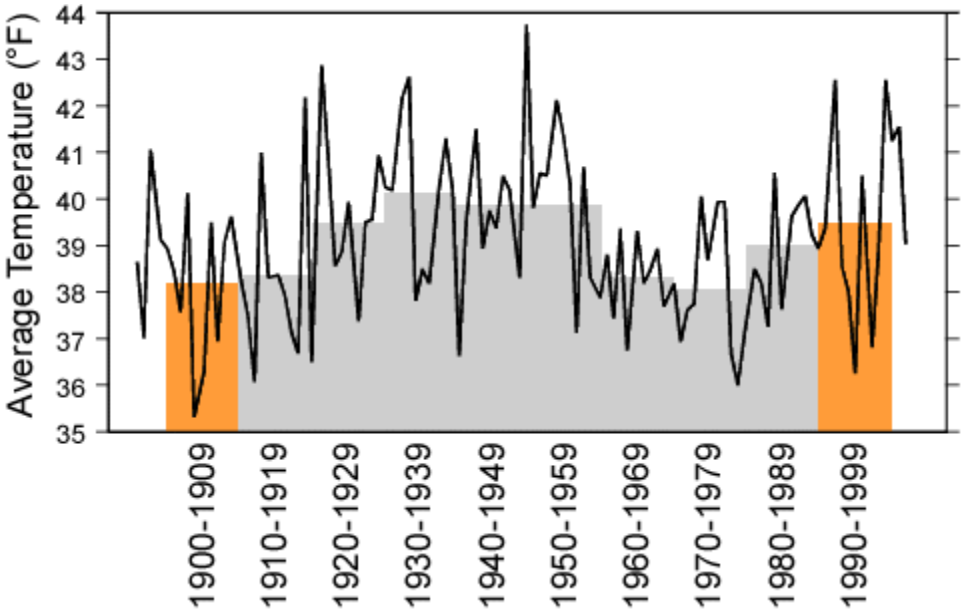


Figure 44. The history of November–June average temperatures around Ithaca, N.Y., with bars representing decadal averages. Gibbs and Breisch (2001) studied only those decades highlighted.

This represents a very uncritical reading of the scientific literature by the IPCC, which was ultimately used by the TSD, leading to an unsupportable conclusion. This is another example of why the TSD is inappropriate to use as the basis for the ANPR.

Include the figure above showing that the recent study period was climatologically unremarkable.

Page 105, lines 6-7. "Reduced water depth, related to recent warming, in Oregon lakes has increased exposure of toad eggs…leading to increased mortality…"

The primary reference here is Kiesecker et al, (2001). Their argument went like this: The lake levels are related to El Niño. The more frequent El Niño is, the lower the precipitation. Global warming is increasing El Niño frequency (a highly debatable proposition). They looked at the period 1900-1999.

Kiesecker et al, or for that matter, Fields et al., (the IPCC AR4 citation in which this appears) could have actually checked the October-March (El Niño-related) precipitation record, but they did not. Here it is, from the National Climatic Data Center (Figure 45):

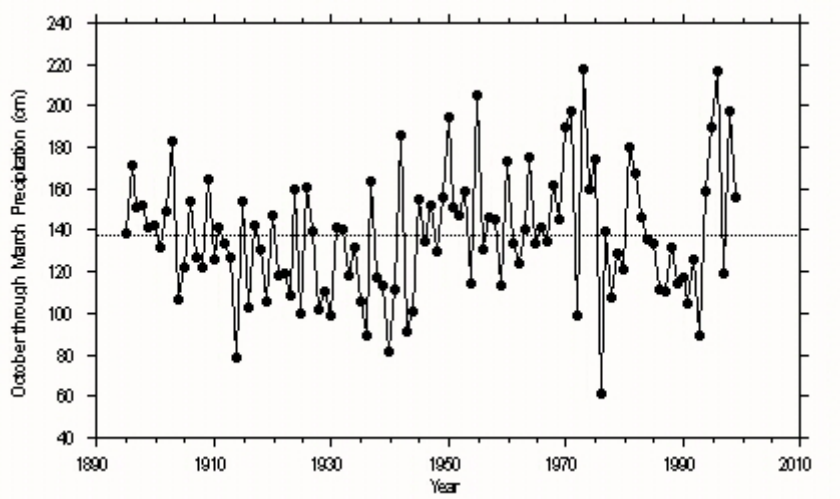


Figure 45. October-March precipitation in the region studied by Kiesecker et al. (2001) (source: NCDC).

There is obviously no downward trend in precipitation. In fact, there's nothing unusual about their study period, either. Again, this represents a very uncritical reading of the scientific literature by the IPCC, which was ultimately used by the TSD, leading to an unsupportable conclusion. This is another example of why the TSD is inappropriate to use as the basis for an endangerment finding.

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required.

Page 105, lines 8-10. "Edith's checkerspot butterfly has become locally extinct in the southern, low elevation portion of its western North American range [Northern Mexico], but has extended its range 90 km north [Southern British Columbia]"

The primary citation is Parmesan, 1996, *Nature*. The argument is that warming is causing extinction in this non-migratory butterfly in the southern part of its range, and expanding it northward.

Again, neither Parmesan nor the IPCC bothered to check the related temperature records, available from the Climate Research Unit at the University of East Anglia. The top one is for the region of maximum butterfly extinction in Northern Mexico, the bottom one is from southern British Columbia (Figure 46). They are the records that she would have found when she published her paper in 1996.

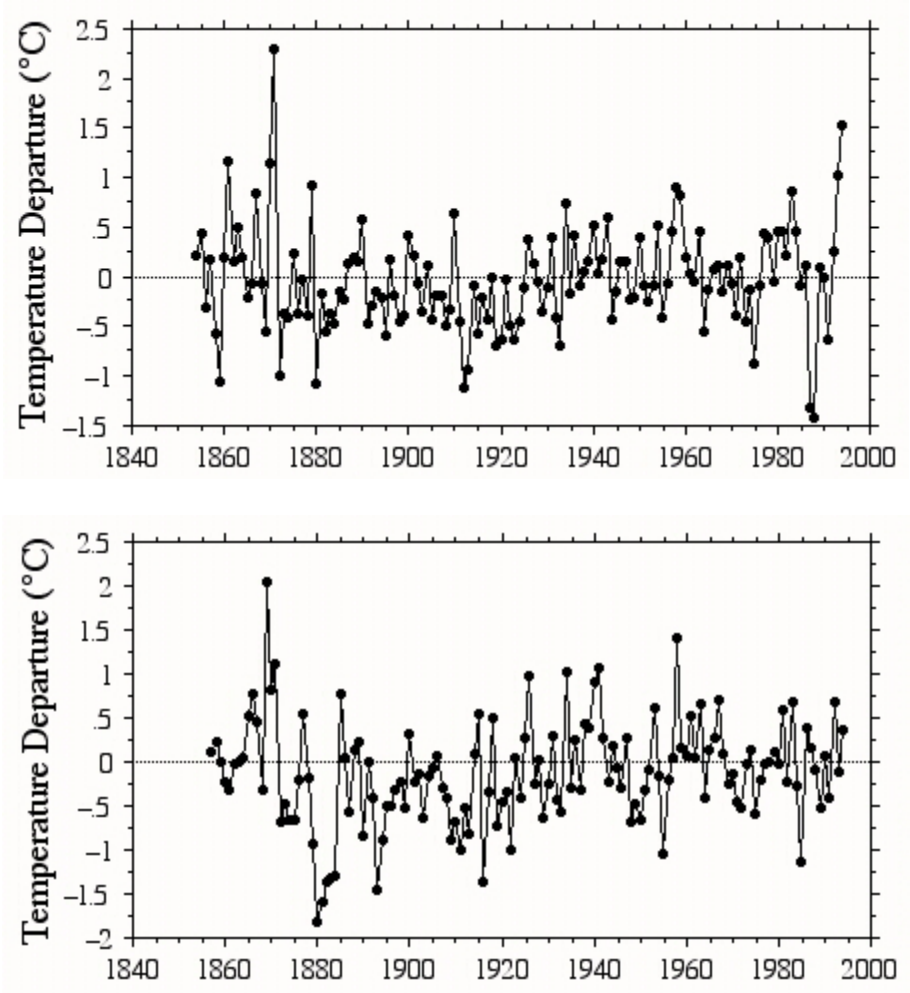


Figure 46. (top) Land-based temperature history from the region of maximum butterfly extinction, (bottom) Land-based temperature history from southern British Columbia.

Again, this represents a very uncritical reading of the scientific literature by the IPCC, which was ultimately used by the TSD, leading to an unsupportable conclusion. This is another example of why the TSD is inappropriate to use as the basis for an endangerment finding.

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically complete. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough discussion of the relevant science is required.

Page 105, lines 32-35.

These describe ongoing climate change and its impact in the Arctic. A large portion of the Comment on Page 87, lines 7-9, applies:

This is not the first time that the climate was this warm in the Holocene. Postglacial warming was Arctic-wide. Darrell Kaufman (2004) noted that for 2,000 years—from 9,000 to 11,000 years ago, Alaskan temperatures averaged 3°F higher than now. He

found that there have been three similarly warm periods in Alaska: AD 0 to 300, 850–1200, and 1800 to the present. Thompson Webb III et al. (1998), found timings similar to MacDonald et al (2000): northwestern and northeastern North America were more than 4°F warmer than the baseline from 7,000–9,000 and 3,000–5,000 years ago, respectively.

The lack of historical perspective in the TSD reduces its credibility as a basis for an endangerment finding.

Page 106, Figure 14.1 "a) trend in April 1 SWE"

This is the same comment given for Page 86, lines 39-42. *There is obviously no significant trend in this time series.* Picking on 1950 as a start data is equally absurd, as there is clearly lower snow water equivalents in the decades prior to this.

This is an prime example of the inadequacy of the IPCC review process, and undermines the credibility of the IPCC report as the basis for this section of the TSD, which, in turn, undermines the credibility of the TSD as the basis for the ANPR.

The TSD is not scientifically complete here because the IPCC AR4 is not scientifically accurate. Therefore the TSD should not rely on the IPCC AR4 on this topic. A more thorough analysis of the relevant science is required

Page 107, lines 4-6. "By the end of the 21st Century, ecosystems in the northeast and southeast U.S. are projected to become carbon sources…"

This implies very static use of forests! These are largely managed ecosystems, and efficient logging and turning of the wood products into quality (long-standing) housing can continue their being a sink. However, locking up the forest in some carbon-sequestration scheme will have the opposite effect as the forest matures, trees die, and decay.

The TSD and the IPCC have failed to recognize this and therefore this is inappropriate for the ANPR. A revision to reflect the best available science is required.

Page 107, lines 42-43: "…timing of breeding cycles and pond drying due to the lack of precipitation can lead to reproductive failure".

Precipitation is generally predicted to increase over the U.S., including Alaska, with the exception of the Southwest, where there are relatively few natural ponds. Consequently, the overall likelihood of precipitation-induced reproductive failure is lower, *in toto*, in the coming century than in the last one (if the precipitation models are to be believed). **The statement in the TSD is meaningless without noting these data.**

Page 109, Box 14.1: Polar bears.

A meta-analysis of fossil-tree data in the Holocene tundra by Macdonald et al. (2000) is highly relevant here. While it focused on northern Russia and Scandinavia, the implications for summer arctic sea-ice are (and therefore for Polar Bears) are obvious.

MacDonald et al. (2000) collated records of trees preserved the acidic environment that is now the Arctic tundra. The remains were dated by radiocarbon analysis.

The boundary between the northern forest and the bare tundra is currently south of the Arctic Ocean, and is determined by summer maximum temperatures. MacDonald found that "Over most of Russia, forest advanced to or near the current arctic coastline between 9000 and 7000 yr B.P. [before present] and retreated to its present position by between 4000 and 3000 yr B.P." In other words, the Eurasian arctic was considerably warmer than today for seven *millennia*. MacDonald wrote "During the period of maximum forest extension, the mean July temperature along the northern coastline may have been 2.5 to 7°C [4.5-12.6°F] warmer than "modern". One reason they give for this warmth is "extreme Arctic penetration of warm North Atlantic Waters".

This, coupled with the findings of Kaufman et al. (2004) and Webb III et al., (1998) (noted in earlier comments) for Alaska, northwestern and northeastern high-latitude North America, means that end-of-summer Arctic sea-ice had to be substantially degraded, or even gone, for many, many years within these millennia. Obviously the Polar Bear survived. Further, as shown by Dyck et al. (2007), populations are at very high levels at today's temperature, despite rapid arctic warming.

Polar bear arguments to support the ANPR are, to put it lightly, on very thin ice.

Page 109, lines 24-26 "…hurricanes can cause mass mortality in wildlife populations and contribute significantly to alterations in species distribution.…"

What does this have to do with global warming and the ANPR? The scientific literature on hurricanes and global warming (including some not included in the 2007 IPCC WG I report) is inconsistent at best. In 2008, Knutson's revised model predicted a 30 percent reduction in the frequency of landfalling hurricanes in North America. The mean windspeed increase in the reduced number of storms is now 2%, a number that is clearly too small to measure. The TSD is not sufficiently current with the tropical cyclone literature to be able to find support for any endangerment finding here. The statement in this chapter is a declarative sentence without any quantitative support.

Correction requested: The TSD is not scientifically complete here. An updated discussion of the relevant science is required.

Page 110, lines 20-26.

This is a paragraph repeating much of what was said about Arctic ecosystems in previous portions of the TSD. As such, the discussion above of publications by MacDonald et al.

(2000), Kaufman et al. (2004), and Webb III et al. (1998) show that it is very likely that Arctic ecosystems have been in similar positions during periods of human habitation, and it is obvious that the inhabitants survived. **The TSD offers no quantitative nor historic climatological guidance here, and therefore is inadequate as support for the ANPR.**

C. References

Bengtsson, L., et al., 2006. Storm Tracks and Climate Change. *Journal of Climate*, **19**, 3518-3543.

Bengtsson, L. et al., 2007. *Tellus*, **59A**, 539.

Cook, E.R., et al., 2004. Long-term Aridity Changes in the Western Unites States. *Science*, **206**, 1015-1018.

Davis, R.E., Knappenberger, P.C., Novicoff, W.M., and P.J. Michaels, 2002. Decadal changes in heat-related human mortality in the Eastern United States. *Climate Research*, **22**, 175-184.

Davis, R.E., et al., 2003a. Decadal changes in summer mortality in the U. S. cities. *International Journal of Biometeorology*, **47**, 166-175.

Davis, R.E., et al., 2003b. Changing heat-related mortality in the United States. *Environmental Health Perspectives*, **111**, 1712-1718.

Davis, R.E., Knappenberger, P.C., Michaels, P.J., and W.M. Novicoff, 2004. Seasonality of climate-human mortality relationships in U.S. cities and impacts of climate change. *Climate Research*, **26**, 61-76.

de Laat, A.T.J., and A.N. Maurellis, 2006. Evidence for influence of anthropogenic surface processes on lower tropospheric and surface temperature trends, *International Journal of Climatology*, **26**, 897–913.

Dlugokencky, E.J., et al., 1998, Continuing decline in the growth rate of the atmospheric methane burden. *Nature*, **393**, 447–450.

Downton, W.M., Miller, J.Z.B., Pielke Jr., R.A., 2005. Renalysis of U.S. National Weather Service Flood Loss Database. *Natural Hazards Review*, **6**, 13-22.

Douglass, D.H., et al., 2007. A comparison of tropical temperature trends with model predictions. *International Journal of Climatology*, doi 10.1002/joc.1651.

Dyck, M.G., et al., 2007. Polar bears of western Hudson Bay and climate change: Are warming spring air temperatures the “ultimate” survival control factor? *Ecological Complexity*, **4**, 73-84.

Emanuel, K., et al., 2008. Hurricanes and global warming. Bulletin of the American Meteorological Society, 347-367.

Fouillet, A., et al., 2008. Has the impact of heat waves on mortality changed in France since the European heat wave of summer 2003? A study of the 2006 heat wave. *International Journal of Epidemiology*, doi:10.1093/ije/dym253.

Gerten, D., et al., 2008. Causes of change in 20th century global river discharge. *Geophysical Research Letters*, **35**, L20405, doi:10.1029/2008GL035258.

Gibbs, J. P., and A. R. Breisch. 2001. Climate warming and calling phenology of frogs near Ithaca, New York, 1900-1999. *Conservation Biology*, **15**, 1175-1178.

Goldenberg, S. B., et al., 2001. The recent increase in Atlantic hurricane activity: Causes and implications. *Science*, **293**, 474– 479.

Gosling, S., et al., 2008. Associations between elevated atmospheric temperature and human mortality: a critical review of the literature. *Climate Change*, doi 10.1007/s10584-008-9441-x.

Hartmann, B., Wendler, G., 2005. On the significance of the 1976 Pacific climate shift in the climatology of Alaska. *Journal of Climate*, **18**, 4824-4839.

Holgate, S. J., 2007. On the decadal rates of sea level change during the 20th century. *Geophysical Research Letters*, **34**, L01602, doi:10.1029/2006GL028492.

Hu, F.S., et al., 2001. Pronounced climatic variations in Alaska during the last two millennia. *Proceedings of the National Academy of Sciences*, **98**, 10552-10556.

Hume, J.D., and M. Schalk, 1967. Shoreline processes near Barrow Alaska: a comparison of the normal and the catastrophic. *Arctic*, **20(2),** 86-103.

Hume, J.D., et al., 1972. Short-term climate changes and coastal erosion, Barrow Alaska. *Arctic*, **25(4),** 272-278.

Jones, J. M., and M. Widmann, 2004. Early peak in Antarctic Oscillation index, *Nature*, 289– 290.

Joughin, I., et al., 2008. Seasonal speedup along the western flank of the Greenland Ice Sheet. *Science*, **320**, 781-783.

Kalnay, E., and M. Cai, 2003. Impact of urbanization and land use change on climate. *Nature*, **423**, 528–531.

Karpechko, A. Yu., et al., 2008. Stratospheric influence on circulation changes in the Southern Hemisphere troposphere in coupled climate models. *Geophysical Research Letters*, **35**, L20806, doi:10.1029/2009GL035354.

Kaufman, D.S., et al., 2004. Holocene thermal maximum in the Western Arctic (0 to 180W). *Quaternary Science Reviews*, **23**, 529-560.

Keenlyside, N.S., et al., 2008. Advancing decadal-scale climate prediction in the North Atlantic sector. *Nature*, **453**, 84-88.

Kiehl, J.T., 2007. Twentieth century climate model response and climate sensitivity. *Geophysical Research Letters*, **34**, L22710, doi:10.1029/2007GL031383.

Kiesecker, J.M., et al., 2001. Complex causes of amphibian population declines. *Nature*, **410**, 681-684.

Kitzberger, T., Brown, P.M., Heyerdahl, E.K., Swetnam, T.W. and T.T. Veblen, 2007. Contingent Pacific-Atlantic Ocean Influence on multicentury wildfire synchrony over western North America. *Proceedings of the National Academy of Sciences*, **104** (2), 543-548.

Klotzbach, P.J., 2006. Trends in global tropical cyclone activity over the past twenty years (1986-2005). *Geophysical Research Letters*, **33**, L010805, doi:10.1029/2006GL025881.

Knutson, T.R., et al., 2008. Simulated reduction in Atlantic hurricane frequency under twenty-first-century warming conditions. *Nature Geosciences*, doi:10.1038/ngeo202.

Kolker, A. S., and S. Hameed. 2007. Meteorologically driven trends in sea level rise. *Geophysical Research Letters*, **34**, L23616, doi:10.1029/2007GL031814.

Landsea, C. W., 2005. Hurricanes and global warming. *Nature*, **438**, E11-13.

Landsea, C.W., et al., 2006. Can we detect trends in extreme tropical cyclones. *Science*, **313**, 452-454.

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

MacDonald, G.M., et al., 2000. Holocene treeline history and climate change across northern Eurasia. *Quaternary Research*, **53**, 302-311.

Mann, M.E., and K. A. Emanuel, 2006. Atlantic hurricane trends linked to climate change. *Eos: Transactions of the American Geophysical Union*, **87**, 233-244.

Masson, V., et al., 2000. Holocene climate variability in Antarctic based on 11 ice-core isotopic records. *Quaternary Research*, **54**, 348-358.

McCabe, G., et al., 2004. Pacific and Atlantic Ocean influences on multidecadal drought frequency in the United States. *Proceedings of the National Academy of Sciences*, **101**, 4136-4141.

MacCarthy, G.R., 1953. Recent changes in the shoreline near Point Barrow, Alaska. *Arctic*, **6(1),** 44-51.

McKitrick, R. R., and P. J. Michaels, 2007. Quantifying the influence of anthropogenic surface processes inhomogeneities on gridded global climate data. *Journal of Geophysical Research*, **112**, D24S09, doi:10.1029/2007JD008465.

Michaels, P.J., et al., 2004. Trends in precipitation on the wettest days of the year across the contiguous USA. *International Journal of Climatology*, **24**, 1873-1882.

Michaels, P.J., 2008. Global warming: Correcting the data. *Regulation*, **31**, 46-52.

Nemani, R.R., et al., 2003. Climate-driven increases in global terrestrial net primary production from 1982 to 1999. *Science*, **300**, 1560–1563.

Palecki, M.A., S.A. Changnon, and K.E. Kunkel, 2001. The nature and impacts of the July 1999 heat wave in the midwestern United States: Learning from the lessons of 1995. *Bulletin of the American Meteorological Society*, **82**, 1353–1367.

Parmesan, C., (1996). Climate and species' range. *Nature* **382**, 765–766.

Pederson, G.T., S.T. Gray, D.B. Fagre, and L.J. Graumlich. 2006. Long-Duration Drought Variability and Impacts on Ecosystem Services: A Case Study from Glacier National Park, Montana. Earth Interactions, **10**, Paper No. 4.

Pielke Sr., R.A. et al., 2007. Documentation of uncertainties and biases associated with surface temperature measurement sites for climate change assessment. *Bulletin of the American Meteorological Society*, **88**, 913-928.

Pielke, Jr., et al., 2008. Normalized Hurricane Damages in the United States: 1900-2005. *Natural Hazards Review*, **9**, 29-42.

Pielke, Jr., R. A., 2007. Future Economic Damage from Tropical Cyclones: Sensitivities to Societal and Climate Changes. *Philosophical Transactions of the Royal Society*, **365**, 1-13.

Precht, W.F., and R.B. Aronson, 2004. Climate flickers and range shifts of reef corals. *Frontiers in Ecology and the Environment*, **2**, 307-314.

Rigby, M., et al., 2008. Renewed growth of atmospheric methane. *Geophysical Research Letters*, doi:10.1029/2008GL036037, in press.

Santer B., D., et al., 2008. Consistency of modeled and observed temperature trends in the tropical troposphere. *International Journal of Climatology*, doi: 10.1002/joc.1756.

Scafetta, N., and B. J. West, 2006. Phenomenological solar contribution to the 1900-2000 global surface warming. *Geophysical Research Letters*, doi: 1029/2005GL025539.

Schiermeier Q., 2006. The methane mystery. *Nature*, **442**, 730–731.

Shepherd, J.M., 2005. A Review of Current Investigations of Urban-Induced Rainfall and Recommendations for the Future. *Earth Interactions*, **9**, 1-27.

Seager, R., A. Tzanova and J. Nakamura, 2008. Drought in the Southeastern United States: Causes, variability over the last millennium and the potential for future hydroclimate change, *Journal of Climate*, Submitted.

Small, D., et al., 2006. Trends in precipitation and streamflow in the eastern U.S.: Paradox or perception? *Geophysical Research Letters*, **33**, L03403, doi:10.1029/2005GL024995, 2006.

Small, D., and S. Islam, 2007. Decadal variability in the frequency of fall precipitation over the United States. *Geophysical Research Letters*, **34**, L02404, doi:10.1029/2006GL028610.

Spencer, R. W., et al., 2007, Cloud and radiation budget changes associated with tropical intraseasonal oscillations. *Geophysical Research Letters*, **34**, L15707, doi:10.1029/2007GL029698.

