# The Missing Science from the Draft National Assessment on Climate Change

**Review of the draft *National Assessment***

## 1. Introduction

This review is a product of the Center for the Study of Science at the Cato Institute. It was largely written by Patrick J. Michaels and Paul C. Knappenberger of Cato. Additional text was supplied by Robert C. Balling, Arizona State University, Mary J. Hutzler, Institute for Energy Research, and Craig D. Idso, Center for the Study of Carbon Dioxide and Global Change.

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One wonders how familiar the 240 authors of the 2013 draft *National Assessment* are with Karl Popper's famous essay on the nature of science and its distinction from "pseudoscience." The essential difference is that science only explains some things and that its hypotheses forbid others, while a theory that is not refutable by any conceivable event—i.e., one that is universally and comprehensively explanatory—is pseudoscience. For Popper, science is characterized by *risky predictions* (such as gravitational lensing of light in relativity), while pseudoscience does not lend itself to such testing. His favorite examples of pseudoscience were Marxism and Freudian psychology.

This *National Assessment* is much closer to pseudoscience than it is to science. It is as explanatory as Sigmund Freud. It clearly believes that virtually everything in our society is tremendously dependent the surface temperature, and, because of that, we are headed towards certain and inescapable destruction, unless we take its advice and decarbonize our economy, pronto. Unfortunately, the *Assessment* can't quite tell us how to accomplish that, because no one knows how.

In the Assessment's 1200 horror-studded pages, almost everything that happens in our complex world—sex, birth, disease, death, hunger, and wars, to name a few—is somehow made worse by pernicious emissions of carbon dioxide and the joggling of surface average temperature by a mere two degrees.

Virtually every chapter in the *Assessment* perseverates on extreme weather, despite the U.N.s Intergovernmental Panel on Climate Change statement that:

> There is medium evidence and high agreement that long-term trends in normalized losses have not been attributed to natural or anthropogenic climate change

The *Assessment* is woefully ignorant of humanity's ability to adapt and prosper in response to challenges. The quintessence of this is the truly dreadful chapter on human health and climate change.

While death, disease, poverty and injustice are all conjured by warming, there is not one mention of the fact that life expectancy in the U.S. is approximately twice what it was in the year 1900, or that per-capita income in real dollars is over ten times what it was then. It emphasizes diseases that will somehow spread because of warming, neglecting the fact that many were largely endemic when it was colder and were eradicated as we warmed a bit.

Further, it conspicuously ignores the fact that doubling the life expectancy of some 200 million Americans who lived in the 20th century is *the same as saving 100 million lives*. The society that achieved this powered itself on the combustion of fossil fuels. Does this community of experts understand that the number of lives that it effectively saved is orders of magnitude above and beyond it could possibly cost? It seems, given the panoply of horrors due to start pronto, to prefer that we not have emitted carbon dioxide in the first place. Perhaps they ought to look a place that didn't. Surely part of the $3.5 billion that the US Global Change Research Program (USGCRP) consumes per year could finance a field trip to Chad, so they can see the world without cheap and abundant energy.

And what is the purpose of this *Assessment*? The motto of the USGCRP says it all:

*Thirteen Agencies, One Mission: Empower the Nation with Global Change Science*.

The operative word is "empower," which is the purpose of the *Assessment*. It is to provide cover for a massive regulatory intrusion, and concomitant enormous costs in resources and individual liberty. History tells us that when scientists willingly endorse sweeping governmental agendas fueled by dodgy science, bad things soon happen. To borrow the meter of Winston Churchill:

*Never in the history of pseudoscientific consensus will so much be done to so many by so few*.

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**Chapter 1, Executive Summary**

We have only two specific comments on this chapter. Manifold changes that need to be made to it flow from our voluminous commentaries on the other chapters that we reviewed, which Chapters 2 are through 9.

**Page 16, line 1**

You need to add that you also relied upon non-peer-reviewed grey literature produced by advocacy or political organizations. The following examples are just from Chapter 3 (Water Resources):

Adams, A., D. Behar, K. Brooks, P. Fleming, and L. Stickel, 2012: Water Utility Climate Alliance's Technical Input to the 2012 NCA

Barsugli, J., C. Anderson, J.B. Smith, and J.M. Vogel, 2009: Options for improving climate modeling to assist water utility planning for climate change. Final Report, Water Utility Climate Alliance. [Available online at http://www.wucaonline.org/assets/pdf/pubs_whitepaper_120909.pdf]

Berry, L., 2012: Florida Water Management and Adaptation in the Face of Climate Change: A white paper on climate change and Florida's resources.