Spencer, R. W., and W. D. Braswell, 2008. Potential biases in feedback diagnosis from observations data: a simple model demonstration. *Journal of Climate*, **21**, 5624-5628.

Strong, C. and R.E. Davis, 2007. Winter Jet Stream Trends over the Northern Hemisphere. *Quarterly Journal of the Royal Meteorological Society*, **133,** 2109-2115, DOI: 10.1002/qj.171.

Swanson, K.L, 2007. Impact of scaling behavior on tropical cyclone intensities. *Geophysical Research Letters*, **34**, doi:10.1029/2007GL030851.

Thompson, D.W., et al., 2008. A large discontinuity in the mid-twentieth century in observed global-mean surface temperature. *Nature*, **453**, 646-650.

van de Wal, R. S. W., et al., 2008. Large and rapid melt-induced velocity changes in the ablation zone of the Greenland ice sheet. *Science*, **321**, 111-113.

Vecchi, G.A. and B.J. Soden. 2007a. Effect of remote sea surface temperature change on tropical cyclone potential intensity. *Nature*, **450**, 1066-1071.

Vecchi, G.A. and B. J. Soden, 2007b. Increased tropical Atlantic wind shear in model projections of global warming. *Geophysical Research Letters*, L08702, doi:10.1029/2006GL028905.

Vecchi, G. A., et al., 2008. Whither Hurricane Activity? *Science*, **322**, 687-689.

Webb, T., III, et al., 1998. Late Quaternary Climate Change in Eastern North America: A Comparison of Pollen-Derived Estimates with Climate Model Results. *Quaternary Science Reviews*, **16**, 587–606.

Westerling, A.L., et al., 2006. Warming and Earlier Spring Increases Western U.S. Forest Wildfire Activity. *Science*, **313**, 940-943.

Wigley, T.M.L., 1998. The Kyoto Protocol: CO2, CH4 and climate implications. *Geophysical Research Letters*, **25**, 2285-2288.

Willis, J. K., et al., 2008. Assessing the globally averaged sea level budget on seasonal to interannual timescales. *Journal of Geophysical Research*, **113**, C06015, doi:10.1029/2007JC004517.

Wopplemann, G., et al., 2007. Geocentric sea-level trend estimates from GPS analyses at relevant tide gauges world-wide. *Global and Planetary Change*, **57**, 396-406.

Wunsch, C., et al., 2007. Decadal trends in sea level patterns, 1993-2004. *Journal of Climate*, **20**, 5889-5911.

Zhang, X., et al., 2007. Detection of human influence on twentieth-century precipitation trends. *Nature*, **448**, 461-466.

Zhang, R., 2007. Anticorrelated multidecadal variations between surface and subsurface tropical North Atlantic. *Geophysical Research Letters*, **34**, L12713, doi:10.1029/2007GL030225.

Zhang, R., 2008. Coherent surface-subsurface fingerprint of the Atlantic meridional overturning circulation. *Geophysical Research Letters*, **35**, L20705, doi:10.1029/2008GL035463.

Zucker, J.R., 1996. Changing patterns of autochthonous malaria transmission in the United States: A review of recent outbreaks. *Emerging Infectious Diseases*, **2**, 37-43.

Part II. Comments on Public Review Draft of the *Unified Synthesis Product Global Climate Change in the United States*

In July, 2008, the U.S. Climate Change Science Program (CCSP) released a document, *Global Climate Change Impacts in the United States, Unified Synthesis Product*, for public comments through August 14, 2008. This document summarized the CCSP documents that served "as an important source" for the ANPR.

As evidence why the EPA should *not* have relied on Climate Change Science Program synthesis reports, this document includes comments of the CCSP's *Unified Synthesis Product* that were filed with the CCSP during their public comment period**.**

The *Unified Synthesis Product* was a remarkable document. Its purpose was to "synthesize" various CCSP sub-reports, on topics such as "abrupt" climate change, or ozone-depletion and climate (among many others) that *were not even published*! Those documents, in turn were supposed to have also been subject to public commentary, *even though they did not exist*.

The Senior Editor and Synthesis Team Coordinator was Susan J. Hassol, incredibly, the writer of HBO's 2006 "documentary" *Not Too Hot to Handle*, a program with a clear policy intent, an extremely alarmist and one-sided view of global warming science, and clearly exclusionary of a large body of refereed scientific literature (some of which is detailed below) that supports a much less alarmist view of climate change.

Following are some very lengthy comments submitted on the CCSP. It is worth noting that in no small part, because of these comments, the *Unified Synthesis Product* was withdrawn and is now subject to a complete rewrite.

II. Comments

General Comment

I would like to preface my remarks with a general statement.

Of all of the "consensus" government or intergovernmental documents of this genre that I have reviewed in my 30+ years in this profession, there is no doubt that this is absolutely the worst of all. Virtually every sentence can be contested or does not represent a complete survey of a relevant literature. I believe this is an agenda-driven polemic. The authors, particularly the senior ones, have a track record of being politically very sensitive and responsive. They know that the Congress that receives this document will be dominated, at least in the Senate, by a strengthened majority in search of some "official", highly inflammatory document which can be used for Findings for legislation mandating stringent near and far-term cuts in carbon dioxide emissions. They know that

this document will be used as the basis for an EPA rulemaking on carbon dioxide emissions. They know this, despite their full knowledge that there is no suite of technologies that can accomplish major reductions, and the full knowledge that the only remaining way to achieve such cuts is to make carbon-based energy outrageously expensive. I am reminded of President Eisenhower's Farewell Address, January 17, 1961:

> The prospect of domination of the nation's scholars by federal employment, project allocations, and the power of money is ever present—and is gravely to be regarded. Yet, holding scientific research and discovery in respect, as we should, we must always be alert to the equal and opposite danger that public policy could itself become a captive of a scientific-technological elite.

It has always been my worry that climate change would become the vehicle that would bring Eisenhower's fear to fruition. The CCSP draft is Eisenhower's fear writ large.

The extremely short comment period, and the lack of transparency that was evident at the beginning of that period (when a bogus "password" was required, thwarting immediate review) is further evidence for the agenda-driven nature of this Product. Why not 120 days? What is the rush? Obviously, so people like me—with day jobs—will not have the time to adequately review this document.

Let me say that, knowing many of the individuals involved, I am deeply saddened by what this document says about my profession. It is in that state that I must tender the rest of my review.

There is an overwhelming amount of misleading material in the CCSP's "Global Climate Change Impacts in the United States." It is immediately obvious that the intent of the report is not to provide a accurate scientific assessment of the current and future impacts of climate *change* in the United States, but to confuse the reader by a loose handling of normal climate events (made seemingly more frequent, intense and damaging simply by our growing population, population movements, and wealth) presented as climate *change* events. Additionally, there is absolutely no effort made by the CCSP authors to include any dissenting opinion to their declarative statements, despite the peer-reviewed scientific literature being full of legitimate and applicable reports and observations that provide contrasting findings. Yet, quite brazenly, the CCSP authors claim to provide its readers—"U.S. policymakers and citizens"—with the "best available science." This proclamation is simple false.

While these are selective comments across several sections of the Report, there was simply not enough time to completely detail the inaccuracies inherent in the entirety of the document. Please don't take any lack of comment on some large sections as an indication that there is nothing wrong. Much of what is detailed in the sections that I did have the opportunity to comment on continues throughout the rest of the report. It is clearly obvious that grave and unacceptable errors are appallingly commonplace throughout the pages of the CCSP report.

It is not much of an exaggeration to claim that virtually every sentence on every page is contentious—and yet the recognition of any contention or contrasting observations is virtually absent. This occurs too often for it to be accidental, and thus I consider it a blatant and purposeful disregard of legitimate and applicable science. The contention does not simply arise from skeptics being skeptics, but instead, it is plain for all to see in the peer-reviewed scientific literature and in other readily available government datasets. The CCSP authors make no effort to acknowledge any dissenting observations or findings, much less an admission as to the wide depth of the contrasting viewpoints found in the literature.

The CCSP report is such a poor handling of the subject of the impacts of climate change on the United States that it cannot be allowed to stand in any semblance of its current form. It should be withdrawn. Any future attempts at producing such an assessment must be done so with authors who will accurately represent the state of scientific knowledge and its limitations. These authors have failed in that effort.

I presume the purpose of this report is to emphasize all of the theoretically possible negative impacts that might occur in a "worst-case scenario" rather than a report that reflects the current state of the climate change literature. I could easily prepare a similar report about all of the great benefits that will arise in the United States from climate change, but none of these ideas have been included in this draft. The uninformed reader (i.e., the public, reporters, and policy-makers) upon reading this report will be lead to believe that a terrible disaster is soon to befall the United States from human-induced climate change and that all of the impacts will be negative and devastating. Of course, if the purpose here is not really to produce an unbiased review of the impact of climate change on the United States, but a political document that will give cover for EPA's decision to regulate carbon dioxide, then there is really no reason to go through the ruse of gathering comments from scientists knowledgeable about the issues, as the only science that is relevant is selected work that fits the authors' pre-existing paradigm.

The most honorable action would be to pull the report and start over, perhaps by finding some scientists who are aware of the ongoing debates in the refereed literature. Given that this is unlikely, then the following sentence should be added to the report's introduction:

"This report is not intended to be an unbiased review of climate change science; rather, it is a political document that emphasizes the worst possible theorized impacts of climate change in the hopes that the U.S. Government will use this report in support of a decision to regulate carbon dioxide emissions."

With that precis, I doubt that you will utilize many of my comments, which argue for balance. A review of the body of work in my career includes papers that both support and counter your paradigm on climate change. In other words, I did science.

Selected Comments on the "Key Findings"

Specific comment 1. *third (un-numbered) title page*

Comment: my comments begin on the third (un-numbered) title page, with the following naïve question: What the heck is this picture supposed to connote about climate change? It is an image showing that people of both sexes and as many races as can be represented are capable of holding hands. If you wanted to set a tone—that this is a political polemic—you couldn't have picked a better front image. The fact that no one involved apparently even questioned the use of this image (enough to have it removed) says very, very much about the author team. Sad to say.

Recommendation: Delete the picture and put in something that is at least vaguely related to climate.

***Specific comment* 2.** *Executive Summary, page 4, second paragraph, "…most impacts are projected to be detrimental, in part because society and ecosystems have developed and evolved based upon historical climate".*

Comment: Let's start with "society". I presume that this means "people". They live and prosper roughly from a range of temperature from -40° to +40°C. Apparently the authors believe in "climatic determinism", the long-discarded notion that climate is responsible for the success or the failure of various cultures with regard to one another. Changing the mean temperature, say, 3°C, within that range lies well within the adaptive range of almost every society on earth. Even the Inuit occupy a range of about 12°. Farmers in Virginia achieve the same yield of corn in the relatively cool, dry Shenandoah Valley that they reap in the hot and muggy Tidewater.

Worldwide, cities have warmed extensively. Tokyo's heat island, a rise of approximately 3°C in the last 100 years, has no demonstrable net negative effect on its urban society. Instead, millions of people with many different subcultures simultaneously adapted to a temperature change that some would estimate would occur as a global average in this century. Let's just say that the CCSP cannot demonstrate a net negative effect on society and leave it at that. No, let's not: CCSP is guilty here of what Paul Waggoner called the "stupid people hypothesis", as gratuitous a swipe as can be made—especially in light of the strange picture of people of all cultures working so together on the third un-numbered title page referred to above.

The Tokyo heat island indeed has had some ecological effects. Vegetation that would normally be restricted to locations further to the south can now survive there. Indeed, there is another problem that CCSP clearly ignores with its statement: *microclimates*. The variegated surface of earth provides for considerable climatic variation within a larger geography. Models of extinction of species that "evolved based upon historical climate" completely neglect the fact that microclimatic refugia are likely to preserve

many species, despite their larger "historical climate". I offer the CCSP the stand of *Abies balsamea* growing in northeast Iowa, far away from the "historical climate" of the boreal forest. According to every climate/extinction model, it should not be there.

Recommendation: Delete the statement about historical climate. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 3. *Page 4, paragraph 3, "The increase in wind erosion associated with drought and the increase in heavy downpours are also expected to lead to irreversible loss of soil".*

Comment: There are common adaptations to drought-induced deflation, such as the shelter belts that were planted in the Great Plains in response to the dust bowl. There are similar adaptations to high-runoff via soil stabilization. Those promoting switchgrass-based ethanol will clearly mitigate much of this. This is yet another example of the overly pessimistic nature of the Report. Why don't you mention that there are likely some very simple adaptations to drought that societies have undertaken for centuries?

Recommendation: Revise the statement to reflect this. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 4. *Page 5, paragraph 1 "Tipping points".*

Comment: The words "tipping" or "tipping point" appear nowhere in the subsequent text.

Recommendation: This paragraph in the executive summary must be removed.

Specific comment 5*. Page 5, paragraph 3 "While humans have adapted to gradual changes in the past...we are now entering uncharted territory".*

Comment: It is very clear that Homo sapiens was around during the two rapid climate changes 12,800 and 8,200 years ago. Despite our small numbers at that time, we clearly adapted and prospered as a species. We are NOT in uncharted territory in human history, and we have a much greater technological arsenal at our disposal than we did in prehistoric times.

Recommendation: Statement is not justified and needs to be removed. ***This requires Key Finding 9.1 to be modified to state that planning requires a wider envelope, but that we are still well within the ranges of climate experienced by our species.*** **As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.**

Specific comment 6*. Key Findings, Page 6, Finding 1, bullet 1:*

Comment: Why is this the only key finding with a reference? And exactly what is the citation to? There is no Endnote section for "Key Findings".

Recommendation: Add references to all "Key Findings"

Specific comment 7*. Key Findings, Page 6, Finding 1, bullet 1:*

Comment: Over which span of years does this bullet refer to? The globe has warmed dramatically in the past 15,000 years, and most of it was not caused by human beings. The globe warmed from 1900 to 1940 and most of that was likely not caused by human beings. The global temperature did not warm the mid-1940s to the mid-1970s. Was that caused by human beings? The global temperature has not warmed much during the 21st century. Is that caused by human beings?

Recommendation: Clarify the timeframe as the statement is not generally applicable to all time periods.

Specific comment 8*. Key Findings, Page 6, Finding 1, bullet 2:*

Comment: Over which span of years does this bullet refer to? Are these supposed to be human-caused climate impacts from greenhouse gas emissions? The statement reads like these changes apply to the U.S. as a whole, when in fact, many of them are very regional in nature. Has the frequency of droughts declined anywhere? If so, why didn't that make the list? Have growing seasons lengthened anywhere? If so, why didn't that make the list? Has annual precipitation increased anywhere, maybe in regions with a growing water demand? Is so, why didn't that make the list?

Recommendation: Clarify the timeframe over which these changes have been observed, clarify that all of them may not be related to anthropogenic alterations to the earth's greenhouse effect, clarify that many of the observed changes are very regional in scope, and include other changes that be seem beneficial. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 9*. Key Findings, Page 6, Finding 1, bullet 3,"Impacts of these changes are apparent in many facets of society including health, water, food, energy, and quality of life."*

Comment: Are the "impacts" negative or positive? And how have they been assessed? For instance, the CCSP SAP 4.6 page 2-16 finds that "Heat-related mortality has declined over the past decades (Davis et al., 2002; Davis et al., 2003a; Davis et al., 2003b)." What has been the impact on "food?" The data from the National Agricultural Statistic Service shows that for the major food crops (e.g. corn, soybeans, wheat), U.S. production and yield have set records highs in recent years. What are the impacts on "quality of life?" Data from the U.S. Department of Commerce shows that the U.S. per capita income has been rising steadily (perhaps even accelerating).

Recommendation: Remove this bullet entirely, or make it clear that the human health, food supply, quality of life etc. have been steadily *improving* in the U.S. and that climate change has had little direct (statistically detectable) impact. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 10*. Key Findings, Page 6, Finding 2, bullet 1, "Global emissions of heat-trapping gases are now increasing even more rapidly than the high emissions scenario scientists have been analyzing."*

Comment: Emissions of "heat-trapping gases" are not a "climatic change"—which is the subject of Key Point 2. The salient measure for "climate change" is the atmospheric *concentration* of "heat-trapping" gases. And current rates of the atmospheric concentration growth of "heat-trapping gases" are most definitely not "occurring faster than projected even a few years ago." See the data on the atmospheric concentration growth rate of radiatively active gases at CDAIC in comparison to the timeline of atmospheric concentrations for the same gases given in the IPCC SRES scenarios.

Recommendation: Remove this bullet entirely, or make it clear that the atmospheric concentration of "heat trapping gases" is not increasing more rapidly than projected a few years ago. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 11. *Key Findings, Page 6, Finding 4, bullet 1, "all of which are disrupting our lives".*

Comment: Definition of disrupt: to cause disorder or turmoil. Does the average person believe his life is being disordered by global warming? Is there sufficient disorder or turmoil to have an effect on our national economy? Show me the growth contractions. Even in the year after Katrina, the economy grew. From fall of 05 to fall 06 the Dow went up 1000 points.

Recommendation: This phrase is simply unsupported and must be removed. *Alter Key Finding 4.1 to reflect this.*

Specific comment 12. *Key Findings, Page 8, Summary of Impacts on Sectors*

Comment: Once the recommendations are made as per my detailed comments on the Chapters "Society" and "Human Health" the majority of these bulleted items will have to be withdrawn or reworked. Without such corrections, the summary statements fail to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 13. *Chapter "About this Report", Page 14, 3rd and 4th paragraphs*

Comment: It is important to make clear to the readers of the report that while the authors of this CCSP report supplemented the other Assessment Reports with papers from the peer-reviewed scientific literature, the CCSP authors went to great lengths to ignore those peer-reviewed literature articles which either presented different conclusions or which directly criticized the papers that the CCSP authors did choose to rely on. The failure to include relevant but contradictory evidence stands in stark opposition to the pledge in the first paragraph of p. 14 which states that "U.S. policymakers and citizens …need the best available science" and that "[t]his report responds to that need…"

Recommendation: Add the following paragraph after paragraph 4, page 14:

"In order to present a unified synthesis product, we chose not to include articles from the peer-reviewed scientific literature or other widely available government data that questioned or run counter to any of the findings that we present in this report." Alternatively, the bulk of the report should be modified such that it actually does present the "best available science."

Specific comment 14. *Chapter "About this Report", Page 14, 6th paragraph, first sentence*

Insert the word "negative" before "impacts of climate change" so that the first sentence reads "While the primary focus of the report is on the negative impacts of climate change on the U.S…." This insures that the reader has a more accurate idea of the intent of the report.

Selected Comments on the Chapter "Global Climate Change"

Specific comment 15. *Chapter "Global Climate Change," page 16, first bullet.*

Comment: New results reported in *Nature* magazine (Thompson et al., 2008) indicate that data issues involving SST data in the mid 20th century may ultimately change the

character of the global temperature record for a few decades starting in the mid-1940s (an apparent cooling period in the global temperature record). Thompson et al. think that the data corrections will result in a lessening of the magnitude of the cooling in the mid-20th century, which will likely have an effect of reducing the total amount of warming during the past 50 years.

Recommendation: Change the first bullet text such that it refers to the past 30 years, rather than the past 50 years.

Specific comment 16. *Chapter "Global Climate Change," page 16, 2nd bullet.*

Comment: As I have just described, new research (Thompson et al., 2008) indicates that the character of the global temperature history will have to be modified as a result of some just-detected problems with the mid-20th century SST data. Thus, the relative contribution of natural vs. anthropogenic factors in determining the global temperature history in the mid-20th century cannot, as of yet, be accurately assessed.

Recommendation: Change the 2nd bullet text such that it refers to the past 30 years, rather than the past 50 years. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 17. *Chapter "Global Climate Change," page 16, 5th bullet.*

Comment: As can be seen on the graph on page 25 (in the "Arctic sea ice decline is accelerating" box), in fact, the character of the timeseries of annual Arctic sea ice minima is not one indicative of "acceleration" but rather of a linear (constant rate) decline. A single low point (outlier) at the end of the record is not a statistically robust indicator of a change in slope or acceleration. Thus this bullet is statistically in error.

Recommendation: Remove bullet entirely, or change to read "Arctic sea ice has been declining steadily for the past two decades." As it now reads it is statistically and scientifically inaccurate.

Specific comment 18. *Chapter "Global Climate Change," page 16, 6th bullet, "Many of the observed changes…"*

Comment: In fact, since 1998, the apparent leveling-off of the global temperature record is strongly dominated by the combined influences of ENSO, volcanic and solar variability—the so-called "natural" signals. These natural signals have, in fact, over the past 10 years, dominated the anthropogenic signal in the global temperature record for that decade, and therefore, also to a significant degree, many "specific patterns of recent climate change."

Recommendation: Remove bullet entirely, or change to read "In recent years, warming has been halted by natural, rather than anthropogenic, signals." Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 19. *Chapter "Global Climate Change," page 16, last bullet, "The human effect on climate…"*

Comment: This wording of this bullet is very vague. The words "minimized" and "sharply reduced" are not scientifically (or otherwise) defined. In fact, the human effect can be reduced (although perhaps undetectably) for any cuts in emission that lead to a slowdown of the atmospheric build-up. Why are "sharp reductions" necessary to "minimize" the effect? Won't even "sharper reductions" reduce the effect even more? So what is the definition of "minimized"?

Recommendation: Remove bullet entirely, or change to read "The human effect on climate can only be slowed in a detectable manner if greenhouse gas emissions are reduced through global cooperation. No locality, state, or even country, acting alone, can produce any scientifically meaningful reduction in the rate of future climate change that results from anthropogenic greenhouse gas emissions."

Specific comment 20. *Chapter "Global Climate Change," page 17, first paragraph, "Impacts, while often local arise from changes in this global system."*

Comment: It is imperative to follow this sentence up with one that lets the reader know that there is a large degree of uncertainty in relating (or projecting) local changes from changes in the global system. The Box TS10 in the IPCC AR4 WGI report discusses the inherent uncertainties and difficulties in making regional (much less local) climate projections. This level of uncertainty should be reflected in the discussion of local changes.

Recommendation: Make it clear that local-scale climate variation, while it is dependent on the "global system," is, in many cases, very difficult to relate to changes in the global system. Thus, it is far from clear how global changes may impact local conditions. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 21. *Chapter "Global Climate Change," page 17, second paragraph, "The amount of warming that we actually experience will be determined largely by the choices made now and in the near future."*

Comment: It is imperative to let the reader know that these "choices" are not ones made at a local, regional, or even, for the most part, at a national level. Any "choice" that will result in any meaningful change in the earth's climate will have to be done at a global level. And, at that level, there does not currently exist any appropriate "choice" (besides nuclear) for energy production.

Recommendation: Remove this whole paragraph as it gives the false sense that there exist "choices" when in fact, the future course of society's energy production is largely inevitable. The types of "choices" to be made are between increases in the standard of living in large industrializing countries/regions of the globe and status quo. These "choices" are much greater than what type of light bulb to should buy, or whether to buy emissions offsets for the travel to the next scientific meeting (they types of things which are often proffered as "choices"). As it now stands, this distinction is not made clear in the text and in fairness to the reader, it should be.

Specific comment 22. *Chapter "Global Climate Change," page 17, last sentence beneath illustration, "The long record of temperature and carbon dioxide tells us something else as well: there is no natural cycle or process revealed in this long climate history that could have caused the global warming of the past 50 years."*

Comment: This is entirely wrong. The 800,000 year-long record of CO2 concentration from the Antarctic ice core tells us absolutely nothing abut the climate change of the past 50 years. True, the record suggests that no natural process of the past 800,000 years has raised the atmospheric CO2 concentration to the level that we have currently, but atmospheric CO2 concentration is but one of many different forcing factors that impact the earth's climate. The warming from the late-1800s to the mid-1940s was nearly as large as the warming during the past 50 years and yet natural forces may have caused a large portion of it (IPCC AR4 FAQ 3.1 Figure 1, caption). Thus, natural factors can cause climate changes of the magnitude that we have seen during the past 50 years and the use of the long-term CO2 ice core record is inappropriate to make such a determination.