City of New York, 2012: PlaNYC Progress Report 2012. A Greener, Greater New York, City of New York. [Available online at http://nytelecom.vo.llnwd.net/o15/agencies/planyc2030/pdf/PlaNYC_Progress_Report_2012_W11 eb.pdf]

Liverman, D., S. Moser, P. Weiland, L. Dilling, M. Boykoff, H. Brown, D. Busch, E. Gordon, C. Greene, E. Holthaus, D. Niemeier, S. Pincetl, J. Steenburgh, and V. Tidwell, 2012: Climate Choices for a Sustainable Southwest. *Assessment of Climate Change in the Southwest United* States*: a Technical Report Prepared for the U.S. National Climate Assessment. A report by the* S*outhwest Climate Alliance*, G. Garfin, A. Jardine, R. Merideth, M. Black, and J. Overpeck, 1 Eds., Southwest Climate Alliance, pp. 684-734

Means, E., M. Laugier, J. Daw, L. Kaatz, and M. Waage, 2010a: Decision support planning methods: Incorporating climate change uncertainties into water planning, Water Utility Alliance. [Available online at http://www.wucaonline.org/assets/pdf/pubs_whitepaper_012110.pdf]

And, our favorite:

Union of Concerned Scientists, 2009: Climate Change in the United States: The Prohibitive Costs of Inaction. [Available online at http://www.ucsusa.org/global_warming/science_and_impacts/impacts/climate-costs-of-inaction.html]

**Page 16, lines 15-17**

By citing the 2009 document, *Global Climate Change Impacts on the United States*, this draft *Assessment* has re-opened commentary on that document, too.

Here is a small portion of our review of the 2009 draft:

> 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…
>
> …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 simply false…
>
> …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 almost 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.

As with the 2013 document, the commentary period was insufficiently long for us to address what we deemed to be all the problems with the 2009 report, even though our comments exceeded 75 single-spaced pages. But, since then, we have produced a comprehensive document, in precisely the same format as the USGCRP document, covering exactly the same material, only including the science the USGCRP left out, or noting when it had improperly cited science, and, in one notable case in the Alaska section, completely misrepresented a refereed paper. The document can be found at http://www.cato.org/pubs/Global-Climate-Change-Impacts.pdf
And we now enter our "Addendum" to it in the official record.

## Chapter 2 Our Changing Climate

### General Comment

The scientific literature is fast becoming populated with studies which show that the equilibrium climate sensitivity is better constrained (especially at the high end) than estimates from the Intergovernmental Panel on Climate Change (IPCC) and, perhaps

more importantly for the purposes of this National Climate Assessment (NCA), that the best estimate of the climate sensitivity is considerably lower than the climate model ensemble average. From the recent literature, the central estimate of the equilibrium climate sensitivity is about 2°C, while the CMIP3 climate model average is about 3.3°C (IPCC, 2007). Thus, the recent literature supports an equilibrium climate sensitivity that is some 40% lower than the model average.

To the extent that the recent literature ultimately produces a more accurate estimate of the equilibrium climate sensitivity than does the climate model average, it means that, in general, all of the projections of future climate change given in the NCA are, by default, some 40% too large (too rapid) and the associated (and described) impacts are gross overestimates.

Our recommendation is that an alternative set of projections is developed for all topics discussed in the NCA which incorporates for the latest scientific findings on the lowered value of equilibrium climate sensitivity. Without the addition of the new projections, the NCA will be obsolete on the day of its official release.

## Specific Comments

### Page 25, lines 27-29

### Key Message 1

"Much of the climate change of the past 50 years is primarily due to human activities."

"Human activities" presumably includes a wide range of activities over and beyond emissions of greenhouse gases and other compounds to the atmosphere. Yet throughout the NCA, the observed changes are described and compared with general circulation model (GCM) recreations and projections of climate conditions—and yet these models by and large do not include the majority of the set of "human activities." For example, they do not include the role of irrigation in the Central Valley of California (Lo and Famiglietti (2013)  or the Midwestern U.S. (e.g., DeAngelis et al., 2010; Groisman et al., 2012) which has been shown to influence precipitation, soil moisture, and run-off in downstream regions. The models do not include changes to river courses, water flow barriers, or infiltration changes which can act to increase the frequency and magnitude of flood events (e.g. Pielke., Jr., 1999). The models do not include the influence of urbanization which has been shown to increase extreme precipitation events (e.g. Ashley et al., 2011) or the influence of dams which also can act to enhance precipitation (Degu et al., 2011). And the list could on.