Recommendation: Remove the sentence. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 23. *Chapter "Global Climate Change," page 18, second paragraph, "Carbon dioxide, the principal driving factor in the warming of the past 50 years…."*

Comment: Unqualified (which means "virtually certain" according to the definitions on page 15), this statement goes well beyond the IPCC AR4 document. There is no reference for the CCSP statement. For a statement to go beyond the general assessments on which it is drawn from, a reference should be given. This IPCC AR4 (SPM page 10) states: "Most of the observed increase in global average temperatures since the mid-20th

century is *very likely* due to observed increases in greenhouse gas contributions." CO2 is but one of many influences on the earth's climate.

Recommendation: Add qualifications to the statement or add an appropriate reference.

Specific comment 24. *Chapter "Global Climate Change, Page 18, Paragraph 4*

Comment: Methane concentration increases began to slow in the 1980s and have actually declined in some recent years. Every projection made by the IPCC assumed they would increase at the pre-1980 rate and that this increase will continue for at least 50 years. Text needs to be altered to note that the increase slowed and then stopped. References, Duglokenky et al. 1998, *Nature*; Schiermeier, 2006, *Nature*.

Recommendation: *This invalidates Key Finding 2.1*. Change it to "some" heat trapping gases, and note that methane, and important heat trapping gas has stabilized or even declined in recent years, which is opposite to any "highest emission scenario". As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 25. *Chapter "Global Climate Change, Temperature chart, page 19.*

Comment: Nice to see the "Hockey Stick" is still unchallenged here.

Recommendation: At least use a multiple compilation for comparison! IPCC AR4 Figure TS.20 (p55) would be a good candidate. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 26. *Chapter "Global Climate Change," page 20, 1st paragraph, second sentence "Over the past several decades, the time during which the human influence has become clear and global temperatures have risen sharply…."*

Comment: Hmm. Actually, over the past decade, global temperatures have risen little. The reason? The combination of ENSO, volcanoes, and solar variability has acted to largely offset the warming pressure from increasing greenhouse gas concentrations.

Recommendation: Better clarify that natural factors still play a large role in the actual global temperature variations. Anyone can see that temperatures have not "risen sharply" over the past decade and so as to prevent the reader from getting

the wrong idea (the perhaps global warming has stopped), it is best to explain the reasons for the slowdown in the temperature rise.

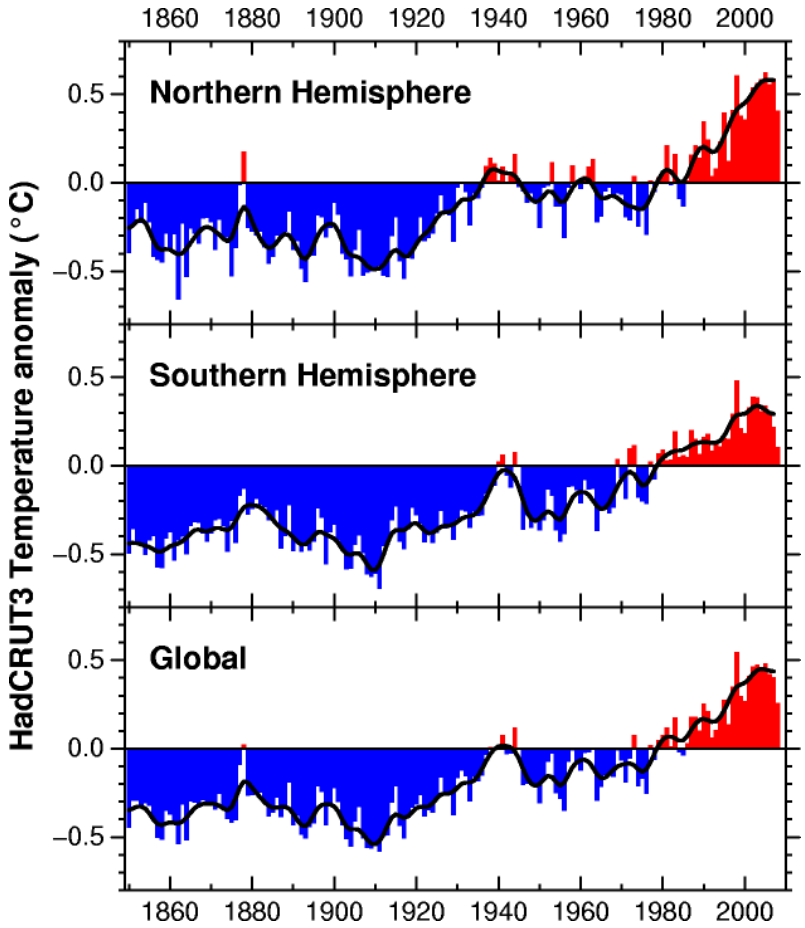
Specific Comment 27. *Temperature chart, page 21.*

Comment: No reference is given under "Figure sources" (page 181). I have never seen this chart published as an "observed global temperature". It certainly is at variance with the IPCC history. Compare to the chart on page 22. In fact, the chart labeled "Change in average global temperature" is not an observed temperature product at all, but instead it is the average of 5 model runs from a climate model (see Hansen et al, 2005—your citation 38 for a complete explanation).

Recommendation: Remove this chart and replace it with a real version of the global temperature history. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14)..

Specific Comment 28. *Chapter "Global Climate Change," page 22, 1st paragraph, first sentence "...with the warming trend accelerating in recent decades."*

Comment: Wrong. In fact, over the past 2-3 decades, the warming trend has been *decelerating*.

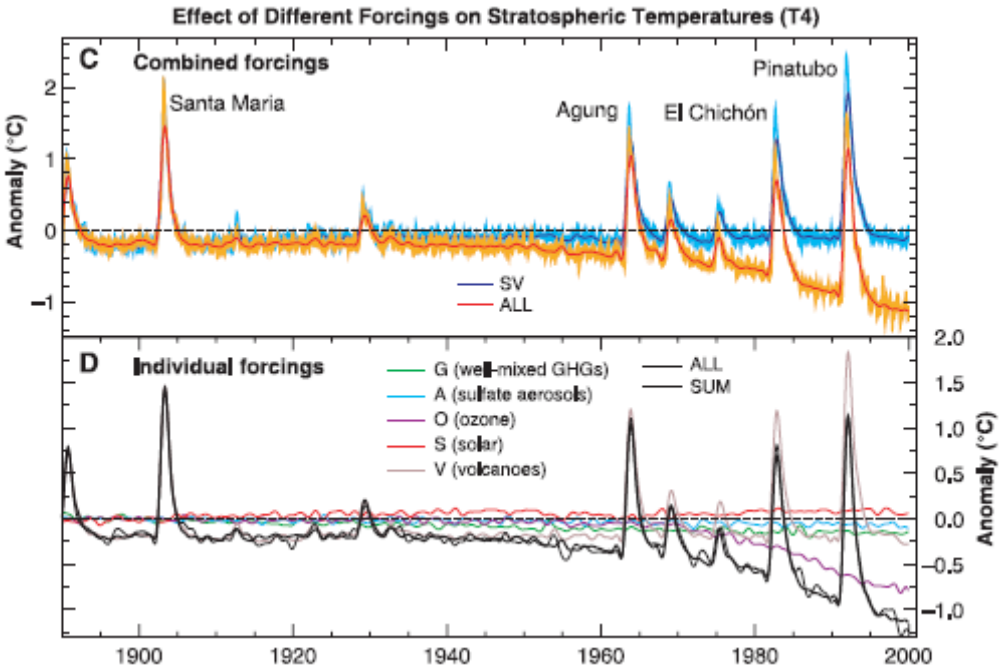


Hemispheric and Global temperature history (source: CRU, http://www.cru.uea.ac.uk/cru/data/temperature/)

Recommendation: Remove statement about warming trend accelerating. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific Comment 29. *Chapter "Global Climate Change," page 22, second paragraph, last sentence, "This pattern of tropospheric warming and stratospheric cooling is consistent…."*

Comment: Actually, as shown by Santer et al. (2003, *Science*, **301**, 429), the stratosphere is cooling largely become of ozone destruction, not an enhanced greenhouse effect.



Influences on stratospheric temperatures (source: Santer et al., 2003, *Science*, **301**, 429)

Recommendation: Remove statement about stratospheric cooling occurring because of increasing greenhouse gases, or better qualify it such that it refers to upper stratospheric temperature trends—not those measured by satellites. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific Comment 30. *Chapter "Global Temperature Change," page 22, paragraph "Precipitation patterns are changing"*

Comment: The descriptions of the precipitation changes don't match the figure very well. The text reads "Pronounced increases in precipitation over the past 100 years have been observed in eastern North America [the map shows no change in eastern North America], southern South America [the map shows no change in southern South America], and northern Europe. Decreases were observed in the Mediterranean [the map shows no change in the Mediterranean], most of Africa [the map shows Africa evenly divided between increases and decreases], and southern Asia."

Recommendation: Change either the map to better reflect the text or vice versa…and make sure that the both the text and the map reflect the most recent literature on precipitation trends.

Specific Comment 31. *Chapter "Global Temperature Change," page 22, paragraph "Precipitation patterns are changing", last sentence.*

Comment: Are there any regions that floods and/or droughts are decreasing in intensity and/or frequency? Or are such changes not of interest to the CCSP? Are they not impacts of a changing climate?

Recommendation: Change text to reflect the fact that some precipitation changes have a positive impact in that they bring more water in a world to locations with growing demands on the water system. And also, state that the distribution of precipitation events is such that more total precipitation comes with more heavy precipitation events. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

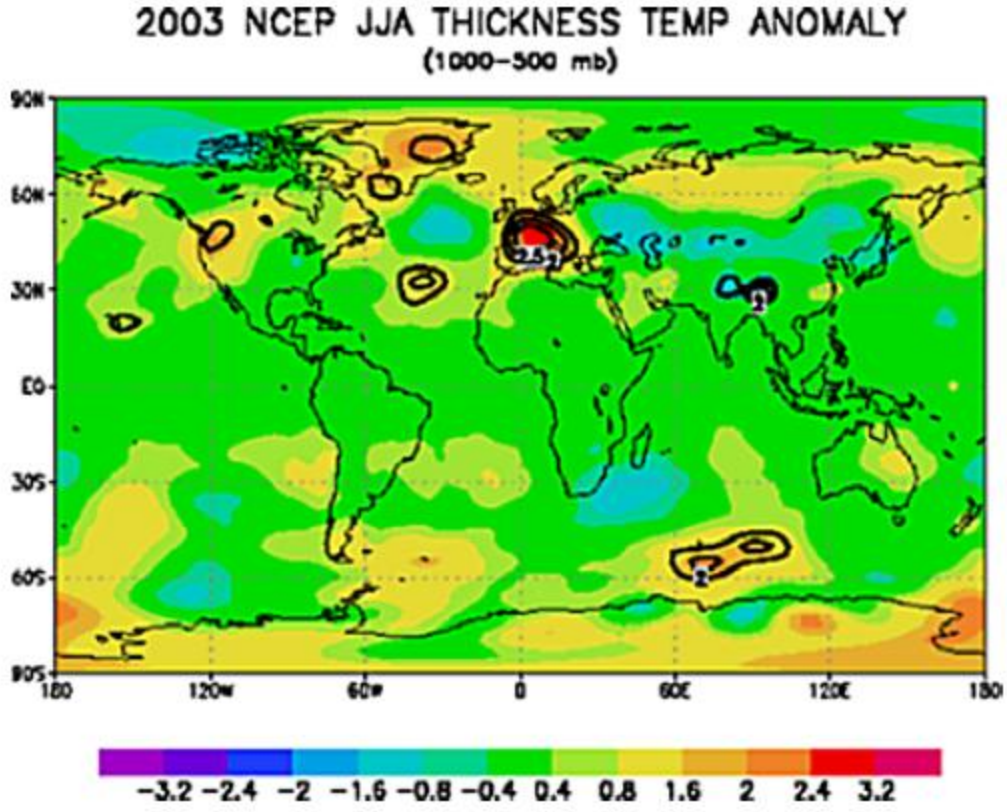
Specific Comment 32. *Chapter "Global Temperature Change," page 23, Figure caption*

Comment: The figure caption discusses European summer heat waves, but shows data for the average *summer* temperature from only Switzerland, then switches back to talking about enormous loss of life during the 2003 heat wave. The average summer temperature is an inappropriate substitute for 'heat wave' frequency or intensity. Is there an established relationship between the two? Further, the loss of life during the 2003 heat wave in Switzerland was much less than in other European locations (Grize et al., 2005).

Recommendation: Change the figure to illustrate actual 'heat waves' and not the average summer temperature distribution. Also, if you want to illustrate an 'enormous loss of life' during the 2003, pick some place other than Switzerland. Basically, the existing figure and caption neither work well together, nor illustrate the point with any sort of scientific accuracy. Without such a correction, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 33. *Chapter "Global Temperature Change," Illustration, page 23.*

Comment: This is highly misleading and does not place the 2003 heat wave in context. The accompanying figure shows the exceedingly small nature of the anomaly, and that it was embedded in a quite moderate summer worldwide.



1000-500mb thickness anomaly in standard deviations, JJA 2003. From Chase et al., *Geophysical Research Letters*, 2006.

It is simply in appropriate to conflate this geographically small anomaly with global warming! Chase et al. note that there were no thickness anomalies above 3 standard deviations for calendar year 2003, while 5% of the planet experienced them in the very warm El Nino year of 1998.

Recommendation: I suggest you remove the illustration, because it will certainly draw a lot of negative attention to the CCSP if it stays in. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 34. *Chapter "Global Temperature Change," page 23, Last paragraph, second sentence "For example, the equatorial region that experiences tropical climate is expanding four times faster than predicted; this tropical belt is now as wide as climate models suggested it would be at the end of the century."*

Comment: This statement implies that climate models are not very accurately portraying the evolution of the earth's climate. Are the climate models wrong about the evolution of the tropics under and enhancing greenhouse effect or are the models under representing

the magnitude of natural variations in the size of the tropical belt? In either case, the observations indicate that climate models are in error and inaccurate.

Recommendation: Add a sentence to this paragraph explaining that the reasons that the observations differ from model projections are uncertain and could mean that natural variability and/or the climate evolution of the tropics is poorly handled by climate models. Without such a correction, the statement fails to meet the authors' claim of conveying the "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

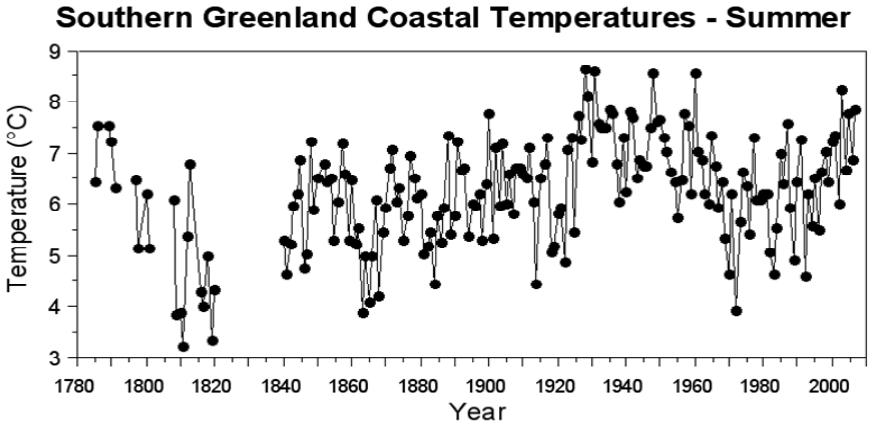
Specific comment 35. *Chapter "Global Temperature Change," Page 24, first paragraph. "After about 2000 years of little change, sea level rose…"*

Comment: See figure 11.7 in the IPCC TAR. It is obvious that sea level rise has been fairly constant since at least the mid 18th century.

Recommendation: Please correct the statement. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 36. *Chapter "Global Temperature Change," Page 24, third paragraph.*

Comment: Here is yet another example of rhetorical coloration. It begins by stating that melting of Greenland's ice would raise sea level by 20 feet, and then says that it is "losing ice mass at increasing rates". That's because Greenland was either gaining ice or was neutral through roughly 2000. Below is the southern Greenland temperature history from the Danish Meteorological Institute. It is very clear that temperatures in the last decade are hardly unusual. If Greenland is shedding ice at these temperatures, it had to have lost much more ice for the period from roughly from 1925 through 1960. CCSP provides no such perspective, and it needs to add the temperature history so readers can see the actual data.



Summer temperature history from southern Greenland. Source: Danish Meteorological Institute**.**

Further, Greenland can take much more integrated warming and retain ice. While CCSP says that it is experiencing "record amounts of surface melting in recent years", CCSP denies known climatic history.

The most comprehensive analysis of Eurasian temperature histories back to the end of the last ice age was published in 2000 by Glen MacDonald et al. in Quaternary Research. MacDonald et al. collated records of trees preserved the acidic environment that is now the Arctic tundra. The remains were dated by radiocarbon analysis.

The boundary between the northern forest and the bare tundra is currently south of the Arctic Ocean, and is determined by summer maximum temperatures. MacDonald found that "Over most of Russia, forest advanced to or near the current arctic coastline between 9000 and 7000 yr B.P. [before present] and retreated to its present position by between 4000 and 3000 yr B.P." In other words, the Eurasian arctic was considerably warmer than today for seven millennia!

How warm? "During the period of maximum forest extension, the mean July temperature along the northern coastline may have been 2.5 to 7°C [4.5-12.6°F] warmer than "modern".

One reason he gives for this warmth is "extreme Arctic penetration of warm North Atlantic Waters". The only entrance for this water is via the passage between Greenland and Europe. In other words, the east coast of Greenland was likely to have been warmer for *several millennia* and it did NOT shed its ice. Why is there no reference to this work in CCSP with regard to Greenland and sea-level rise?

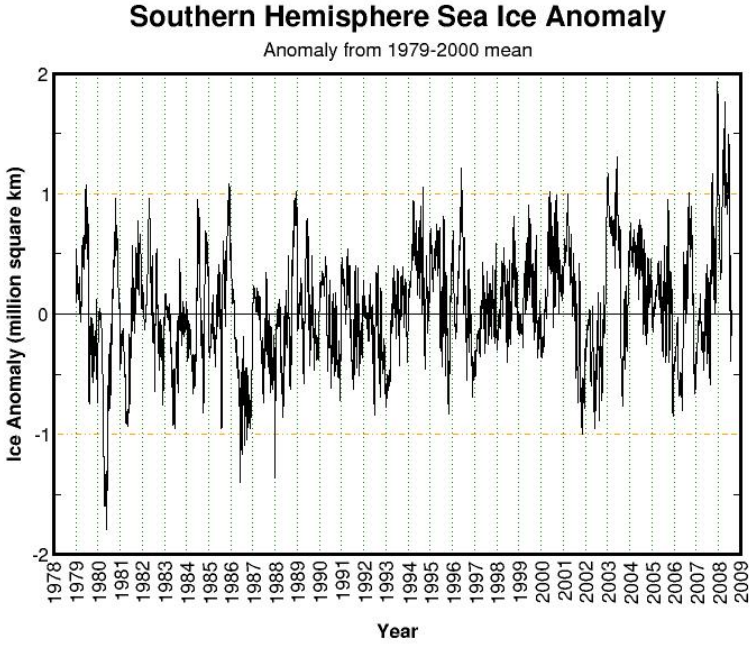
In a 2006 comprehensive review of regional temperature histories, Jason Briner from the University of Buffalo wrote in *Quaternary Research*, "…summer temperatures from Qipisargo Lake on southern Greenland were 2 to 4ºC [3.6-7.2ºF] warmer in the early Holocene [post-ice age era beginning around 11,500 years ago] versus the late Holocene [more recent era]…Greenland ice sheet borehole paleothermometry indicates a temperature change of ~3.5ºC [6.3ºF] between the middle and late Holocene [roughly 4,000-7,000 years ago]"

Finally, Luthcke et al, in *Science* in 2006 estimate the current rate of ice loss from Greenland at 25 cubic miles per year. Given that the total volume is approximately 670,000 cubic miles, the loss rate is 0.4% per *century*. In their 2007 review in *Science*, Shepard and Wingham confirm this figure.

These observations—of warmer temperatures in Greenland, and of very modest ice loss, are totally relevant to potential sea-level rise in the U.S. and are totally ignored by CCSP. Why?

Recommendation: These findings must be incorporated into the text. *Modify Key Finding 2b to reflect Greenland's history*. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 37. *Chapter "Global Temperature Change," Page 25 (entire).* Suggest you substitute BOTH the NH and SH monthly sea-ice anomaly plots from *Cryosphere Today*, which gives a much clearer picture. Using the latest data will reveal that the 2008 anomaly in the Arctic is pretty much back on the established trend line.



Southern hemisphere sea-ice anomalies, from Cryosphere Today. These figures prove that the CCSP explanation for the expansion is simply wrong.

The Southern Hemisphere anomalies clearly demonstrate that the explanation that the rise in SH ice anomaly is caused by ozone depletion is simply wrong. Yes, we have a model—Shindell's—but, no, it is clearly incorrect. Why?

Block out all of the data after 1995. There is clearly no rise in ice anomaly prior to then. Yet ozone depletion was substantial and slightly increasing. There has been a very slight decrease in that depletion in recent years. But, for all intents, the depletion has been fairly constant throughout the satellite record. So why is there no rise in ice until 1995? Please—I expect the CCSP would at least have the temerity to test its assertions against reality. Picking a model (Shindell's) when the data are readily available that invalidate that model is a scientific outrage.

Recommendation: Change the text and drop the ozone reference. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 38. *Chapter "Global Temperature Change," Page 26, Last paragraph, first and second sentences.*

Comment: See Specific Comment 29. Santer et al. (one of the members of the synthesis team) has shown that the vast majority of lower stratospheric cooling is because of ozone depletion, with only a very small residual caused by carbon dioxide. That should be stated in the text, even if it makes the solar argument a bit muddier (because the ozone-related cooling would likely dominate any solar warming).

Recommendation: Change the text to remove reference to observed lower stratospheric cooling being caused by and enhanced greenhouse effect. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

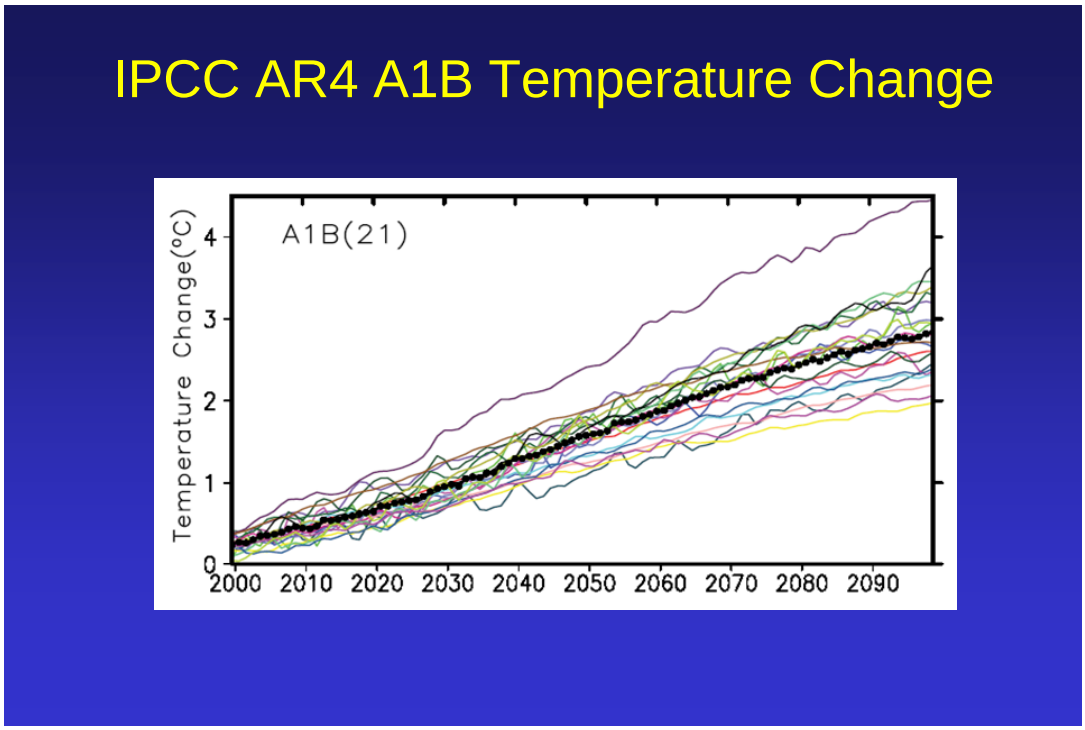
Specific comment 39. *Chapter "Global Temperature Change," Page 26, Bottom Figure*

Comment: The temperature trends in the lower stratosphere depicted in the figure have little to do with an enhanced greenhouse effect (despite the implication).

Recommendation: Remove the stratospheric temperature panel.

Specific comment 40. *Chapter "Global Temperature Change," First paragraph, page 28.*

Comment: CCSP states that 21st century warming will depend upon emissions and "how sensitive the climate will be". This needs a much greater explanation for the intended audience. Discriminate between emission ranges and sensitivity. I would add an additional illustration here to show different models under one scenario (the midrange scenario).



Warming projected by various models and the average (black line), IPCC midrange emission scenario.

Now comes the hard part. CCSP has got to stop the BS (I chose my words carefully) about the models and observed temperatures and come clean. *Something is very wrong*.

We are in our eleventh year without a net warming trend. Yes, I know this is in part because of the large 1998 El Niño. However, removing 1998 and the subsequent La Niña response (1999 and 2000), still yields no warming. One has to go back prior to the beginning of the second warming of the 20th century (pre-1975) to find such a string.

Then there is the problem caused by Keenlyside et al. (*Nature*, 2008), which indicates there may be no additional warming until the middle or the latter part of the next decade. Now—please point out that NOT ONE of the models used in the midrange scenario in AR4 has a 15-20 year period with no net warming. Not one. The implications of course, are manifold. First, the models must, in general, be predicting too much warming. Holding temperatures constant for 1-2 decades obviously delays any oceanic water vapor feed back for even longer!

Recommendation: The CCSP needs to state this somewhere, and page 28 looks like a pretty good place. Why not make a splash? We leave it up to some op-ed writing scientist when the public would be so much better served if CCSP itself brought up the problem?

This invalidates Key Finding #2, headline. Change or remove.

Specific comment 41. *Chapter "Global Temperature Change," Figure on the bottom of page 28.*

Comment: The caption and the graphic are very unclear and need expansion. The y-axis says "percentage change", the caption says "projected changes in the heaviest 5 percent of precipitation events". Does this mean that under the A2 scenario that the frequency of the 5% events will become 10%?

Further, CCSP needs to tell how important these numbers are(n't). The 96-99% (on an annual basis) precipitation events are largely no big deal. Consider a year with 100 precipitation days. The average amount on the highest day is in fact the realization of the 1-year "flooding" event for that year. Obviously it is the 50-100 year events (which should show some increases in some places, given (for example) the secular increase in continental US rainfall) that are of much more interest.

Recommendation: I suggest a much different figure here, detailing a change in, say, 100-year flood frequencies, because picking on the top 5% arguably is picking upon many rain events that are much more beneficial than detrimental.

Specific comment 42. *Chapter "Global Temperature Change," Page 29, fifth paragraph*

Comment: First, see above comments on the stability of the Greenland ice cap when it was warmer for *millennia*. The statement about "additional processes are at work which affect the dynamic response" of ice sheets has to be couched in the findings of van de Wal in *Science* (2008) and showing very limited response, Howat et al. (*Science, 2008),* reporting a slowing of major outlet glaciers to previous rates, and Joughin et al. (*Science*, 2008) showing minimal acceleration of outlet glaciers. It is quite clear that the balance of evidence argues against rapid ice loss from Greenland.

Recommendation: These findings need to be mentioned and referenced. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 43. *Chapter "Global Temperature Change," Page 29, Last paragraph.*

Comment: To give some sense of the nature of the range of sea-level rise projections, I think it would be fair for inclusion of a sentence about estimates from the IPCC mid-range emissions scenario, noting that the lower bound was raised from its previous (2001) estimate and the upper bound was lowered, from 28 to 19 inches.

Specific comment 44. *Chapter "Global Temperature Change," Page 31, Last paragraph, "Recent observations show that melting on the surface of an ice sheet…*

Comment: The implication is that moulins dramatically accelerate glacial flow. In fact, recent observations show this to be a relatively minor phenomenon (Das et al, *Science*, 2008).

Recommendation: Mention this work and modify the statement. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 45. *Chapter "Global Temperature Change," Page 31, Last Paragraph, "Recent studies suggest that sea level could rise as much as 3 to 5 feet per century over the next several centuries"*

Comment: this statement is from a single citation from Rhamstorf.

Recommendation: Why not cite the much broader literature from which the IPCC estimates were made than a single reference? As it now stands, the statement violates applicable objectivity requirements.

Broad comment on Greenland. CCSP's reporting on ice sheet collapse, especially with respect to Greenland, is remarkably one-sided, biased, and ignores a large body of the most recent literature. As this stands now, expect strong and repeated public criticism of the CCSP report.

Selected Comments on the "National Climate Change" Chapter

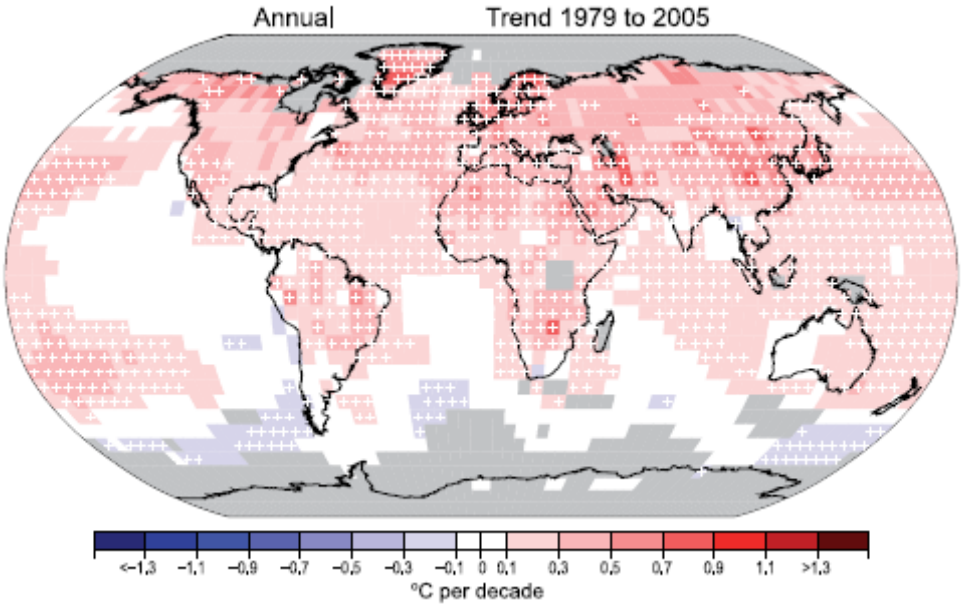
Specific comment 45. *Chapter National Climate Change, page 33, first paragraph, first sentence, "Like the rest of the world, the United States has been warming significantly over the past 50 years in response to the build-up of heat trapping gases."*

Comment: That is a poor description of the temperature history of the U.S. during the past 50 years. In fact, based upon the Figure, the average temperature in the U.S. declined from 1958 to the late-1970s, and then has been warming since, being consistently warm since 1998. Thus the warming has been occurring for the past 30 years. It seems odd to include 20 years of cooling in with 30 years of warming to conclude that we have been warming for 50 years.

Recommendation: Change "past 50 years" to "past 30 years." Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 46. *Chapter National Climate Change, page 33, first paragraph, last sentence, "…whereas the warming of recent decades has been truly global—hence the term* global *warming."*

Comment: Below are the surface temperature trends from 1979 to 2005 ("recent decades") taken from the IPCC AR4 page 250. Notice that large areas of the eastern Pacific Ocean, southern Atlantic Ocean, and southern Indian Ocean show no temperature change and many portions of the southern oceans around Antarctica show significant cooling trends. Clearly, the "warming in recent decades" has NOT been global.

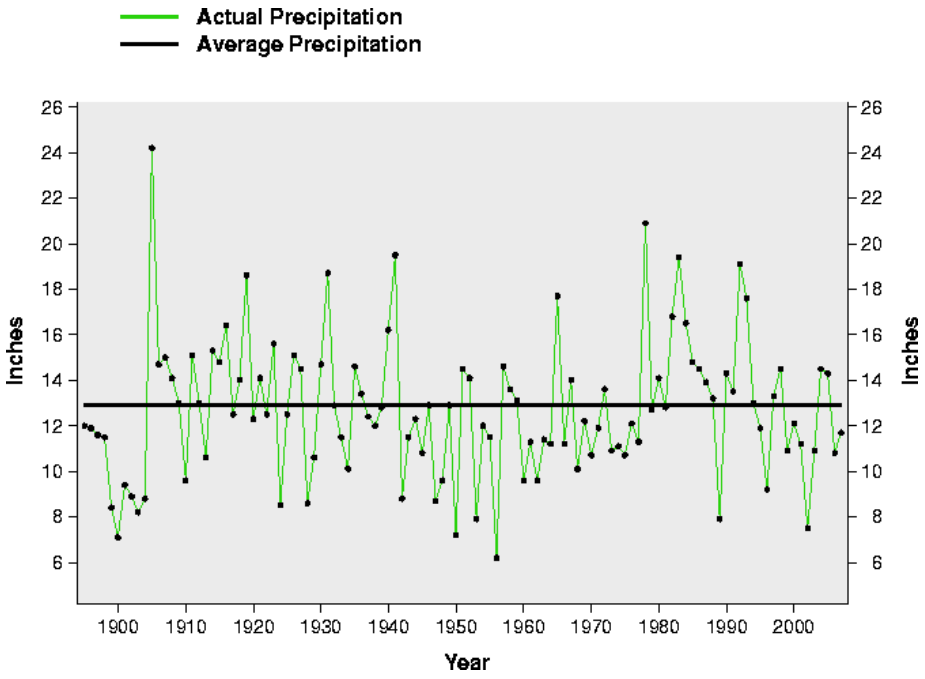


Surface temperature trends, 1979-2005. Significant trends are hatched (source: IPCC AR4, p. 250).

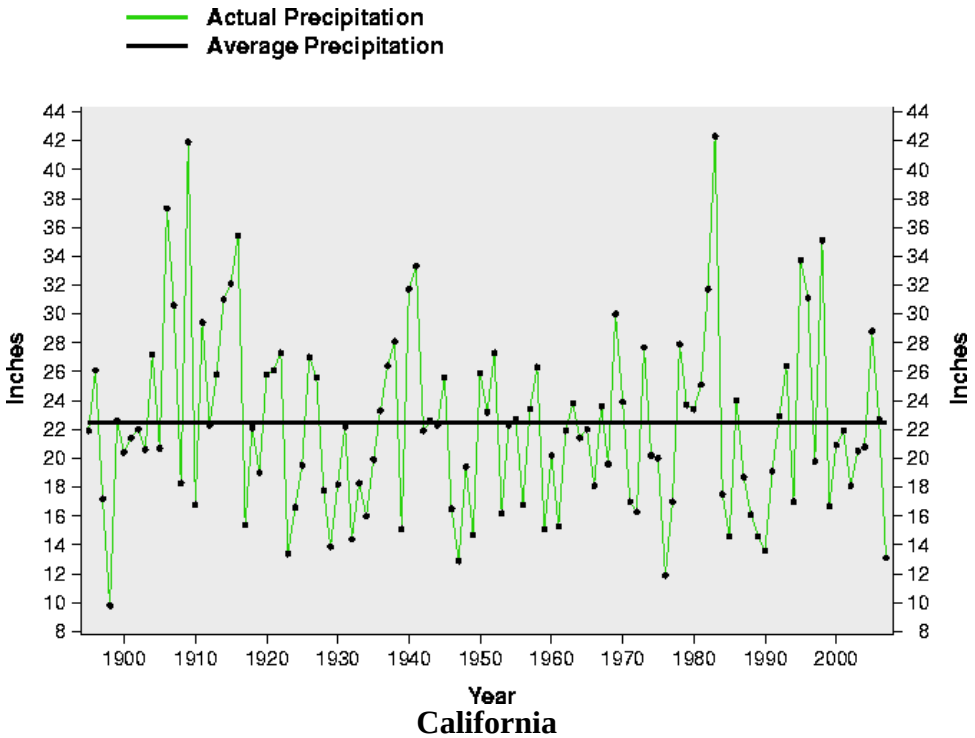
Recommendation: Remove this very non-scientific description of "global warming," and replace the maps of specific years (i.e., weather) with the IPCC map of climate trends. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 47. *Chapter National Climate Change Page 36, paragraph 1. "…drier areas, such as the Southwest, have generally become drier".*

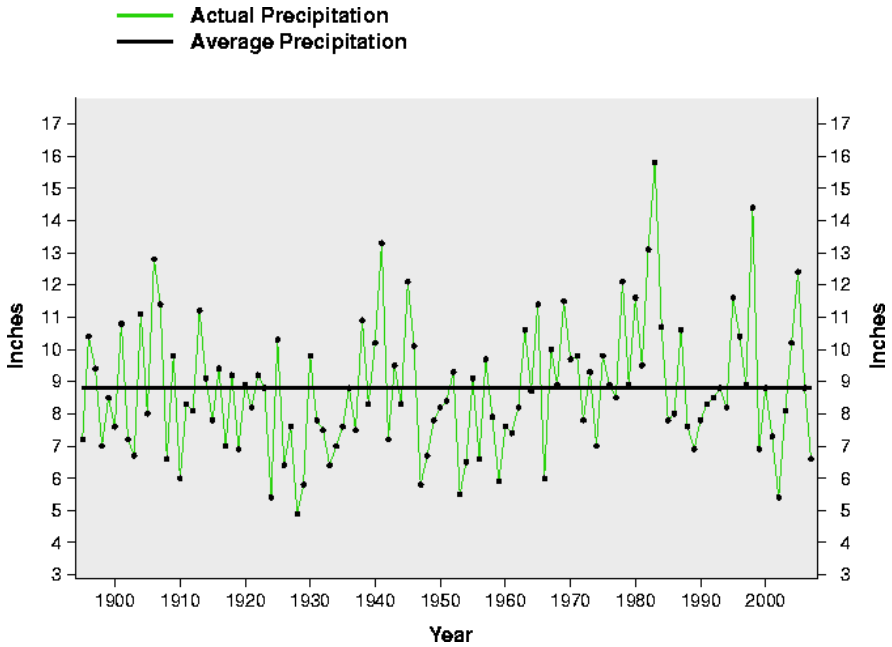
Comment: Below are graphs of annual precipitation from NCDC for Arizona, California, Nevada, and New Mexico. I believe this is the U.S. Southwest.



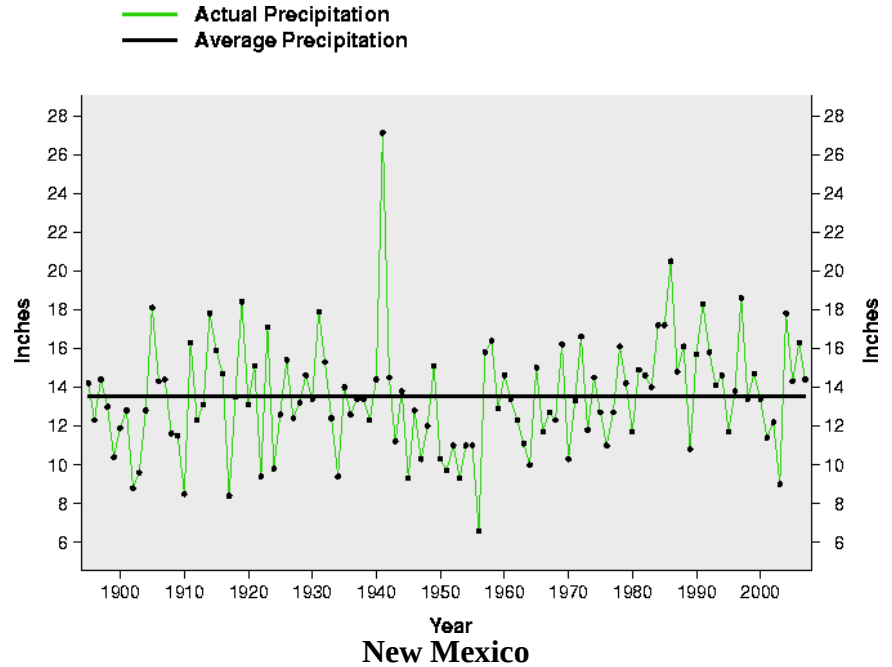
Arizona



California



Nevada



New Mexico

"….the Northeast, ha(s) generally become wetter".

Comment: I'm not going to waste time here plotting things, but Maine precipitation has declined significantly, while New York and Pennsylvania have seen significant increases.

However, the magnitude of the increase in the pre-war period is generally the same as in the postwar era. So what caused the large increase in the early 20th century that is the same magnitude as the later one? The obvious answer is something like "secular variation", which makes it hard to distinguish causation for the latter increase.

Recommendation: Change the text so that it reflects reality. Modify the text to say that some areas in the southwest have shown a decrease in precipitation, but, on the average, regional precipitation is unchanged since records began in 1895. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 48. *Chapter National Climate Change, Comment on temperature and precipitation maps for the U.S.*

Comment: According to CCSP, "The maps…are based on sixteen models' projections of future temperature". Later, there is NO mention of the model sourcing for the precipitation maps, but the reader is left to assume that it is from the "16" models noted on page 36. The CCSP needs to show that the temperature projections are fairly robust across models, by putting in a graphic with all 16 maps for 2050 or 2090, and then it must do the same for all the precipitation models.

Let me argue to CCSP's selfish interest. If you can't do precipitation reliably, you can't have the surface energy balance or the vertical distribution of moisture right, either. Which means that there is something likely to be fundamentally fishy with the temperature projections, too…so how come they can look so alike when the precipitation maps look so different? Pointing this out in the CCSP report only serves to demonstrate how much more research is needed, and that the science is hardly "settled", except that everyone agrees that carbon dioxide results in a slightly warmer lower atmosphere, which has been "settled", I think, for something like 130 years or so. So telling the sad truth might actually increase CCSP funding, no?

Specific comment 49. *Chapter National Climate Change, U.S. precipitation, page 36-37*

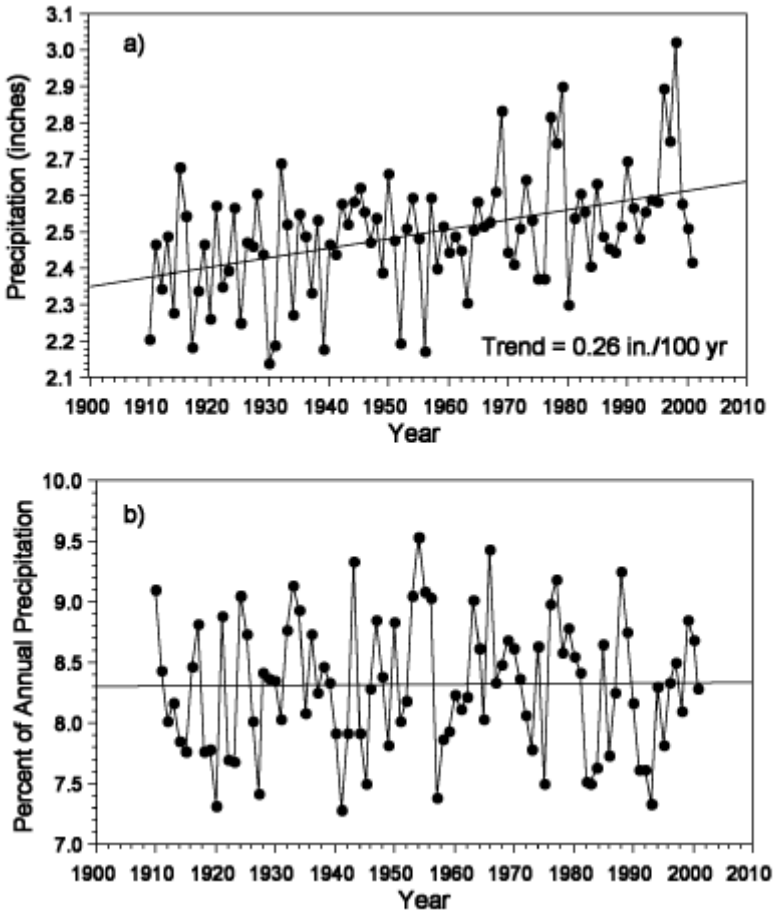
Comment: There is no map of the observed U.S. precipitation trends.

Recommendation: Include a map of observed trends in total annual precipitation across the U.S.

Specific comment 50. *Chapter National Climate Change, U.S. precipitation, page 36, second paragraph, last two sentences.*

Comment: Not so sure about these numbers. While we showed that precipitation on the wettest day of the year for stations across the U.S. (roughly the wettest few percents of

rain events) was increasing, we also showed that it was not increasing faster than the total annual precipitation. (Michaels, et al., 2004, Trends in precipitation on the wettest days of the year across the contiguous USA, *International Journal of Climatology*, **24**, 1873-1882.).



Top: Trend in the average precipitation falling in the wettest day of the year (averaged across the U.S.). Bottom: Trend in the average annual percentage of precipitation falling on the wettest day of the year. (source: Michaels, et al., 2004, Trends in precipitation on the wettest days of the year across the contiguous USA, *International Journal of Climatology*, **24**, 1873-1882.).

Recommendation: Change the text to reflect our findings. Also, the caveats that are stated in association with the total precipitation trends, i.e. "with individual locations ranging from much more to much less than this average" also should be associated with findings of the trends of the heaviest 1% of precipitation (reported in the previous sentence). Why mention them in one place and not the other? Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 51. *Chapter National Climate Change, U.S. precipitation, page 36, second paragraph*

Comment: Please explain that the nature of the distribution of daily precipitation amounts is such that an increase in total annual precipitation is accompanied by an increase in heavier precipitation amounts. Therefore, by nature, areas with increasing trends in total annual precipitation will also have increasing trends in heavier daily precipitation amounts while areas trending towards less total annual precipitation will typically receive less precipitation in heavy daily events. Such a correspondence would be readily visible if a map of annual precipitation trends was presented along with the regional graphs of the changes in precipitation trends by intensity level.

Recommendation: Include text to explain this correspondence. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 52. *Chapter National Climate Change, Page 36, third paragraph. "Model projections of future precipitation generally suggest continuations of observed patterns, with northern areas becoming wetter and southern areas, particularly the west, becoming drier".*

Comment: See graphs above in Comment 47 and the content of Comment. As the teacher would say here "Show your work!"

Specific comment 53. *Chapter National Climate Change, U.S. precipitation, page 37, maps*

Comment: A simple averaging of the climate models does not accurately present the range of model outcomes when it comes to projected precipitation changes. The patterns of precipitation changes are quite different from model to model. This uncertainty is not represented at all by the maps as presented.

Recommendation: Replace the existing maps with histograms showing the distribution of the model projections for precipitation changes for various regions of the country at various times in the future. This way the reader can get a sense of model agreement/disagreement. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 54. *Chapter National Climate Change, U.S. precipitation, page 37, bar chart*

Comment: This bar chart at the bottom of the page is uninterpretable. It shows the projected changes from what baseline? As we showed (Michaels, et al., 2004, Trends in

precipitation on the wettest days of the year across the contiguous USA, *International Journal of Climatology*, **24**, 1873-1882.) it is totally inappropriate to show percentile changes in fixed bins. I have a suspicion this is what is presented here, but I can't be sure because the description is inadequate.