All these changes are the results of "human activities" and impact the observed climate, yet the NCA treats most of these impacts as if they are the result of human atmospheric emissions, primarily greenhouse gases. This is wrong.

Recommendation: The NCA must include a fuller description of observed climate changes that may be caused by things other than anthropogenic global warming (AGW) and how these non-AGW influences complicate the climate change picture and often lessen the ability to identify changes causes by AGW.

References:

Ashley, W.S., M.L. Bentley, and J. A. Stallins, 2011. Urban-induced thunderstorm modification in the Southeast United States. *Climatic Change*, doi:10.1007/s10584-011-0324-1.

DeAngelis, A., F. Dominguez, Y. Fan, A. Robock, M. D. Kustu, and D. Robinson, 2010. Observational evidence of enhanced precipitation due to irrigation over the Great Plains of the United States *Journal of Geophysical Research*, 115, D15115, 14 pp., doi:10.1029/2010JD013892.

Degu, A. M., F. Hossain, D. Niyogi, R. Pielke Sr., J. M. Shepherd, N. Voisin, and T. Chronis, 2011. The influence of large dams on surrounding climate and precipitation patterns. *Geophysical Research Letters*, 38, L04405, doi:10.1029/2010GL046482.

Groisman, P. Ya., R. W. Knight, and T. R. Karl, 2011. Changes in intense precipitation over the central U.S. *Journal of Hydrometeorology*, 13, 47-66.

Lo, M-H., and J.S. Famiglietti. 2013. Irrigation in California's Central Valley strengthens the southwestern U.S. water cycle. *Geophysical Research Letters,* 40, doi:10.1002/GRL.50108.

Pielke, R.A., Jr., 1999. Nine fallacies of floods. *Climatic Change*, 42, 413-438.

**Page 25, lines 30-33**

**Key Message 2**

"The magnitude of climate change beyond the next few decades depends primarily on the amount of heat-trapping gases emitted globally, and how sensitive the climate is to those emissions."

The NCA correctly recognizes that both future emissions AND the climate sensitivity impact the magnitude of future climate change and yet throughout the NCA projections are only shown for different emissions pathways. Where is the set of projections showing the impact of different climate sensitivities? Evidence continues to build that the true equilibrium climate sensitivity (i.e., the best estimate from a collection of recent publications in the scientific literature) is 40% smaller than the average climate

sensitivity of the GCMs used in the NCA (see our Comment Page 31, Lines 15-18 for further details).

Recommendation: Develop an alternative set of projections for all topics discussed in the NCA which incorporates for the latest scientific findings on the lowered value of equilibrium climate sensitivity.

**Page 25, lines 34-35**

**Key Message 3**

"U.S. average temperature has increased by about 1.5°F since record keeping began in 1895; more than 80% of this increase has occurred since 1980."

The 80% number is grossly misleading and statistically unjustified (see our Comment on Page 35, lines 8-9, and lines 20-23 for an in-depth analysis). It is a glaring example of the bias which currently exists in the NCA.

Recommendation: Entirely remove any references to increases in temperature relative to the overall change as such statements are statistically unsound.

**Page 26, lines 8-9**

**Key Message 5**

"More winter and spring precipitation is projected for the northern U.S., and less for the Southwest, over this century."

While it may be true that the GCMs used in the NCA make these projections, there has been no verification study performed which indicates that these same GCMs have any skill in projecting the observed patterns of precipitation which have taken place to date. In fact, there are scientific studies which suggest just the opposite (Zhang et al., 2007; Polson et al., 2013).

Recommendation: Remove all discussion from the NCA concerning future patterns of precipitation change unless/until the GCMs producing them can be demonstrated to accurately capture observed characteristics (spatial and temporal patterns and magnitudes) of precipitation changes across the U.S.