Recommendation: Remove this chart as it is uninterpretable. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 55. *Chapter National Climate Change, Storms, page 38, first paragraph, second sentence, "Many extremes and their associated impacts are now changing."*

Comment: This statement in not supported in the scientific literature. For example, see any of the articles listed below for analyses of the trends in various extreme weather events and the related impacts—in virtually all cases, they find that 1) few long-term trends in extreme events, and 2) little change in the impacts when changing population and economic statistics are properly included in the analyses.

Pielke, Jr., R. A., Gratz, J., Landsea, C. W., Collins, D., Saunders, M., and Musulin, R., 2008. Normalized Hurricane Damages in the United States: 1900-2005. Natural Hazards Review, Volume 9, Issue 1, pp. 29-42

Downton, M., J. Z. B. Miller and R. A. Pielke, Jr., 2005. Reanalysis of U.S. National Weather Service Flood Loss Database, Natural Hazards Review, 6:13-22.

Downton, M. and R. A. Pielke, Jr., 2005. How Accurate are Disaster Loss Data? The Case of U.S. Flood Damage, Natural Hazards, Vol. 35, No. 2, pp. 211-228.

Pielke, Jr., R. A., 2005. Are there trends in hurricane destruction? Nature, Vol. 438, December, pp. E11. Brief comment on K. Emanuel's "Increasing destructiveness of tropical cyclones over the past 30 years", Nature, Vol. 436, pp. 686-688.

Pielke, Jr., R.A., S. Agrawala, L. Bouwer, I. Burton, S. Changnon, M. Glantz, W. Hooke, R. Klein, K. Kunkel, D. Mileti, D. Sarewitz, E. Thompkins, N. Stehr, and H. von Storch, 2005.Clarifying the Attribution of Recent Disaster Losses: A Response to Epstein and McCarthy, Bulletin of American Meteorological Society, Volume 86 (10), pp. 1481-1483. Reply by P.R. Epstein and J.J. McCarthy.

Changnon, S., R. A. Pielke, Jr., D. Changnon, D., R. T. Sylves, and R. Pulwarty, 2000. Human Factors Explain the Increased Losses from Weather and Climate Extremes, Bulletin of the American Meteorological Society, 81(3), 437-442.

Pielke, Jr., R. A. and M.W. Downton, 2000. Precipitation and Damaging Floods: Trends in the United States, 1932-97. Journal of Climate, 13(20), 3625-3637.

Kunkel, K., R. A. Pielke Jr., S. A. Changnon, 1999: Temporal Fluctuations in Weather and Climate Extremes That Cause Economic and Human Health Impacts: A Review. Bulletin of the American Meteorological Society, 80, 6, 1077-1098.

Recommendation: Remove this sentence. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 56. *Chapter National Climate Change, Storms, Page 38, first paragraph, third sentence "The U.S. has been experiencing more unusually hot days and nights".*

Comment: OK, that's true, but incompletely misleading. I don't think winter nights are generally considered "hot", so this must be talking about the warm part of the year. You *could* reference, say Michaels et al. in *Climate Research* in 2001, which clearly demonstrates that the coldest nights of the winter are warming more than any other diurnal/seasonal combination.

This is just another example of the remarkable one-sidedness of the CCSP report. The more I go through it, the more I think that it has been so poorly done on purpose—so that the reviewers would essentially write you a much better document without the synthesis team having to do much at all except spout rhetoric that looks more like a NRDC or Sierra Club document (or dare I say a HBO global warming infomercial) on global warming than a real scientific report.

Specific comment 57. *Chapter National Climate Change, Storms, page 38, first paragraph, fifth sentence, "Droughts are becoming more severe in some regions."*

Comment: This is quite a slanted sentence. What about in the other regions? Are the drought severity trends driven by droughts in recent years (i.e. heavily weighted because they fall near the end of the record)?

Van der Schrier et al. (van der Schrier, G., K. R. Briffa, T. J. Osborn, and E. R. Cook, 2006. Summer moisture availability across North America, *Journal of Geophysical Research*, **111**, D11102, doi:10.1029/2005JD006745) find no trend in the percentage area of the contiguous United States experiencing either moderate or extreme moisture availability conditions for the 1901–2002 period.

Recommendation: Either fully describe the drought trends across the United States for the past 50 years (the same period you use in the temperature trends descriptions) or remove this sentence. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 58. *Chapter National Climate Change, Storms, page 38, second paragraph and (charts on hurricanes)*

Comment: This discussion of hurricanes is either grossly out of date or deliberately ignoring the recent scientific literature.

The first sentence of the paragraph discusses the recent increases in the power and frequency of Atlantic hurricanes and then the following sentence discusses projections for the future. The implication of these back-to-back sentences (although admittedly unstated) is that recent changes are driven by global warming. As this is an unfair treatment of the state of scientific knowledge, rather than leading the reader to believe this, the CCSP authors should make it plainly clear that the scientific literature does not support the idea that recent changes in Atlantic hurricane patterns are "likely" driven by global warming. It should also be made abundantly clear that the scientific literature of future hurricane changes finds it "likely" that hurricane frequencies will *decline* in the future, and gives unclear guidance on future intensity.

A pretty comprehensive listing of recent papers in the scientific literature concerning climate change and hurricanes is available from NOAA's AOML (http://www.aoml.noaa.gov/general/lib/Regional/climate_change/climatechangechrono.htm). You will find that a great deal of the recent papers do not support the general sense that the CCSP authors leave with the reader.

Recommendation: This paragraph needs be brought up to date with the literature. As it stands, it is a poor and biased treatment of the current state of scientific understanding. As it now stands, the paragraph fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 59. *Chapter National Climate Change, Extreme Weather, Page 39, Figure b,*

Comment: Again it is important to show the spread of the models.

Recommendation: There needs to be some kind of hatching on the models where the ensemble is significant or where it is too dispersed. Refer to my general comment above on showing all of the precipitation results.

Specific comment 60. *Chapter National Climate Change, Emissions of Heat Trapping Gases, page 40, second paragraph, third sentence*

Comment: If sinks in the U.S. currently take up the equivalent of about one-third of U.S. emissions annually, then how can the statement in the preceding paragraph that "One-third of the carbon dioxide released from fossil fuel burning remains in the atmosphere after 100 years, and one-fifth of it remains after 1000 years" be correct? For the case of U.S. emissions, if we stopped all emissions today, all of them would be taken up by U.S. sinks in just 3 years. So how can the atmospheric lifetime be 100s to 1000s of years? This makes no sense.

Recommendation: The atmospheric lifetime of CO2 and how it relates to carbon sinks needs to better be described.

Specific comment 61. *Chapter National Climate Change, Emissions of Heat Trapping Gases, Page 41, Second paragraph, "The thawing of permafrost due to warming will cause this peat to decompose, releasing methane and carbon dioxide. The potential is enormous:…"*

Comment: No reference is given, but one that should be is Delisle (2007) *Geophysical Research Letters*, who used a 600 layer model, which extended 300 feet into the permafrost. Previous models go about 10 feet. With this more complete model, Delisle reports that continuous permafrost in Alaska and Siberia will survive over the next 100 years, even if a significant warming takes place. Delisle wrote: "Based on this result and on the presented analysis, it appears that all areas north of 60°N will maintain permafrost at least at depth. North of 70°N, surface temperature values today are in general below -11°C. These areas should maintain their active layer. It appears unlikely that almost all areas with near-surface permafrost today will lose their active layer within the next 100 years". Delisle claims that the new model is far more consistent with field measurements and far more realistic in terms of including the interaction with the deeper and colder permafrost core.

Another common fear is that melting of permafrost will release trapped methane. Delisle notes this at the end of his article:

> "A second, rarely touched upon question is associated with the apparently limited amount of organic carbon that had been released from permafrost terrain in previous periods of climatic warming such as e.g. the Medieval Warm Period or during the Holocene Climatic Optimum [the warmer millennia after the end of the recent ice age—see our next section]. There appear to be no significant CH4 [methane] excursions in ice core records of Antarctica or Greenland during these time periods which otherwise might serve as evidence for a massive release of methane.

Recommendation: CCSP needs to note these findings. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific Comments on the "Society" Chapter

Specific comment 62. *Chapter Society, page 42, bullet 1*

Recommendation: Change the phrase "more Americans vulnerable to climate change impacts" to "more American vulnerable to climate impacts." Projected climate change plays only a very small part in overall natural climate and climate variability of a region.

Specific comment 63. *Chapter Society, page 42, bullet 3*

Recommendation: This bullet is plain and simply the pessimistic and selective opinions of the authors and apparently (there is no citation given) not based at all on any sort of analysis. Please add the citation to a quantitative ranking of the activities that "Americans hold dear" and a weighting of how the opportunities to pursue those ranked activities are projected to change in the future. Are the reduced opportunities offset by increased opportunities to pursue other activities that perhaps even more of us hold dear? Please cite the appropriate analysis or remove the bullet. After all, this report is supposed to be science-based, is it not? **Without such a correction, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.**

Specific comment 64. *Chapter Society, Page 42, Bullet 3. "Climate change will affect the tourism and recreation industries in ways that reduce opportunities for many activities that Americans hold dear".*

Comment: True—MAYBE—but people always have found different ways to entertain themselves. Won't there be more people going to the beach if it is warmer? Won't more people go in the water if is warmer?

Recommendation: This silly bullet needs to be changed to something like "climate change is likely to change the variety of recreational activities that Americans enjoy". Instead, what you have just asserted is another example of the "stupid people hypothesis". Has anyone at CCSP noted the increasing number of combined ski/golf resorts?

Specific comment 65*. Chapter Society, page 43, second paragraph, first sentence. "Because societies and their built environments have developed in concert with a relatively stable historical climate…"*

Comment: Are the writers of this sentence at all familiar with the climate history of the United States??? There are scores of books and articles written about the impacts of climate variations on virtually every aspect of the development of the U.S.

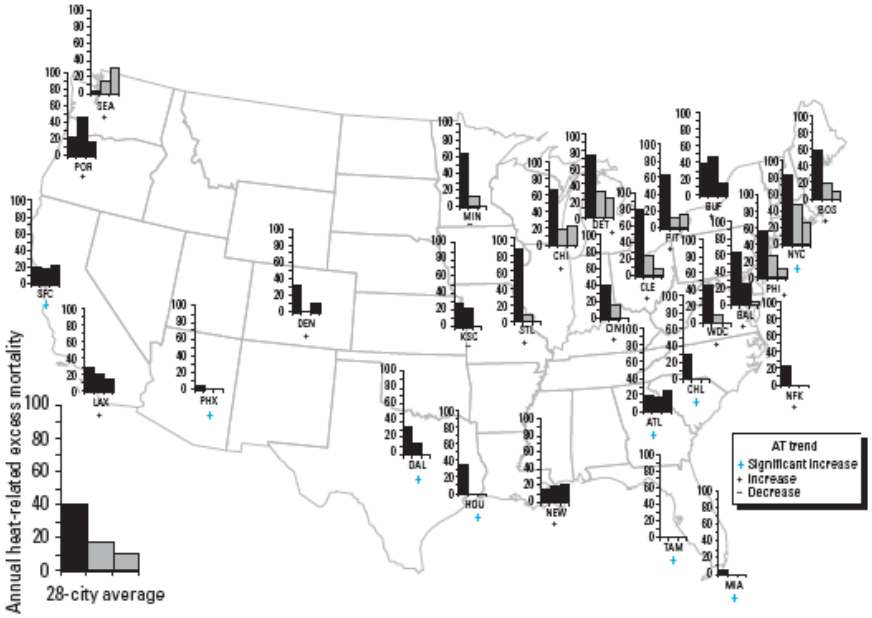
Recommendation: Remove this sentence. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 66*. Page 43, second paragraph: "Because societies…have developed in concert with a relatively stable historical climate….*

Comment: See specific comment 2, above.

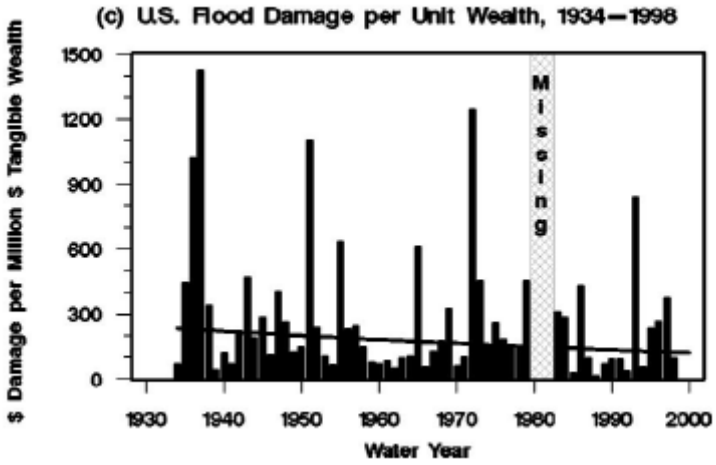
Specific comment 67*. Chapter Society, page 43, second paragraph, second sentence. "Society is especially vulnerable to extremes, such as heat waves and floods, many of which are increasing as climate changes"*

Comment: Several papers in the peer-reviewed scientific literature have found that the vulnerability of American cities to heat waves has been declining since the 1960s, despite rising heat (Davis et al., 2003, *International Journal of Biometeorology*; Davis et al., 2003, *Environmental Health Perspectives*) and others have shown that the impact of flooding on American society has shown a slight decline of the 20th century (Downton et al., 2005, *Natural Hazards Review*).



Annual average excess summer mortality due to high temperatures, broken down by decade, for 28 major cities across the United States. For each city each of the three bars represents the average mortality during successive decades (left bar 1964-66 + 1973-1979; middle bar 1980-1989, right bar 1990-1998). Bars of different color indicate a statistically significant difference. No bar at all means that no

temperature/mortality relationship could be found during that decade/city combination (taken from Davis et al., 2003, *Environmental Health Perspectives*).



U.S. flood damage normalized for changes in tangible wealth (Downton et al., 2005).

Recommendation: Remove this misleading sentence. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 68*. Chapter Society, page 44, Pull-quote at top. "Population movements…"*

Comment: This statement is silly. Does a scenario exist in which future population growth and population dynamics will occur in such a way as to reduce American's vulnerability to climate change? More people = more vulnerability.

Recommendation: Remove this pull quote.

Specific comment 69*. Chapter Society, page 44, Second paragraph, second sentence, "For example, the most rapidly growing area of the country is the mountainous West…"*

Comment: The map of population growth on p. 44 does not reflect the statement. Southern California, Texas, Florida and the major cities of the U.S. appear to be the fastest growing areas. The 'mountainous West' doesn't seem to be anywhere near the fastest growing area.

Recommendation: Remove this sentence or explain why it doesn't square with the map.

Specific comment 70*. Chapter Society, page 44, Second paragraph, last sentence, "…even as the probability of harm increases."*

Comment: It is well-established in the literature that damage from hurricanes in the U.S. has not increased when changes in wealth and population are taken into account (Pielke Jr. et al., 2008, *Natural Hazards Review*). Nor is their a strong consensus in the most recent scientific literature that an enhanced greenhouse effect will lead to an increased risk of harm along to the United States from future hurricanes (once population and wealth changes are accounted for.)

Recommendation: Remove this misleading sentence. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 71. *Chapter Society, page 44, Third paragraph, last sentence, "…and share significant vulnerability to coastal storms, severe drought, sea-level rise, air pollution, and urban heat island effects."*

Comment: First of all, Florida and Texas are relatively immune from heat-related mortality in their urban regions (e.g., Davis et al., 2003, *International Journal of Biometeorology*; Davis et al, 2003, *Environmental Health Perspectives*). And secondly, the vulnerability to these cites is to the elements of *climate* much more so than to *climate change*. Further, the population growth in these states mean that people are moving there by choice, either because of, or in spite of the climate.

The overall tendency of the U.S. populace has been to migrate to warmer locations. Despite people experiencing the exact conditions that this report argues will create disastrous consequences to society, they not only are surviving, but seem to be thriving. I don't understand why so many people would willingly place themselves into such grave danger? Obviously, most Americans who have the wherewithal prefer to live were it is warmer.

Do not conflate increasing temperatures from greenhouse gas increases to the urban heat island. These are completely different warming processes that have nothing to do with each other. In fact, the rates of urban climate change can be effectively used to argue against many of the impacts you discuss in this section. Urban dwellers in the past few decades, have experienced a warming that is far greater (in many cases) than any predicted warming from greenhouse gases that will occur over the next 100 years. For example, a person moving from, say, Manassas to Georgetown will experience a temperature change that far exceeds what is expected to occur from a doubling of greenhouse gases. Yet despite these changes, there is no evidence of any major, negative impacts on city dwellers from living in a warmer locale. But this is a completely different issue and it is confusing. My suggestion is to remove all references to urban heat islands from your revision throughout the document.

Recommendation: Emphasize that the increased vulnerability from population movement is from the elements of climate much more so than climate change. In other words, the Gulf Coast cities are vulnerable to hurricane impacts…which are an element of the region's climate. If future climate change somehow modifies the occurrence of hurricanes, this change is small compared with the fact that hurricanes exist in the first place. The same is true for water stresses in the desert and heavy rain in the northeast. These events are part of the region's climate, climate change won't alter that fact. Without such a clarification, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 72*. Chapter Society, page 45, Third paragraph, first sentence,*

Comment: Why have you skipped over climate as one of the considerations in this discussion of why American's make the development choices they do? You list a host of things, but leave out "the type of climate" when undoubtedly this is a major consideration of many Americans. The fact is American's are (re)locating in regions of the country with a warmer climate. More American's are probably at climate risk simply by relocating than by actual climate change.

Recommendation: Add to the list of things American's consider when developing, "the region's climate." Without such an inclusion, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 73*. Chapter Society, page 46, Box "Spotlight in Alaska Coastal Villages" Second paragraph, first sentence, "…ancestral homes…*

Comment: The Inuit people of Alaska have been historically nomadic. As such, they didn't have "ancestral homes." These coastal villages are a relatively new feature as the Inuit have been forced to settle rather than pursue their nomadic lifestyle. Establishing these villages on the Alaska coastline was a bad idea any way, and the coastline has been eroding for at least 50 years. See any of these references:

Harper, J.R., 1978. Coastal erosion rates along the Chukchi Sea coast near Barrow, Alaska. *Arctic*, **31(4),** 428-433.

Hartwell, A.D., 1973. Classification and relief characteristics of northern Alaska's coastal zone. *Arctic*, **26(3),** 244-252.

Hume, J.D., and M. Schalk, 1967. Shoreline processes near Barrow Alaska: a comparison of the normal and the catastrophic. *Arctic*, **20(2),** 86-103.

Hume, J.D., et al., 1972. Short-term climate changes and coastal erosion, Barrow Alaska. *Arctic*, **25(4),** 272-278.

Lewellen, R., 1977. *A study of Beaufort Sea coastal erosion, northern Alaska. Environmental Assessment of the Alaskan Continental Shelf,* Annual Reports of the Principal Investigators, Vol. XV (Transport). National Oceanic and Atmospheric Administration, pp. 491-527.

MacCarthy, G.R., 1953. Recent changes in the shoreline near Point Barrow, Alaska. *Arctic*, **6(1),** 44-51.

Recommendation: Change the sentence to read "A number of villages are now facing the prospect of having to relocate to safer ground as they were unwisely built upon an unstable and eroding shoreline."

Specific comment 74. *Chapter Society, Page 46, box. "A number of villages are now facing the prospect of having to abandon their ancestral homes and relocate to safer ground".*

Comment: These residents were warned of this by Hume and Schalk in *Arctic* in 1967! Following their analysis of a very damaging storm in 1963, they said "Another such storm can be expected, and care should be exercised in the selection of building sites…[which] would be at least 30 feet above sea level and either inland or along a coast which is not eroding".

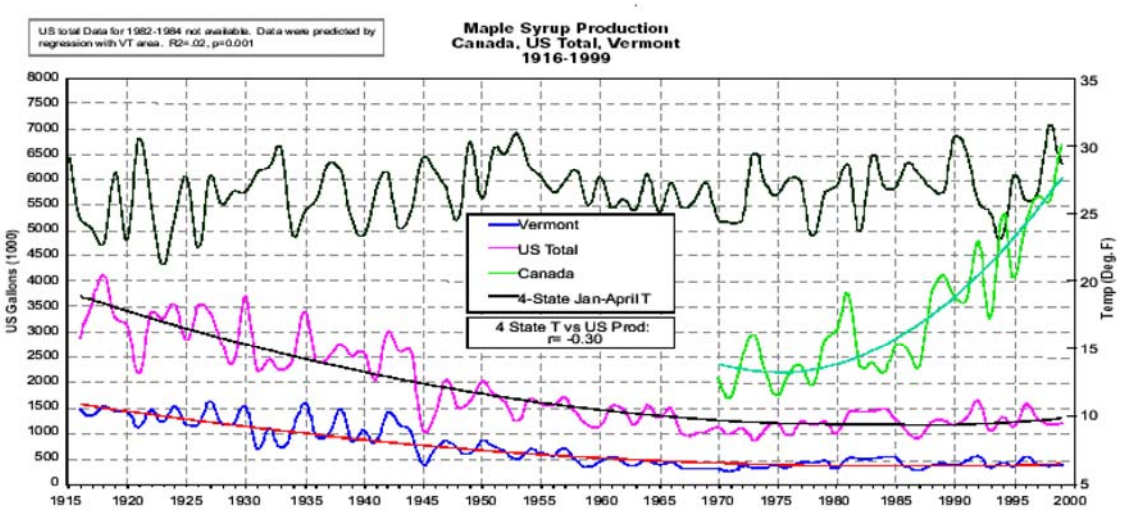
Recommendation: Change the box and note that the problem of rapid erosion of the north and west Alaskan coast has been known for over four decades, and that with or without global warming, this continues. Global warming may, in fact, be the final stimulus that forces these populations to adapt into safer ground.

Specific comment 75. *Chapter Society, page 46, Last paragraph, second sentence, "For example, maple syrup production is heavily reliant…*

Comment: This statement about maple syrup production is simply an oft-repeated claim that is not supported by an analysis of the climate data.

I reproduce Figure 5.5 from the NERA report (NERA, 2001. The New England Regional Assessment – *Preparing for a Changing Climate: the Potential Consequences of Climate Variability and Change. New England Regional Overview*. U. S. Global Change Research Program, Durham, NH, 96pp.) that depicts the critical January-April temperature history (averaged across the states of VT, NH, ME, NY) along with the maple syrup production history from Vermont, the U.S., and Canada. Notice that temperature history is dominated by short-term annual and decadal fluctuations more

than by an overall long-term trend, at least over the past 70-80 years. The January-April temperature history is better characterized by three periods—a warming from 1915 to the mid-1950s, a cooling from the mid-1950s to the early 1970s, and then a warming since then. Notice that New England's maple syrup production history does not comport with these multi-decadal fluctuations. Syrup production declined from about 1915 to about the early 1970s, and has remained relatively steady since then. The decline took place during a period of warming *and continued through a period of cooling temperatures.* Obviously, cause and effect are not operating here. Further, during the past several decades, a period of slightly warming temperatures, production appears to show no overall trend. In Canada, syrup production has increased since records are first available in 1970—slowly at first, and then more rapidly during the past 2 decades—and bears little relationship to the temperature history depicted in the Figure. Overall, this indicates that other aspects of the maple syrup industry largely dominate the patterns of syrup production—such things as changing technology, number of trees tapped, economic conditions, etc.



Maple syrup production in Vermont (blue line), total United States (pink line), Canada (green line) and the 4-state (VT, NY, ME, NH) average January-April (black line) temperature history. This figure is reproduced from the New England Regional Assessment, Figure 5.5. (NERA, 2001. The New England Regional Assessment – *Preparing for a Changing Climate: the Potential Consequences of Climate Variability and Change. New England Regional Overview.* U. S. Global Change Research Program, Durham, NH, 96pp.)