References:

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

Zhang, X., et al., 2007. Detection of human influence on the twentieth-century precipitation trends. *Nature*, 448, 461-466, doi:10.1038/nature06025

**Page 26, lines 10-13**

**Key Message 6**

Changes in heavy precipitation have been shown to be influenced by things other than AGW. Influences include among other things, anthropogenic aerosol emissions (e.g., Rosenfeld and Bell, 2011; Li et al., 2011; Koren et al., 2012; Heiblum et al. 2012; Fan et al., 2012), changes in irrigation (e.g., DeAngelis et al., 2010; Groisman et al., 2012), water impoundments (e.g., Degu et al., 2011), changes in land use/urbanization (e.g., Ashley et al., 2011), and natural variability (e.g., Balling and Goodrich, 2012). Until these other influences can be accounted for, it is misleading to imply that AGW is the cause and thus GCM projections of future changes are applicable, much less reliable.

Recommendation: Discuss the broader array of influences on heavy precipitation and how influence other than AGW may impact the reliability of GCM projections.

References:

Ashley, W.S., M.L. Bentley, and J. A. Stallins, 2011. Urban-induced thunderstorm modification in the Southeast United States. *Climatic Change*, doi:10.1007/s10584-011-0324-1.

Balling, R.C., and G.B. Goodrich, 2011. Spatial analysis of variations in precipitation intensity in the USA. *Theoretical and Applied Climatology*, 104, 415-421, doi:10.1007/s00704-010-0353-0.

DeAngelis, A., F. Dominguez, Y. Fan, A. Robock, M. D. Kustu, and D. Robinson, 2010. Observational evidence of enhanced precipitation due to irrigation over the Great Plains of the United States *Journal of Geophysical Research*, 115, D15115, 14 pp., doi:10.1029/2010JD013892.

Degu, A. M., F. Hossain, D. Niyogi, R. Pielke Sr., J. M. Shepherd, N. Voisin, and T. Chronis, 2011. The influence of large dams on surrounding climate and precipitation patterns. *Geophysical Research Letters*, 38, L04405, doi:10.1029/2010GL046482.

Groisman, P. Ya., R. W. Knight, and T. R. Karl, 2011. Changes in intense precipitation over the central U.S. *Journal of Hydrometeorology*, 13, 47-66.

Fan, J., D. Rosenfeld, Y. Ding, L. R. Leung, and Z. Li, 2012. Potential aerosol indirect effects on atmospheric circulation and radiative forcing through deep convection, *Geophysical Research Letters*, doi:10.1029/2012GL051851.

Heiblum, R. H., I. Koren, and O. Altaratz, 2012. New evidence of cloud invigoration from TRMM measurements of rain center of gravity. *Geophysical Research Letters*, 39, L08803, doi:10.1029/2012GL051158.

Koren, I., O. Altaratz, L. A. Remer, G. Feingold, J. V. Martins, and R. H. Heiblum**,** 2012. Aerosol-induced intensification of rain from the tropics to the mid-latitudes, *Nature Geosciences*, **5**, 118–122, doi:10.1038/ngeo1364.

Li, Z., F. Niu, J. Fan, Y. Liu, D. Rosenfeld, and Y. Ding, 2011. The long-term impacts of aerosols on the vertical development of clouds and precipitation. *Nature Geoscience*, doi: 10.1038/NGEO1313.

Rosenfeld, D., and T. L. Bell, 2011. Why do tornados and hailstorms rest on weekends? *Journal of Geophysical Research*, 116, D20211, doi:10.1029/2011JD016214.

**Page 26, lines 14-19**

**Key Message 7**

Again, this is another example of where the NCA discusses changes in the frequency and intensity of some types of climate events without any formal attribution, and then follows the descriptions up with future GCM projections. There are a host of potential causes for the observed changes (including natural variability) and many do not necessary support future projections made from GCMs run under increasing greenhouse gas emissions pathways. Additionally, new research suggest that finer scale models project less drying in the Southwest than do coarser GCMS (Gao et al., 2011; Gao et al., 2012, Lo and Famiglietti, 2013).

Recommendation: A broader exploration of the reasons behind the observed changes needs to be made and a discussion as to how these causes, as well as new finer-scale models, may impact the reliability of GCM projections.

References:

Gao, Y., J. Vano, C. Zhu, and D. P. Lettenmaier. 2011. Evaluating climate change over the Colorado River basin using regional climate models. *Journal of Geophysical Research,* 116, D13104, doi:10.1029/2010JD015278.

Gao, Y., et al. 2012. Moisture flux convergence in regional and global climate models: Implications for drought in the southwestern United States under climate change. *Geophysical Research Letters,* 39, L09711, doi:10.1029/2012GL051560.

Lo, M-H., and J.S. Famiglietti. 2013. Irrigation in California's Central Valley strengthens the southwestern U.S. water cycle. *Geophysical Research Letters,* 40, doi:10.1002/GRL.50108.