Recommendation: Remove the sentence on climate as the primary driver responsible for shifting maple syrup production from New England to Canada. Without such a correction, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 76. *Chapter Society, page 46, Last paragraph, last sentence, Citation 4*

Comment: Citation 4 is to a report from the Union of Concerned Scientists!

How does the UCS report fit into the description of the "About this Report" of page 14:

This report is based on published, peer-reviewed data and reports including the Synthesis and Assessment Products completed by the U.S. Climate Change Science Program (CCSP, 2006 through 2008), the Intergovernmental Panel on Climate Change (IPCC, 2007) assessments, the U.S. National Assessment of the Consequences of Climate Variability and Change (NAST, 2000 through 2001), the Arctic Climate Impact Assessment (ACIA, 2004 through 2005), the National Research Council's Transportation Research Board report on the Potential Impacts of Climate Change and U.S. Transportation (NRC, 2008), and other peer-reviewed assessments.

To incorporate the latest findings and fill gaps, this report also draws directly from articles in peer-reviewed scientific journals as well as widely available government data and information compiled on a regular basis for public use, including census figures and statistics on energy usage and greenhouse gas emissions. The author team did not conduct original research for this report, but rather drew on existing peer-reviewed research. In order to convey the most relevant and up-to-date information possible, the report does contain summaries, tables, and graphics using updated data sets drawn from peer-reviewed literature and official government data.

In fact, the UCS report does not fit into this description at all.

Recommendation: Either remove the reliance on the UCS report, or redo the "About this Report" section to indicate that you are relying on information from advocacy organizations with an openly-stated political agenda. Otherwise you are grossly misleading the reader.

Specific comment 77. *Chapter Society, Page 47. Entire page.*

Comment: Virtually every change is for the worse. How can this be? I can name plenty of activities that would welcome shorter winters: virtually all of the "summer sports", for example. This page needs balance—I leave it up to the writers to supply some. It also needs a teeny bit of economics. If the ski season becomes compressed, there are fewer days available, which means the supply is lowered. Consequently, the price of lift tickets will rise, lowering the number of days required for break-even. You might mention that skiing might become more expensive and that people will naturally substitute other forms of recreation (or even hold on to more warm-season activities). In addition, reference 8 about the Carolina beaches does not meet the "About this Report" standard on Page 14.

Specific comment 78. *Chapter Society, page 47, Second paragraph, first sentence*

Comment: Enough with the pessimism.

Recommendation: Change the sentence to read "A changing climate may mean reduced opportunities for some activities and increased opportunities for many other of the activities that Americans hold dear."

Specific comment 79. *Chapter Society, page 47, Second paragraph, second sentence, "…coral reefs are already severely compromised."*

Comment: Warming SSTs along the U.S. Gulf and Atlantic shores should encourage coral reefs to expand northward. In fact, evidence of northerly range expansion of elkhorn and staghorn has recently been reported (Precht, W.F., and R.B. Aronson, 2004. Climate flickers and range shifts of reef corals. *Frontiers in Ecology and the Environment*, **2**, 307-314). Currently, the southern portions of Florida define climatologically the northernmost portion of the coral habitat in the western Atlantic, a warming climate presents the opportunity for a habitat expansion that could bring corals further northward and closer to the U.S. mainland. Since coral reefs represent a major tourist destination, not only would a northward range expansion be a benefit to the corals themselves, but may well also represent enhanced economic opportunities along the southeastern U.S. coast.

Recommendation: Update the paragraph on the changing patterns of recreational activities to include the likelihood that coral reefs will expand northward into U.S. coastal waters and increase recreational opportunities associated with them. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 80. *Chapter Society, page 47, Third paragraph, first sentence, "Examples of economic impacts include…"*

Comment: While skiing opportunities may decline in some areas of the Northeast, many of the regions ski areas are transforming themselves into "four seasons" resorts – offering activities year round to offset some of the lost revenue from the ski seasons (for example, http://www.completenewengland.com/index.php/2008/05/17/the-survival-of-the-new-england-ski-resort-species/)

Recommendation: Update this sentence to indicate that ski resorts are adapting to the changing conditions by expanding the activities that they offer. As it now stands, the paragraph fails to meet the authors' claim of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 81. *Chapter Society, page 47, Third paragraph, first sentence, "Examples of economic impacts include…"*

Comment: Again, the citation (number 7) is to the Union of Concerned Scientists report. They have a stated political agenda on climate change!

Recommendation: Either remove the reliance on the UCS report, or redo the "About this Report" section to indicate that you are relying on information from advocacy organizations with an openly-stated political agenda. Otherwise you are grossly misleading the reader.

Specific comment 82. *Chapter Society, page 47, Third paragraph, last two sentences*

Comment: Another pessimistic scenario on climate change's impact of recreational activities and the economy dependent on them, this time on the shores of North Carolina—a coastline that is booming. And again, the citation is to a non-peer reviewed report.

Recommendation: Remove these sentences and reference.

Specific comment 83. *Chapter Society, page 47, Box 'Spotlight on Skiing in the West'*

Comment: Another pessimistic scenario on climate change's impact of recreational activities and the economy dependent on them, this time on the ski industry in the West. And again, the citation is to a non-peer reviewed report.

Recommendation: Remove this box and reference.

Specific comment 84. *Chapter Society, page 48, first paragraph, third sentence*

Comment: This sentence contains a long list of negative things associated with city living. Based on your statement that "80 percent of the U.S. population resides in urban areas" don't you think that there must be some positive aspects?

Recommendation: Include a list of the benefits of urban living to contrast with the list of negatives. In fact, it should be made obvious that the positives far outweigh the negatives, or why else would 80% of our population reside there? As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 85. *Chapter Society, page 48, first paragraph, last sentence, "Climate-related changes including increased heat, air pollution…"*

Comment: Air quality has been increasing and air pollution emissions have been decreasing across the United States for the past 20-30 years (U.S. EPA, http://www.epa.gov/airtrends/sixpoll.html).

Recommendation: Remove "air pollution" from the sentence. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 86. *Chapter Society, page 48, second paragraph*

Comment: The urbanization changes of the local climate that are described are as large (or larger) than the projected climate changes. And yet catastrophe has not been observed.

Delete this entire paragraph. Urban warming is completely independent of anthropogenic greenhouse-gas induced warming. If cities are bellwethers of climate impacts, then why, for example, are heat-related mortality rates declining in cities? (See, for example, Davis, R.E., P.C. Knappenberger, W.M. Novicoff and P.J. Michaels (2003b). Decadal changes in summer mortality in U.S. cities. *International Journal of Biometeorology*, **47**, 166–175). Why isn't there evidence of a massive exodus to rural areas? How can you relate "drought" to urbanization? Drought is related to a lack of rainfall, and urbanization has nothing to do with that (in fact, some recent research in and around Atlanta indicates that the presence of urban areas actually increase rainfall; see, for example, *Atmospheric Environment*, Volume 34, Number 3, February 2000 , pp. 507-516.)

Recommendation: Make it clear to the reader that the urbanization changes to the local thermal climate are as large, or larger, than any projected climate changes. And yet cities have adapted and thrive—clear evidence that they are able to deal with warmer temperatures. As it now stands, the statement fails to meet the authors' claim of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 87. *Chapter Society, page 48, third paragraph*

Comment: More doom and gloom talk about city life and climate change—despite the fact that cities have experienced urban temperature effects similar to projected climate changes and yet they continue to grow and thrive.

Recommendation: Make it clear to the reader that the urbanization changes to the local thermal climate can be similar to projected climate changes. And yet cities

have adapted and thrive—clear evidence that they are able to deal with warmer temperatures. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 88. *Chapter Society, page 48, fourth paragraph, second sentence*

Comment: While heat waves will likely increase in intensity and frequency in urban environments (a combination of climate change and urbanization), it has been demonstrated in the literature that when population changes are taken into account, the sensitivity of the population of America's major cities to extreme high temperatures has been significantly declining over the past several decades as we have become better adapted to such conditions (Davis et al., 2003, *International Journal of Biometeorology*; Davis et al., 2003, *Environmental Health Perspectives*). Obviously, a growing and aging population means a higher absolute number of deaths, but absolute deaths is a grossly misleading number.

Recommendation: Remove this misleading sentence entirely. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 89. *Chapter Society, page 48, fourth paragraph, last two sentences*

Comment: How can you talk about Chicago in 1995 and not Chicago in 1999? As has been published in the scientific literature (and undoubtedly known to the CCSP authors), a heat wave of nearly similar intensity struck Chicago during the summer of 1999 and the mortality was much less than in 1995 because the city was better prepared (Palecki, M.A., S.A. Changnon, and K.E. Kunkel, 2001. The nature and impacts of the July 1999 heat wave in the midwestern United States: Learning from the lessons of 1995, *Bulletin of the American Meteorological Society*, **82**, 1353–1367.). Chicago didn't redesign its city between 1995 and 1999, but it did redesign its response measures to heat waves. And it was quite successful. Clearly, if heat wave intensity and/or frequency increase in the future, cities will better prepare for them.

Recommendation: Remove these misleading sentences entirely. And replace them with the example of Chicago's rapid improvement in its response to heat waves and how such adaptive measures will take place in the future and better prepare us for increasing heat waves and likely lower the population's sensitivity to them. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 90. *Chapter Society, page 48, fifth paragraph, first sentence, "…will cause greater property damage…"*

Comment: Somehow you forgot to include the part about more damages occurring because there is going to be more things to damage in the future.

With reference to the last sentence on the page, Is the frequency of rainfall increasing (as you state here) or is drought increasing? You can't have it both ways. Why don't you decide on precisely how you think U.S. climate is really going to change, then decide on which sets of disasters are therefore imminent?

Recommendation: Add a phrase to include increased wealth in harm's way. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 91. *Chapter Society, page 48, Figure caption, "Arizona's heat-related deaths are 13 times the national average"*

Comment: This number is seemingly out of the blue. True, the claim is made in citation 15 (Baker et al., 2002) but it is neither backed by an analysis (mortality was not tabulated by Baker et al. 2002) nor a reference. Baker et al. state that "Hot days take a toll on human life, resulting in about 30 heat-related deaths per year in Arizona, about 13 times the national rate." The CDC lists the annual U.S. average heat-related mortality (from 1999-2003) to be 688 deaths per year (see http://www.cdc.gov/mmwR/preview/mmwrhtml/mm5529a2.htm). The U.S. Census lists the population of Arizona to be about 6 million and the population of the U.S. to be about 300 million (see http://quickfacts.census.gov/qfd/states/04000.html). A simple calculation shows that the heat-related mortality in Arizona to be about 5 deaths per million people (30/6 million), while that of the U.S. to be about 2.3 deaths per million (688/300 million). So Arizona's annual heat-related mortality is only about twice the U.S. average, not 13 times.

This note that heat-related deaths in Arizona are 13 times the national average is surprising (and by surprising, I mean wrong). My research shows that there are no excess heat-related deaths in Phoenix owing to the population's adaptation to the heat. (Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff (2003), Changing Heat-related Mortality in the United States. *Environmental Health Perspectives*, **111**, 1712-1718 (doi:10.1289/ehp.6336)). You are much more likely to see human mortality impacts in places where heat is less common, such as the Midwest or Northeast (see Kalkstein, L.S. and R.E. Davis (1989). Weather and Human Mortality: An Evaluation of Demographic and Inter-regional Responses in the United States. *Annals of the Association of American Geographers*, **79**, 44–64). Delete the existing caption and replace it with "Even though the average number of hours per summer day in Phoenix

that the Heat Index was over 100°F has doubled over the past 50 years, there is no evidence of increasing heat-related mortality, primarily because the populace has adapted to these uncomfortable conditions."

Recommendation: Change the caption accordingly. The citation is clearly in error. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Comment: What is the basis for this sentence? Is there a source for this statement or is it wild speculation? Is there documentation of cities shifting funds from "social programs to cope with necessary emergency responses to extreme weather" to meet the "rising expenses to city health systems to cope with increased summer hospital admissions due to excessive heat"? It seems that just the opposite likely occurs. Cities spend more on social programs to be better prepared for extreme weather such as heat waves so to avoid more hospital admissions.

Recommendation: Either provide a citation or remove this sentence.

Specific comment 93*. Chapter Society , Page 50, top chart and second paragraph.*

Comment: You cite Changnon et al. in the text (ref 22) simply saying that insurers provide data on losses from extreme events, and then you show an illustration from a GAO report that does NOT adjust for inflation!

Recommendation: Remove the figure and substitute an analogous one from Changnon that is adjusted for inflation and you will see no major trend. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 94*. Chapter Society, page 50, second paragraph, second-to-last sentence, "About half of all economic losses in the United States are insured; these are shown on the accompanying chart.*

Comment: What is the purpose of this sentence? This assessment report is on climate change, not insurance losses. It merely serves as a cheap excuse to show the trend in insured losses from catastrophes which is intended to mislead. Yes it shows damages are increasing, but it makes no mention that the reason that they are increasing is that the underlying wealth subject to damage has increased. The implication of the figure is that extreme weather events causing damage are increasing, when in fact, when the data are properly adjusted for wealth changes, no such trend is present.

Recommendation: Remove the sentence and the figure. It is misleading unless further clarified. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 95*. Chapter Society, Page 50, bottom figure, "Lightning-related insurance claims increase sharply with temperature" Citation 27a*

Comment: What is the point of this elementary exercise? Of course lightning increases with temperature—it largely comes from convective storms which arise from summer heating. The illustration appears to show that lightning-related insurance claims asymptotically increase at about 75ºF…thus there appears to be no increased risk above 75ºF, so why worry about climate change when most parts of the country exceed 75ºF routinely in the summer?

Recommendation: Remove this figure. It is confusing. Replace it with a figure showing population/wealth adjusted lightning damages with time.

Specific comment 95*. Chapter Society, Page 51, second paragraph, first sentence, "Weather-related losses are increasing much faster than population, inflation, and insurance penetration." Citation 27*

Comment: Citation 27 is to Mills, *Science*, 2005. The Mills paper has come under intensive criticism, largely from Dr. Roger Pielke Jr—another expert in the field of disaster losses. Pielke Jr. published a response to the Mills paper in *Science*.

It is inconceivable that the authors of the CCSP report don't know of the Pielke Jr. criticisms (after all Evan Mills is listed as a CCSP author) and yet it is equally inconceivable that knowing of them, that absolutely no reference is made to them.

The fact is, is that Pielke Jr. concluded "Presently, there is simply no scientific basis for claims that the escalating cost of disasters is the result of anything other than increasing societal vulnerability." (Pielke Jr., *Science*, 2005). This is precisely the *opposite* of the CCSP conclusion. How can such criticism be completely ignored?

Recommendation: Remove this sentence entirely. It is ill-founded and ill-justified. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 97*. Chapter Society, Page 51, second paragraph, second sentence, "Damages from U.S. storms…" Citation 27*

Comment: There is a mixture of U.S. and world figures in this sentence describing weather-related losses. Citation 20 is to a Swiss Re publication and it's reference to flood losses applied to *global* totals, while the first half of the sentence refers to U.S. damages. This is a completely apples and oranges comparison. The appropriate reference to U.S. flood damages is Downton et al. (Downton, W.M., Miller, J.Z.B., Pielke Jr., R.A., 2005. Renalysis of U.S. National Weather Service Flood Loss Database. *Natural Hazards Review*, **6**, 13-22), which shows in fact that flood damages, when properly adjusted for increases in U.S. wealth, show no trend (a slight, significantly insignificant downward trend, to be exact).

Recommendation: Remove this misleading sentence and replace it with one which accurately reflects the appropriate science. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 98. *Page 51, paragraph 5.*

Comment: A sentence needs to be added to the effect that "premiums paid to the National Flood Insurance Program have been insufficient to cover losses." It's that simple. If people want to live in places where hurricanes can destroy their homes, then they need to pay appropriate insurance costs. No…instead we give out money to rebuild in the same vulnerable place! All of that is well and good, as long as premiums are high enough to permit this. Society does NOT owe everyone whose beach house is inundated. Insurance companies should do this, at a high premium.

Specific Comments on "Human Health" Chapter

Specific comment 99. *Chapter Human Health, Page 52, first bullet*

Comment: If someone is projecting increases in human heat-related mortality, then they are making bad predictions. Observed trends show that heat-related mortality is declining across the major cities of the U.S. once population changes are taken into effect heat (Davis et al., 2003, *International Journal of Biometeorology*; Davis et al., 2003, *Environmental Health Perspectives*). True, as the population increases and the elderly portion of the population increases, more people will die, but this is because of population changes, not climate changes.

The current, observed trend is toward fewer heat-related deaths despite increasing temperature and humidity, and cold weather is hardly related at all to mortality rates. Few people actually believe that the U.S. death rate will increase in the future because of "global warming," especially in light of the long-term trend toward fewer heat-related deaths and greater life expectancy. This report would be better served to simply admit to the established trends and focus more on impacts that are much more likely.

References:

Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff (2004). Seasonality of Climate-human Mortality Relationships in US Cities and Impacts of Climate Change. *Climate Research,* **26**, 61–76.

Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff (2003a). Changing Heat-related Mortality in the United States. *Environmental Health Perspectives,* **111**, 1712-1718 (doi:10.1289/ehp.6336).

Davis, R.E., P.C. Knappenberger, W.M. Novicoff and P.J. Michaels (2003b). Decadal changes in summer mortality in U.S. cities. *International Journal of Biometeorology,* **47**, 166–175.

Davis, R.E., P.C. Knappenberger, W.M. Novicoff, and P.J. Michaels (2002). Decadal Changes in Heat-related Human Mortality in the Eastern United States, *Climate Research,* **22**, 175–184.

Recommendation: Remove this bullet as it relies on poor science and ignores established and proven trends. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 100. *Chapter Human Health, Page 52, second bullet*

Comment: Reduced air quality? The EPA clearly demonstrates that air quality has been improving across the country as air pollution emissions have declined (U.S. EPA, http://www.epa.gov/airtrends/sixpoll.html). Is there some reason to think that a changing climate is going to decrease air quality to a greater degree than direct emissions declines will improve it? Not according to your own cited sources (likely Huang et al., 2008, although this reference is not available for me to confirm this directly). The only decreases in air quality in future climate change projections assume that air pollution emissions will increase—something that is not happening and has not been happening for more than 30 years. Why the pessimism for the future? Is the Clean Air Act expected to be overturned?

Recommendation: Remove this bullet as it relies on poor science and ignores established and proven trends. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 101. *Chapter Human Health, Page 53, first paragraph*

Comment: If a warmer climate is so bad, then why are more and more Americans moving southward into Florida, the Gulf Coast, Texas, the desert Southwest and southern California as demonstrated by your map on page 44? Doesn't this movement provide a direct test of your warming climate hypothesis? Have all these people suffered the innumerable woes from the "multiple pathways that lead to harmful exposures"? If so, please provide the documentation whereby America's moving into warmer climates have suffered more than those who did not make such a move. If no such documentation exists, then remove this paragraph.

Recommendation: Remove this paragraph or provide documentation as to how Americans in the countries warmer regions are less healthy than those in the colder regions. As it now stands, the paragraph fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 102. *Chapter Human Health, Page 53, second paragraph, first sentence*

Comment: This has nothing to do with climate change, but instead to global transportation systems and mobility.

Recommendation: Remove this statement as it is immaterial.

Specific comment 103. *Chapter Human Health, Page 54, pull-quote at top of page*

Comment: This statement is unsubstantiated by proper analysis and is misleading.

Recommendation: Remove this statement. It is misleading. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 104. *Chapter Human Health, Page 54, first paragraph, last two sentences*

Comment: You are describing population changes (and health changes) here. These are NOT related to climate change. Sure, more people may die on extremely hot days because there are more people at risk, but, as demonstrated (Davis et al., 2003,

International Journal of Biometeorology; Davis et al., 2003, *Environmental Health Perspectives*) the sensitivity of a standardized population to extreme heat is declining. Thus, if more people die in the future, current trends would suggest that they are doing so not because of climate change, but as a result of population changes. Not to recognize this fact, and make it clear to the readers is simply being either uninformed and/or dishonest.

Recommendation: Make this point clear! As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 105. *Chapter Human Health, Page 54, third paragraph*

Comment: The reference cited for this paragraph (citation 3 = Kalkstein et al., 2008, BAMS) produces no such analysis as described in this paragraph. Instead it (inadvisably and using poor methodology) attempts to estimate what the mortality would be like in 5 US cities if they experienced a heat wave of the character that hit Europe in 2003. Kalkstien et al. (2008) did not project future mortality in 21 cites from some global warming scenario.

Recommendation: Remove this paragraph. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 105a. *Chapter Human Health, Page 55, last line of "Adaptation Strategies"*

While there is value in Philadelphia's system for responding to heat emergencies, they are taking too much credit for the actual number of lives saved. In Philadelphia, there had been a significant downward trend in heat-related mortality prior to the installation of their warning system in the mid-1990s, and this estimate of 177 lives does NOT take into account this trend (see Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff (2003a), Changing Heat-related Mortality in the United States. *Environmental Health Perspectives,* **111**, 1712-1718 (doi:10.1289/ehp.6336). In other words, there would be fewer deaths even if the system was not implemented, and these lives were not subtracted from the baseline.

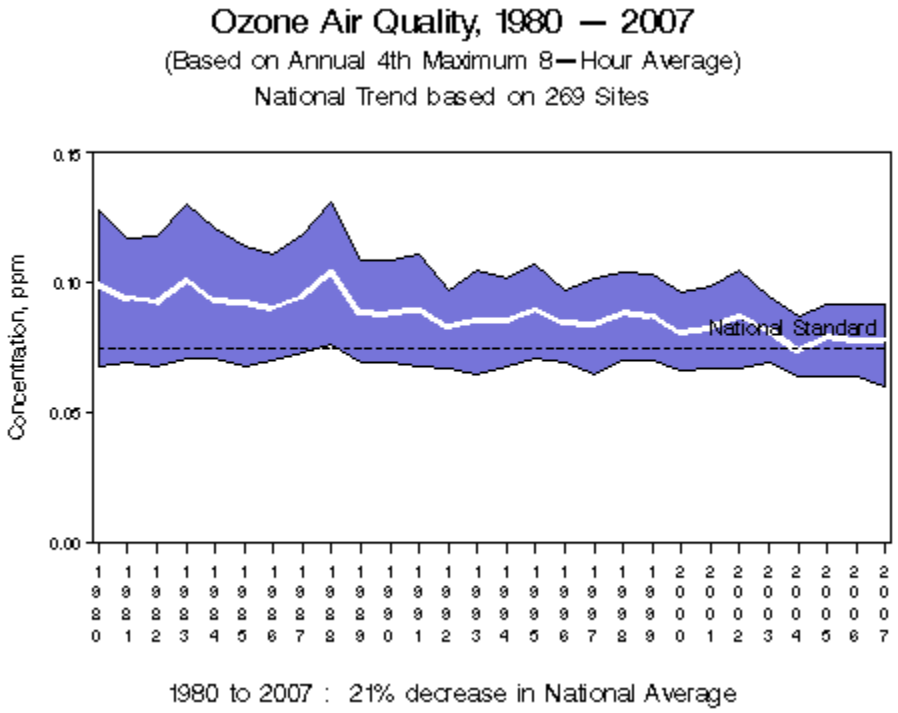
Specific comment 105b. *Chapter Human Health, Page 55, last two paragraphs.*

It is often argued that the deaths saved in warmer winters will partially to completely counteract the additional deaths during summer heat waves. This is a specious argument (that is employed in reference 5 in this section). I wrote an extensive paper on the seasonality of mortality in the United States in which I attempt to clarify this issue (Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff (2004), Seasonality

of Climate-human Mortality Relationships in US Cities and Impacts of Climate Change. *Climate Research*, **26**, 61–76). However, recent clinical research suggests that there actually is a link between influenza and temperature (cold and dry conditions increase transmission rates in guinea pigs) but to my knowledge this has not been demonstrated in humans based on ecological data. (Lowen AC, Mubareka S, Steel J, Palese P., Influenza virus transmission is dependent on relative humidity and temperature, *PLoS Pathog*. 2007 Oct 19;3(10):1470-6.)

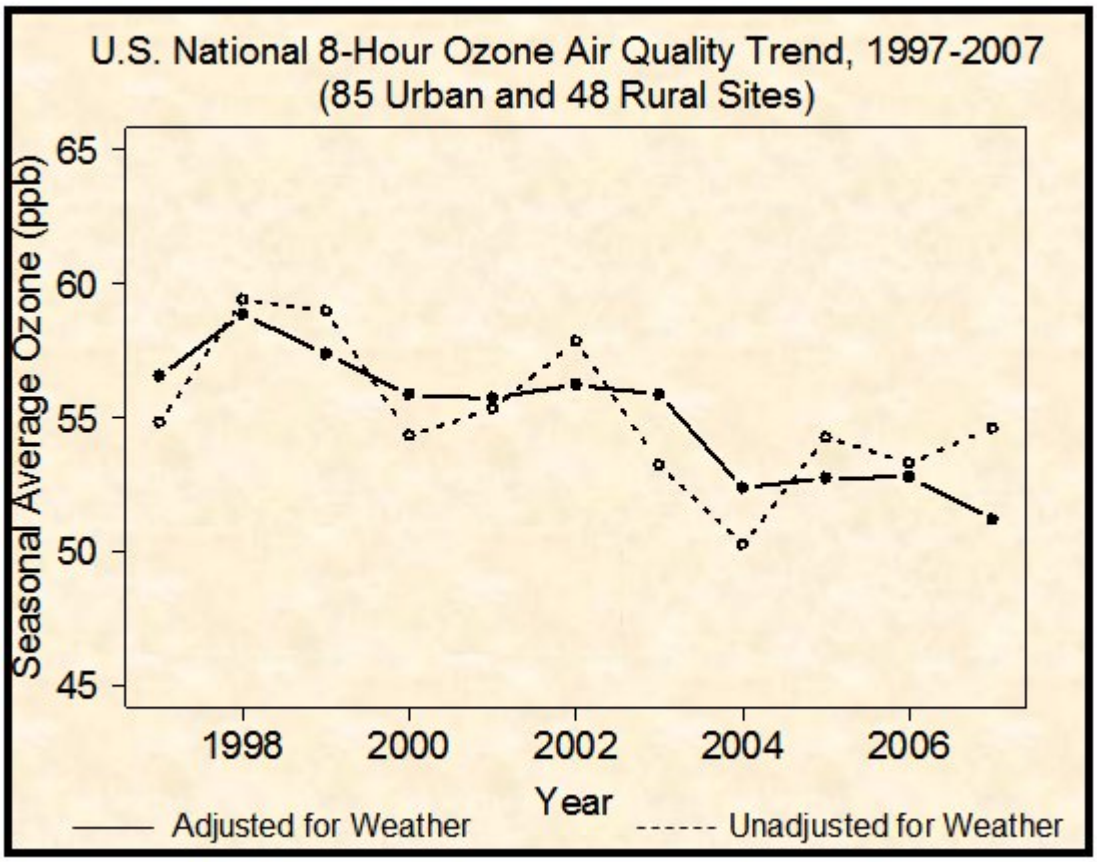
Specific comment 106. *Chapter Human Health, Page 56, first two paragraphs*

Comment: How can you have this two-paragraph discussion on the woes of ground level ozone and never mention that the trends on ground-level ozone concentrations are decreasing (U.S. EPA, http://www.epa.gov/airtrends/ozone.html)?! Why are you all being so elusive and unwilling to present relevant facts? It seems as if this report is more interested in pushing an agenda than it is in presenting the reader with an accurate review of the available scientific data.



Trends in ozone air quality (source US EPA, U.S. EPA, http://www.epa.gov/airtrends/ozone.html)

or if you perfer,



U.S. National 8-hour ozone air quality trend, 1997-2007, with and without adjustments for weather (source: US EPA, http://www.epa.gov/airtrends/weather.html)

Earlier, the CCSP argued that precipitation will increase. Given that ground-level ozone is lowest on cloudy, rainy days, your internal argument is inconsistent. Also, increasing surface temperatures relative to temperatures aloft will decrease static stability, so we should expect fewer stagnation episodes. However, higher temperatures may increase natural VOC emissions and ozone reaction rates.

Recommendation: Tell the full truth and include a discussion about improving ozone air quality. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 107. *Chapter Human Health, Page 56, fifth paragraph*

Comment: Again, there is no discussion about how observed trends may help you make a determination as to which of the two scenarios is likely to be most correct (hint: the low emissions one).

Recommendation: Add a discussion about observed ozone trends. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 108. *Chapter Human Health, Page 58, first paragraph, first sentence, "Injury, illness, and death are projected to increase as the number and intensity of extreme weather events rises."*

Comment: I don't understand this projection. Are you taking into account population changes or not? If not, then the statement is a reflection of population and not weather. If population changes are accounted for, then if seems as if you are turning the clock back many, many years and suggesting that instead of improving technologies saving lives—such as doppler radar, heat watch/warning systems, improving technologies employed by the National Hurricane Center, air pollution awareness systems, etc.—will start to become ineffective and the long established trends towards fewer people dying from extreme weather events (in some cases even despite rising population numbers) will turn around.

Recommendation: Clarify whether the projected increase in injury, illness and death takes into account a growing population. Also explain how the agencies such as the NWS and the NHC will begin to fail in one of their main purposes—to help protect lives. As it now stands, the statement fails to meet the authors' claim of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 109. *Chapter Human Health, Page 58, second paragraph, first sentence, "Over 2,000 Americans were killed in the 2005 hurricane season, more than double the average number of lives lost to hurricanes in the U.S. over the previous 65 years."*

Comment: I don't understand this sentence. More than "double the average number of lives lost…over the previous 65 years"? The U.S. doesn't average 1,000 deaths from hurricanes a year. Do you mean double the TOTAL number of lives lost to hurricanes in the past 65 years? If so, then things are still off. According to the NHC, there were 2,464 lives lost to hurricanes in the U.S. in the 65-year period from 1940-2004 (http://www.nhc.noaa.gov/Deadliest_Costliest.shtml?). This averages to 38 deaths per year. The number of lives lost to Katrina is likely at least several hundred less than the total of the previous 65 years. If you want to keep the sum under 1,000 deaths, then you need to go back only 35 years—the time since Camille.

Recommendation: Clarify exactly what you really mean to say here and double check your hurricane death numbers. This is very sloppy. As it now stands, the

statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 110. *Chapter Human Health, Page 58, third paragraph, second sentence, "In the U.S., the amount of precipitation falling…"*

Comment: We discussed the discrepancy between these numbers and our findings in a previous comment.

Recommendation: Rectify these numbers with our findings (Michaels, et al., 2004, Trends in precipitation on the wettest days of the year across the contiguous USA, *International Journal of Climatology*, 24, 1873-1882.). It is misleading unless further clarified. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 111. *Chapter Human Health, Page 59, Box "Spotlight on West Nile Virus*

Comment: Come on. This is truly bad. How can you blame climate change for the spread of West Nile Virus from New York City to the four corners of the country in just 5 years? What is the range of climate that it now resides in? Exactly how much anthropogenic climate change occurred over that 5-year period? I seem to recall swatting plenty of mosquitoes prior to 1999. And New York City is hardly the warmest place in the U.S. It seems odd that climate change is causing a disease to spread southward.

The vector for West Nile is mosquitoes and wherever there is a suitable host mosquito population, an outpost for West Nile virus can be established. And it is not just one mosquito species that is involved. Instead, the disease has been isolated in over 40 mosquito species found throughout the United States. So the simplistic argument that climate change is allowing a West Nile carrying mosquito species to move across the United States is simply wrong. The already-resident mosquito populations—existing across the huge range of climates in the U.S.--are appropriate hosts for the West Nile virus. Once it was introduced, it rapidly took hold—*because of the existing mosquito populations, not climate change*.

Recommendation: Remove this entire box. It is wrong and embarrassing. As it now stands, this information fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 112. *Chapter Human Health, Page 60, The entire page*

Comment: You have got to be kidding me. This is a Chapter on Human Health and instead of focusing on the benefits of rising levels of carbon dioxide on food crop production (better yield, better water use efficiency, better pollution tolerance, and on and on), you instead decide to focus on CO2's benefits on ragweed and poison ivy! There is not a single reference to anything positive arising from plants growing better. This single page epitomizes the problems of this entire report. There is not the slightest notion of trying to present to true state of scientific knowledge. Instead the authors seem set on dreaming up a litany of pessimistic projections and negative impacts. The report is an insult to everyone who reads it.

Recommendation: Replace this page with one discussing the positive effects of CO2 on plants and how they interact positively with humans. It is not really that hard to find examples from the monumental literature on the subject. In fact, it is much harder to find the few negative impacts to cherry pick. As it now stands, the statement fails to meet the authors' claim of providing the "best available science" (p. 14) and of conveying "the most relevant" information possible (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 113. *Page 74, paragraph 4. "….as global warming and land subsidence combine to produce relative sea-level rise in the range of four feet"*

Comment: The midrange emissions scenario sea-level rise in the IPCC AR4 is 8-19 inches, with a mean of 13.5. One presumes CCSP is using this median, too. (If not, they need to say which emissions scenario they are using). At any rate, this leaves slightly less than 3 feet as a result of subsidence. So, barring ANY climate change it would appear that the roads/railroads would go under water about 25 years later simply because of local geology. Ivins, in *Geophysical Research Letters* calculated subsidence on the central Gulf Coast at between .04 and 0.3 inches per *year*.

Recommendation: You need to note that this is and WILL BE the major cause of sea-level rise on the Gulf Coast in the next 100 years, and that adaptation to subsidence is the same as adaptation to climate-changed induce rising sea-levels. Modify Key Finding 5 to indicate that there is a large component of sea –level rise on the Atlantic and Gulf Coasts that is a result of geological, not climatic, processes, and that these will continue. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 114*. Page 75, Box, "Regional Spotlight: New York" "With potential sea-level rise estimated under business as usual emissions to be up to 3.5 feet by 2080…."*

Comment: In the IPCC AR4, the greatest projected rise is in A1F1 ("fossil intensive") scenario is slightly less than two feet.

Recommendation: CCSP needs to state here why their estimate of maximum rise is so much higher than IPCC's (or, rather, what specifically is wrong with the IPCC's estimates and how did CCSP decide to use a different figure). As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 115. *Page 76-77, fourth and first paragraph.*

Comment: No where here is it stated that the vast majority of the overwash is a result of land subsidence. This is extremely and purposefully misleading the reader.

Recommendation: Please correct it this misleading statement. *And again modify Key Finding 5.* As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 116. *Page 79, first paragraph. "water levels in the St. Lawrence Seaway were at their lowest point in 35 years."*

Comment: There's no trend at all in the level of Lake Ontario from the beginning of the Canadian record in 1918 (http://www.waterlevels.gc.ca/C&A/netgraphs_e.html).

Recommendation: Please note this in the text.

Specific comment 117. *Page 79, second paragraph. "If low water levels become more common because of drier conditions…"*

Comment: See my "General Comment on Temperature and Precipitation Maps for the U.S.". There, I requested a graphic showing the changes projected by each of the 15 or 16 models. Then this sentence can be corrected to say "If low water levels….conditions *predicted by xx% of our models*," then there is something to this statement.

Specific comment 118. *Page 79 Citation 19*

Comment: Citation 19 is "Tom Karl and D. Anderson: Emerging issues in abrupt climate change, Internal Briefing." Personally, I've never heard of the refereed journal "Internal Briefing", but maybe that meets CCSP's standards given on page 14.

Recommendation: Suggest that, in an attempt to maintain credibility, that references like this (and the text they support) be removed.

Specific comment 119*. Last paragraph, page 79.*

Comment: What's missing here is the fact that, on average, the U.S. is becoming wetter as precipitation minus potential evapotranspiration shows a significant increase as both the U.S. and the planet warmed.

Recommendation: This needs to be mentioned here—in other words that what IS happening is going to have to reverse itself. *Add to Key Finding 1.2 that, as a whole, the U.S. has become wetter*. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 120. *Page 81, Sidebar, "Hurricane Katrina was the most destructive and costliest natural disaster in U.S. History, claiming more than 1800 lives and causing an estimated 134$ billion in damage."*

Comment: Hmm…the Galveston Hurricane in 1900 killed 8000, and according to Pielke Jr. et al., (2008, *Natural Hazards Review*), and the costliest storm was the 1926 Great Miami Hurricane, at 140-157$ billion in normalized damage. Pielke Jr has Katrina at 81$ billion. In any case, Katrina was neither the deadliest nor the costliest.

Recommendation: Please make the text accurate here. What really surprises me about this one is that many of the CCSP people *must* know of the 1926 hurricane and 1900 Galveston. How does stuff like this get through? As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 121*. Page 82, second paragraph.*

Comment: This is the typical metric of this report. "Something good might happen, but there is also something bad to cancel the good". In this case you correctly state that the Arctic Passage will be navigable, and then follow that with a statement to the effect that because ice varies from year to year, shippers will have "higher costs".

If the cost to ship in the Arctic is greater than simply staying out, shippers will stay away. Or do you think they are just stupid?

Recommendation: Change the sentence to something like—"variable conditions in coming decades make it difficult to predict when this will become cost-effective".

Specific comment 122. *Page 85, picture.*

Comment: UNLESS you can demonstrate that the suite of models used are generating a statistically-significant change in rainfall (less in the west and more in the east), this picture must be removed. See comment above on presenting the entire suite of the models. I do not have a clue whether or not the model average differs significantly from zero, but you need to demonstrate that it does before you can use this picture.

Recommendation: Remove picture unless it can be justified.

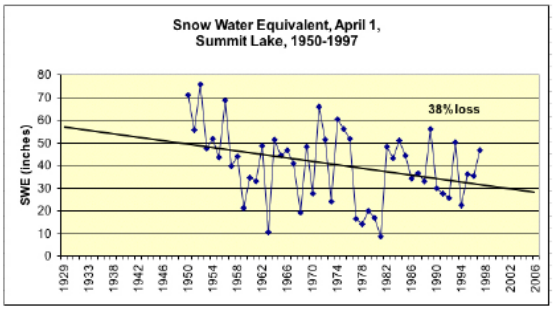
Specific comment 123. *Page 87, top-left table.*

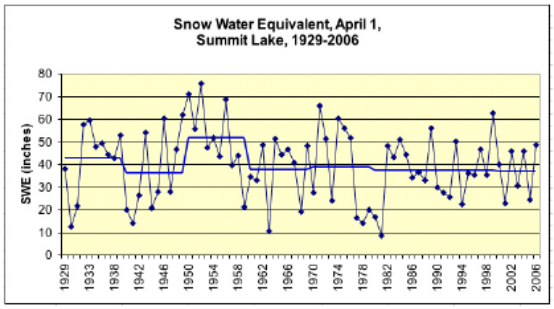
Comment: EVERYTHING is bad!? Why couldn't increased precipitation and runoff result in greater capture of water (maybe we might even build a new dam or two, or raise others?). More water then becomes available for irrigated agriculture. How about more warm-water fish (Greenlanders were happy to see the return of the cod during the warming of the early 20th century)? Tables like this just reveal the profound biases in this report.

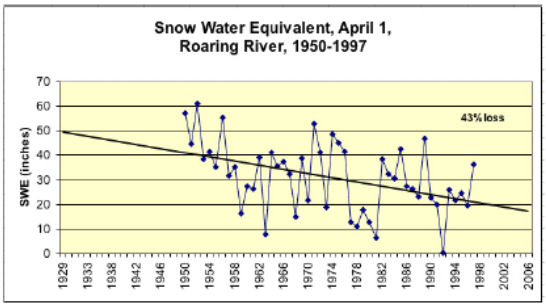
Recommendation: Changes the column heading in the table from "Impacts" to "Negative Impacts" and add an entirely new column with the heading "Positive Impacts" and populate it accordingly. You must be able to think up a few. No? If this remains as is, I suspect there will be a lot of public attack on CCSP's credibility.

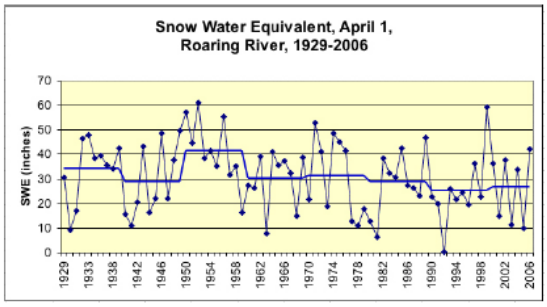
Specific comment 124. *Page 87, first paragraph. "Over the last 50 years there have been widespread temperature-related reductions in snowpack in the West, with the largest reductions occurring in lower elevation mountains in the Pacific Northwest…"*

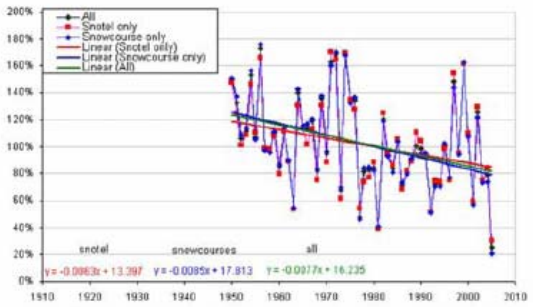
Comment: This is an old story. Starting in 1950 is starting at a high point in the snowpack. Records that go back into the early 20th century show little if any trend (Oregon Climate Service; National Water and Climate Center, USDA), as shown below:





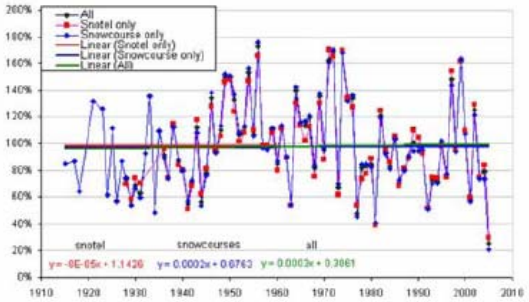






Washington-wide snowpack since 1950,
Relative to 1971-2000 normal

Snowpack in usual maximum month



Washington-wide snowpack period of record,
Relative to 1971-2000 normal

Snowpack in usual maximum month

Recommendation: I suggest you modify text to say "a more limited set of records going back to the early 20th century indicates no overall trend in snowpack, as the mid-century appears to be a high point in these histories", and that you show one (or more) of the figures supplied above.

These historical records invalidate Key Finding 6.1. Modify it to state that snowpack has returned to values that were observed in the early 20th century, before substantial emissions of heat-trapping gases.

As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 125*. Needed insert on pp 86-87.*

Comment: The reader is certainly left with the impression that most of these observations are caused by anthropogenerated climate change from greenhouse gases. The primary reference is often CCSP SAP4.3. Regarding this, it says, "Most of the studies reviewed in this chapter to not incorporate methods of trend attribution, and conclusions must be qualified to this effect".

Recommendation: Change that into something the general reader can understand and insert it somewhere in these pages.

.

Specific comment 126. *Page 96, Figures for corn and soybean optima.*

Comment: There has to be something wrong here. The reproductive optimum for corn is given as 68 degrees and the vegetative at 84. But, in fact, the vegetative state is before reproduction. The last I heard, in the central part of its range, corn grows vegetatively through roughly the first week in July, and then tassels out in early or mid July (depending upon location). If the reproductive optimum is below the vegetative optimum, then the reproductive optimum is dramatically exceeded every year. Figure was obviously labeled by someone who has never lived near a cornfield.

Recommendation: Change the figures so that they are labeled correctly. Also, what temperature does the x-axis refer to? Mean daily? Weekly? High temperature?

Specific comment 127. *Page 96, Figures for corn and soybean optima.*

Comment: I am assuming that you have labeled things wrong (see last comment), so, let's just say that the curve on the right is the reproductive optimum. Note that for corn, my straightedge has it around 84 degrees. Then (see comment 129) CCSP says that corn yields will flatten out because the optimum will be exceeded. Is this such a bad thing? Moving whatever temperature this is (see last comment) up six degrees puts the temperature at the optimum for soybeans. Hmmm….soybeans require less nitrogen fertilizer (being legumes) than corn…they tend to command about 2-3 times more per bushel in price, and the yields are about one third that of corn. Sounds like more protein (beans vs. corn) to me. CCSP on page 105 says changing species is "high-risk". Not true, it's what has been done throughout agricultural history. The soybean was virtually nonexistent in the US in the 1930s. Was the changeover of some corn land to soybeans in succeeding decades "high risk"?

Recommendation: Modify text where appropriate in this chapter (there are many places) to indicate that agriculturalists will generally adapt their practices as they have in the past. A transition from corn to soybeans in the central part of the corn belt can accommodate several degrees and a transition from soybeans to grain sorghum allows for several more. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 128. *Page 97, second paragraph. "A seemingly paradoxical impact of warming is that appears to be increasing the risk of plant frost damage".*

Comment: Wait a minute. Growing seasons are lengthening. That's the time between the last and first frosts. Further, there's no evidence that there is any change in the distribution of the relationship between temperature and last frost in the spring. CCSP is implying that this has happened—that it is warming the date of last frost has not adjusted accordingly.

Recommendation: Please cite a reference and show a graphic proving that indeed this phase shift is occurring, or, failing that, remove the entire paragraph. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 129. *Page 98, figure caption. "Yields are not expected to continue their historical upward trend as temperatures rise above the optimum for vegetative and reproductive growth".*

Comment: Funny. The range of temperatures in which corn is grown is enormous. But, more important, it is really difficult to believe that there will not be a tremendous economic incentive to genetically modify corn in fashions that will make it more water-efficient and less sensitive to very high temperatures. The relatively constant rise in yields shown in this figure has been predicted to stop for at least 40 years now by consistently-wrong soothsayers such as Lester Brown and Paul Ehrlich. By saying this, CCSP throws in with that non-credible crew. The trend continues upwards because of incentives to produce more which are satisfied by different varieties, nitrogen use, changes in tillage practices, etc…it strains the imagination to believe that other such technological improvements will suddenly stop because of global warming.

Recommendation: Remove the sentence. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 130. *Page 98, figure caption. "Increased variation in yield is likely to occur as temperatures increase and rainfall becomes more variable".*

Comment: There is NO evidence for this occurring in recent years (based upon national average yields), despite what CCSP indicates is happening on page 87, i.e., lengthened drought, longer time between rainfall events, and increased heavy rain frequency. So if there is no evidence for increased variability given what CCSP says is already happening, how can you say it will occur in the future as these things continue to happen?

Recommendation: Change text to acknowledge that the variation in yields has remained constant despite all the climate changes noted in the Report. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Selected Comments on Regional Climate Change Impacts

Northeast

Specific comment 131. *Chapter "Northeast," page 120, first paragraph, second to last sentence. "Since 1970…"*

Comment: The citation for sentence (citation 1) is (unbelievably) a reference to a report by the Union of Concerned Scientists. The UCS report is neither peer-reviewed literature nor official government data, as such, it does not fit into the description of relied-upon material provided on page 14. The UCS is an organization who issued the report in support of its efforts advocating a regulatory position on greenhouse gas emissions.

Recommendation: Either remove all reliance on the UCS report, or redo the "About this Report" section to indicate that you are relying on information from advocacy organizations with an openly-stated political agenda. Otherwise you are grossly misleading the reader.

Specific comment 132. *Chapter "Northeast," page 120, third paragraph and all bullets*

Comment: Alarmist. It is inappropriate the list only the impacts from climate model projections from the high emissions scenario. Why run the low emissions scenario at all if you aren't going to discuss it? Just so you can claim you did when asked? While you may claim that current emissions are running ahead of even the highest scenarios, you must also realize that the atmospheric concentration of CO2 is not increasing at the rate projected by the high emissions scenarios, nor is the rate of global temperature rise. Impacts are based upon climate changes, not emissions. The rate of global temperature rise, in fact, in recent decades is falling at or below the lowest rate projected by the IPCC.

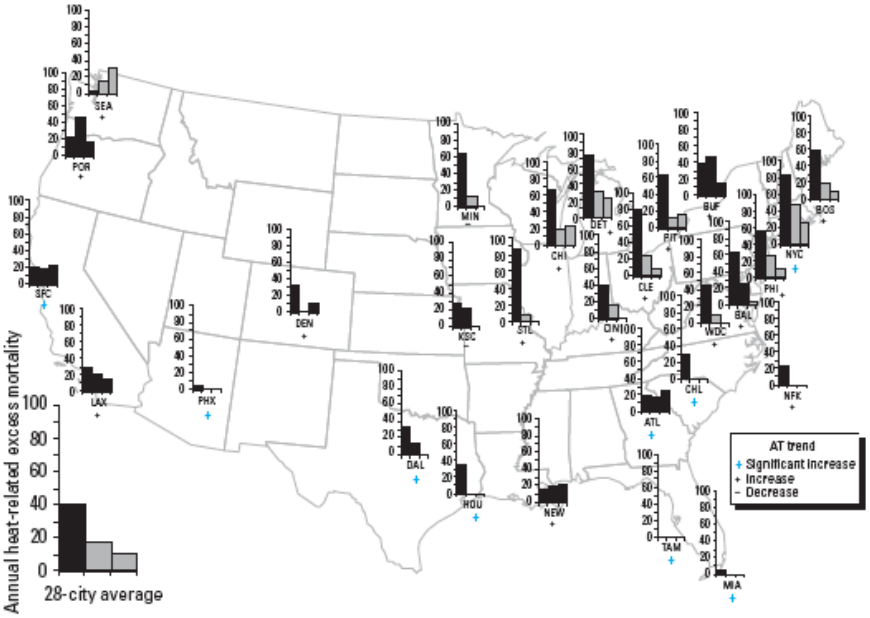
Recommendation: Add the impacts from a low emissions scenario for comparison.

Specific comment 133. *Chapter "Northeast," page 120, fourth paragraph, first sentence, "Heat waves…are projected to become commonplace…with implications for human health."*

Comment: Yes! The implications are overwhelmingly *positive*. See the series of papers by Davis et al. (Davis et al., 2003, *International Journal of Biometeorology*; Davis et al., 2003, *Environmental Health Perspectives*). Davis et al. found 1) that the sensitivity to extreme heat in the population's of cities in the Northeast has been declining over the past 3-4 decades, despite rising summertime heat, and 2) that in locations where heat waves are more common, the population is even better adapted to them and thus less

sensitive. Therefore, if heat waves become more common events in the future, the region will become much better prepared and adapted to them. This is evident from the vastly different response to heat waves in Chicago in 1995 and 1999 (Palecki, M.A., S.A. Changnon, and K.E. Kunkel, 2001. The nature and impacts of the July 1999 heat wave in the midwestern United States: Learning from the lessons of 1995, *Bulletin of the American Meteorological Society*, **82**, 1353–1367.), as well as in France in the 2003 and the 2006 heat waves (Fouillet, A., G. Rey, V. Wagner, K. Laadi, P. Empereur-Bissonet, A Le Tetre, P. Frayssinet, P. Bessemoulin, F. Laurent, P. De Crouy-Chanel, E. Jougla, and D. Hémon, 2008. Has the impact of heat waves on mortality changed in France since the European heat wave of summer 2003? A study of the 2006 heat wave. *International Journal of Epidemiology*, doi:10.1093/ije/dym253). Adaptation to heat waves takes place rapidly. Even better news for human health can be found on the first illustration on page 121 which shows where the climate of New Hampshire may take it in the future. Whether it ends up being like Washington DC or like Charlotte, NC, the outlook for the response to heat-waves is excellent because in either of these location (as is true for most cities across the southeast and southern tier of the country) there are virtually no statistically detectable heat-related deaths in recent decades.

Notice the large declines in heat-related mortality for the Northeastern cities of Boston, New York, and Philadelphia in the figure below as well as the lack of any statistically distinguishable heat-related deaths in Washington DC or Charlotte NC in the 1990s. The future definitely looks brighter for the Northeast, as far as human-response to heat waves.

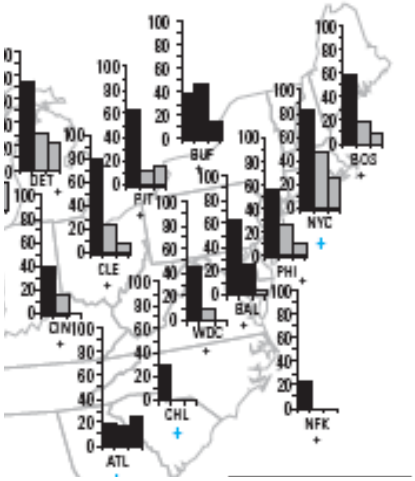


Annual average excess summer mortality due to high temperatures, broken down by decade, for 28 major cities across the United States. For each city each of the three bars represents the average mortality during successive decades (left bar 1964-66 + 1973-1979; middle bar 1980-1989, right bar 1990-1998). Bars of different color indicate a statistically significant difference. No bar at all means that no

temperature/mortality relationship could be found during that decade/city combination (taken from Davis et al., 2003, *Environmental Health Perspectives*).

Recommendation: Add the sentence, "However, research has shown that the population has adapted to these conditions in the past, and heat-related mortality has actually declined in many northeastern cities" (Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff (2003a), Changing Heat-related Mortality in the United States. *Environmental Health Perspectives,* 111, 1712-1718 (doi:10.1289/ehp.6336).

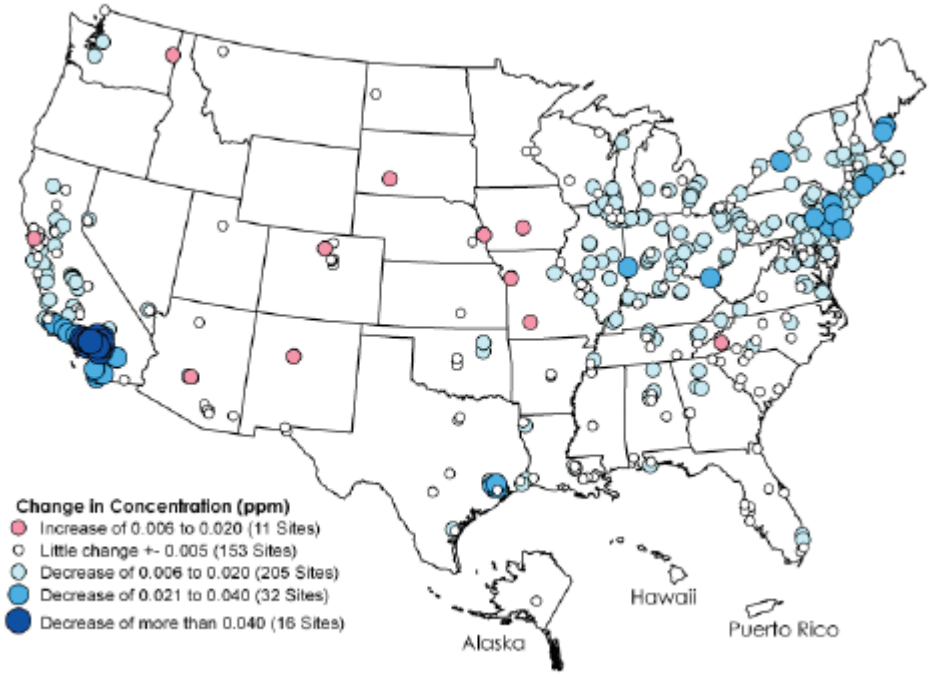
Recommendation: Emphasize the positive outlook for increasing frequency of heat waves across the Northeast and include a figure that is the close-up of the trend in heat-related mortality across the Northeast, like the example below.



Without such an update, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 134. *Chapter "Northeast," page 120, last paragraph, first sentence, "…for cities that now experience ozone pollution problems…*

Comment: Trends in ground level ozone are strongly *negative* across the Northeast for the past several decades meaning that air quality has been *improving* across the region despite rising air temperatures! These observations contrast with your pessimistic projections.



Change in ozone concentrations in ppm, 1990-1992 vs. 2004-2006 (3-year average of annual fourth highest daily maximum 8-hour concentrations) (source: http://www.epa.gov/air/airtrends/2007/report/groundlevelozone.pdf).

Recommendation: Note the positive trends in air quality across the regions which have occurred despite rising temperatures—a strong indication that climate change does not necessarily mean bad things for the region's air quality and include the map of ground level ozone trends illustrated above (easily available from the EPA). Without such an update, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

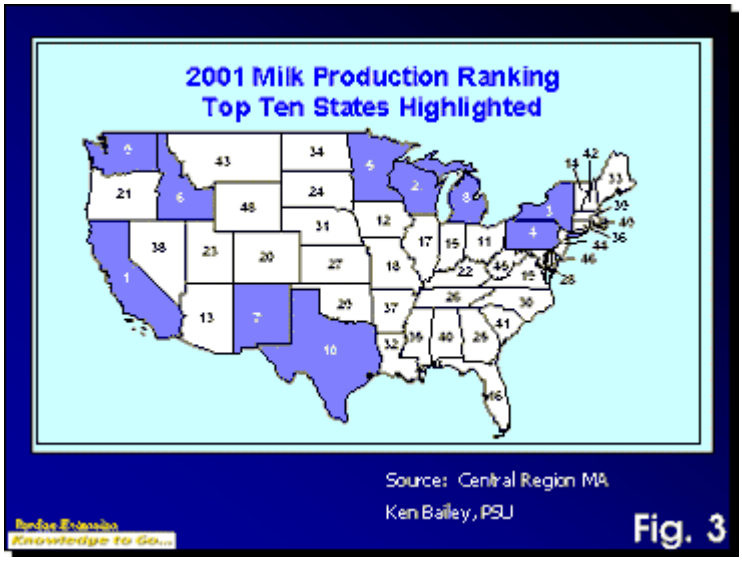
Specific comment 135. *Chapter "Northeast," page 121, first paragraph, first sentence*

Comment: As I discussed above, a warming climate should mean good things for the population's response to heat waves as more frequent heat waves will spur improved adaptive measures such as increased public awareness of potentially dangerous weather situations, and proactive responses of municipalities during extreme weather events. The text points out the relative lack of air conditioning in the New England homes, but luckily air conditioners are readily available as represent an easy adaptation to undertake.

Recommendation: Emphasize the positive aspects of a warming climate as it makes heat waves more common and thus the population will become better prepared for them. Without such an update, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 136. *Chapter "Northeast," page 121, third paragraph*

Comment: Do the projections in milk production allow for changing types of dairy cattle and farming practices? Why should dairy production in the Northeast suffer when as the map below (taken from the EPA) illustrates, milk is produced all across the U.S. including in locations with a far warmer climate than the Northeast is projected to have at the end of the 21st century under even your worst-case scenario. Texas and New Mexico rank among the top-10 milk producing states in the county. Instead of seeing their production steadily decline as the climate changes, I am sure that the dairy farmers in the Northeast will take the necessary steps to insure continued success.



Ranking of milk production by states (source: http://www.epa.gov/oecaagct/ag101/dairybackground.html)

Recommendation: Emphasize that while climate change may force dairy farmers in the Northeast to alter their production methods, they should be able to readily adapt and continue to be successful in raising milk producing cows. Without such an update, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 137. *Chapter "Northeast," page 121, fourth paragraph, first sentence, "Many current sea-level projections…"*

Comment: Not sure which "ice flow dynamics" you are talking about here. A series of recent papers (van de Wal, R.S.W., et al. 2008. Velocity changes in the ablation zone of the Greenland Ice Sheet, *Science*, 321, 111-113; Howat, I., et al., 2007. Rapid changes in ice discharge from Greenland outlet glaciers. *Science*, 315, 1559-1651; Joughin, I., et al., 2008. Seasonal speedup along the western flank of the Greenland Ice Sheet. *Science*, 320,

781-783) indicate that ice flow dynamics are not sufficient to suggest a major speed-up of glacial flow rates in Greenland.

Recommendation: Remove this sentence. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 138. *Chapter "Northeast," page 122-123, section on the ski industry*

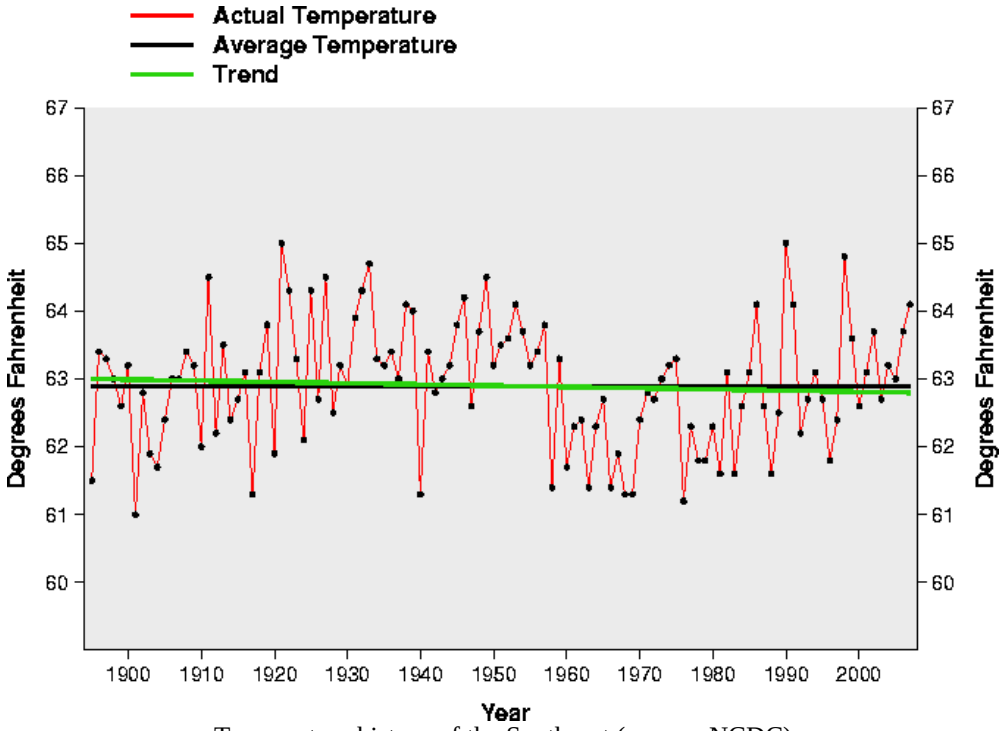
Comment: There is nothing in this section about the efforts of the ski industry to counter the potential loss of winter recreational opportunities (and lost revenue) by expanding recreational opportunities (and increasing revenue) in the other seasons. And yet such efforts are underway (for example, http://www.completenewengland.com/index.php/2008/05/17/the-survival-of-the-new-england-ski-resort-species/). There is also no description of any kind of any other such efforts to promote tourism and other recreational activities in the Northeast in the non-winter seasons, or even any hint that such a strategy, if it is not being employed now, will surely be a good one for the future. The future is not as dark as you all seem intent on making it out to be!

Recommendation: Add some comments about how the ski industry is coping now and may cope in the future. You speculate on bad impacts, what can't you speculate of some good ones?

Southeast

Specific comment 139. *Chapter Southeast, Page 124, first paragraph, second sentence, "The annual average temperature in the Southeast rose about 2ºF between 1970 and 2007…"*

Comment: You failed to mention that despite the recent warming trend, temperatures in the Southeast have yet reach the warmth that was commonplace in the 1920s through the 1950s in the region, or that there has been no long-term change in temperatures in the region for more than 100 years. All of a sudden, in this section, you switched to discussing 50 or 100 year temperature trends and instead now only discuss 30 year trends. That seems odd (or convenient).



Temperature history of the Southeast (source: NCDC)

Recommendation: Include an illustration similar to the one above and describe how temperatures were typically higher in the Southeast during the 1920s through the 1950s than they are now. As it now stands, the statement fails to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" and otherwise violates applicable objectivity requirements.

Specific comment 140. *Chapter Southeast, Page 124, third paragraph, second sentence, the list of citations (cite 3,4,5,6,7)*

Comment: Well, you certainly hit upon the hurricane literature that was all the rage a couple of years ago! But somehow you left off the long list of subsequent literature that suggest that 1) the relationship between hurricanes and SST is not as strong as suggested in those papers, and 2) that the behavior since 1975 is not particularly unusual in the overall history of Atlantic hurricanes, and 3) that the behavior of hurricanes since 1975 is likely not related to anthropogenic "global warming." The AOML has a good bibliography of relevant hurricane literature (http://www.aoml.noaa.gov/general/lib/Regional/climate_change/climatechangechrono.htm).

Here are a few papers to start with:

Briggs, W.M. 2008. On the changes in the number and intensity of North Atlantic tropical cyclones. *Journal of Climate*, **21**, 1387-1402.

Knutson, T.R., et al., 2008. Simulated reduction in Atlantic hurricane frequency under twenty-first-century warming conditions. *Nature Geosciences*, doi:10.1038/ngeo202

Wang, C., & Lee, S.K. (2008). Global warming and United States landfalling hurricanes. *Geophysical Research Letters, 35*(1), L02708.

Kossin, J.P., & Vimont, D.J. (2007). A more general framework for understanding Atlantic hurricane variability and trends. *Bulletin of the American Meteorological Society,88*(11), 1767-1781.

Landsea, C.W. (2007). Counting Atlantic tropical cyclones back to 1900. *EOS: Transactions of the American Geophysical Union, 88,*

Latif, M., Keenlyside, N., & Bader, J. (2007). Tropical sea surface temperature, vertical wind shear, and hurricane development. *Geophysical Research Letters, 34*(1), L01710.

Nyberg, J., Malmgren, B.A., Winter, A., Jury, M.R., Kilbourne, K.H., & Quinn, T.M. (2007). Low hurricane activity in the 1970s and 1980s compared to the past 270 years. *Nature, 447*(7145), 698-701.

Vecchi, G.A., & Soden, B.J. (2007). Effect of remote sea surface temperature change on potential tropical cyclone intensity. *Nature, 450*(7172), 1066-1070.

Vecchi, G.A., & Soden, B.J. (2007). Increased tropical Atlantic wind shear in model projections of global warming. *Geophysical Research Letters, 34*(8), L08702.

Klotzbach, P.J. (2006). Trends in global tropical cyclone activity over the last twenty years (1986-2005). *Geophysical Research Letters, 33*(10), L10805.

Recommendation. Bring your literature and your conclusions up-to-date. As it now stands, the statement fails miserably to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 141. *Chapter Southeast, Page 125, first paragraph, First bullet, "Widespread illness and loss of life due to increased summer heat stress [citation 9]"*

Comment: A more incorrect statement could not have been made (as we have been through this many times before in my Comments). First of all, the citation is

inappropriate. Citation 9 is given in the endnotes for the Chapter as 'World Health Organization, 2008: Protecting Health in Europe from Climate Change" it is not applicable to the Southeastern United States.

The most applicable reference should be:

Davis, R.E., P.C. Knappenberger, P.J. Michaels, and W.M. Novicoff, 2003: Changing heat-related mortality in the United States, *Environmental Health Perspectives*, 111(14), 1712-1218.

Of course, the Davis et al. reference concludes the *exact opposite* of the first bullet.

Recommendation: The bullet should be changed to read 'The southeast will be largely unimpacted by changes in the summer heat stress as this area is virtually completely adapted to high temperature events." As it now stands, the statement fails miserably to meet the authors' claim of representing the "the best available science" (p. 14) and of conveying "the most relevant and up-to-date information possible" (p. 14) and otherwise violates applicable objectivity requirements. In fact, it is an out and out fabrication.

Specific comment 142. *Chapter Southeast, Page 125, Second paragraph, first sentence*

Comment: Holy smokes, you all actually do know about the Davis et al. mortality studies! But, for some reason, the above mentioned Davis et al. reference (listed as reference 15 in the endnotes for the Chapter) is used as a citation for a sentence about cold vs. warm season deaths—a topic that is not covered by Davis et al. and thus is an inappropriate reference as cited! This is amusing, there are probably a dozen or more places in this report that the Davis et al. (2008) reference should have been relied upon, but the one place that you do use it, is incorrect. Nice work. The appropriate Davis et al. reference that shows that heat-related mortality typically exceeds cold-related mortality is:

Davis, R.E., Knappenberger, P.C., Michaels, P.J., and W. M Novicoff, 2004. Seasonality of climate-human mortality relationships in US cities and impacts if climate change. Climate Research, 26, 61-76.

Recommendation: Change citation 15 in the Southeast Chapter to the above citation.

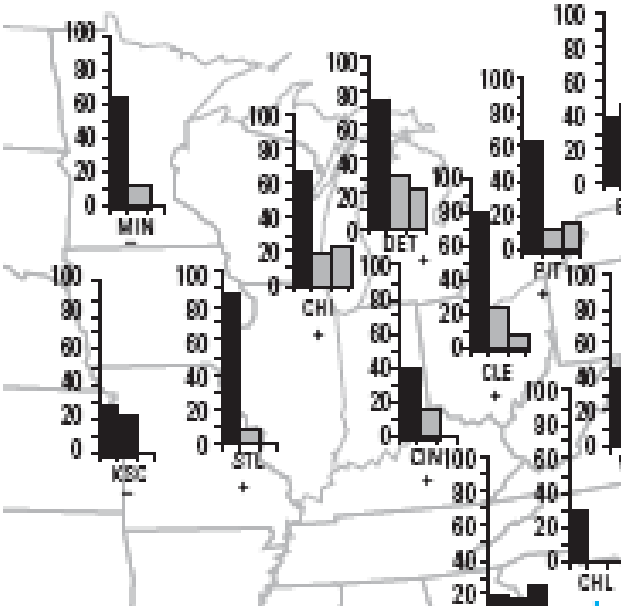
Specific comment 143. *Chapter "Midwest," page 128, second paragraph,*

Comment: Throughout this paragraph you make claims about the projected increase in heat waves across the Midwest and yet not once do you describe that the population's sensitivity to excessive heat has been declining across the region in recent decades. And

as the data on heat-related mortality in the warmer portions of the country shows, the more that high temperatures are commonplace in a region, the better adapted the population is to them, and consequently heat-related mortality rates decline.

I realize that you all are probably getting tired of me making the same point over and over, but if you all would have initially incorporated this information into your text and portrayed the situation as it really is, rather than how you'd like it to be, I wouldn't have to keep doing this.

Recommendation: Emphasize the positive outlook for increasing frequency of heat waves across the Midwest and include a figure that is the close-up of the trend in heat-related mortality across the Midwest, like the example below (I'll gladly provide you with one as I have the actual data).



Without such a modification, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 144. *Chapter "Midwest," page 128, second paragraph, last sentence, "Even more severe heat waves, such as the one that claimed tens of thousands of lives in Europe in 2003, are projected to become more frequent in a warmer world, occurring every other year in the Midwest by the end of the century under the higher emissions scenario."*

Comment: This claim is preposterous. According to the source of your figure on page 23 (Schår et al., 2004, *Nature*, doi:10.1038/nature02300), the European heat wave of 2003 was about a 46,000-yr event. I would like to see the analysis that shows that the expected return interval of a 46,000-yr event currently becomes 1 in 2 in the future.

Recommendation: Remove reference to the 2003 European heat wave. Without such an update, the statement fails to meet the authors' claim of conveying the "best available science" (p. 14) and otherwise violates applicable objectivity requirements.

Specific comment 145. *Chapter "Midwest," page 129, bottom figure*

Comment: The lake level histories (first two points for each lake) bear little resemblance to the lake level histories from the NOAA's Great Lakes Environmental Research Laboratory (http://www.glerl.noaa.gov/data/now/wlevels/levels.html). In particular, water levels in Lake Erie and Lake Ontario have not declined since the 1960s.

Recommendation: Make the observed points on the Figure more accurately reflect observed data.

Southwest

Specific comment 146. *Chapter "Southwest", Page 138, Lake Powell picture.*

Comment: Profoundly misleading. The "bathtub ring" is the high water mark from the 1983 El Nino, far above the mean lake level. Hasn't ANYONE involved in this report been to Lakes Mead or Powell where the guide will be happy to inform what that ring is from?