**Page 26, lines 20-24**

**Key Message 8**

There is a complete lack of discussion as to the potential changes to hurricane tracks under AGW, yet there are many studies in the scientific literature that find that hurricane tracks will be shifted eastward, reducing the frequency of tropical cyclones which make landfall in the U.S. (e.g., Wang and Lee, 2008; Wang et al, 2008; Murakami and Wang, 2010; Wang et al., 2011; Murakami et al., 2012). As the overwhelming impact from Atlantic tropical cyclones on U.S. interests is associated with systems which make landfall, as it stands now the NCA is terribly remiss not to discuss this most important topic.

Recommendation: Add a discussion on potential AGW influences on the preferred tracks of Atlantic basin tropical cyclones.

References

Murakami, H., and B. Wang, 2010. Future Change of North Atlantic Tropical Cyclone Tracks: Projection by a 20-km-Mesh Global Atmospheric Model. *Journal of Climate*, 23, 2699–2721. doi:10.1175/2010JCLI3338.1.

Murakami, H., et al., 2012. Future Changes in Tropical Cyclone Activity Projected by the New High-Resolution MRI-AGCM. *Journal of Climate*, 25, 3237–3260. doi: 10.1175/JCLI-D-11-00415.1.

Wang, C., and S.-K. Lee, 2008. Global warming and United States landfalling hurricanes, *Geophysical Research Letters*, 35, L02708, doi:10.1029/2007GL032396.

Wang, C.L., S-K. Lee, and D.B. Enfield, 2008. Atlantic Warm Pool acting as a link between Atlantic Multidecadal Oscillation and Atlantic tropical cyclone activity. *Geochemistry, Geophysics, Geosystems*, 9, Q05V03, doi:10.1029/2007GC001809.

Wang, C., L. Hailong, S-K. Lee, and R. Atlas, 2011. Impact of the Atlantic warm pool on United States landfalling hurricanes. *Geophysical Research Letters*, 38, L19702, doi:10.1029/2011GL049265.

**Page 26, lines 30-31**

**Key Message 9**

In discussions of the projections of global sea level rise, it is imperative to discuss the impacts of new evidence that suggests that the equilibrium climate sensitivity is 40%

lower than the GCM average used in the NCA projections. Almost certainly, models with a 40% lower equilibrium climate sensitivity will project less sea level rise.

See our Comment Page 31, Lines 15-18 for further details.

**Page 26, lines 34-35**

**Key Message 11**

While certainly there is "concern" about marine ecosystems in a changing climate, there is also mounting evidence that marine ecosystems are not as fragile as they are often made out to be and that they can adapt to changing environmental conditions (e.g., Schmidt, 2013).

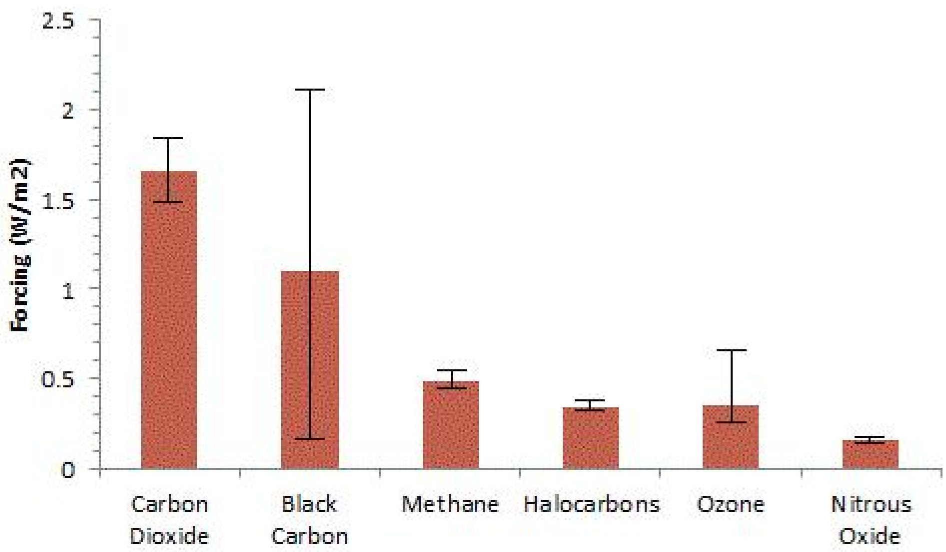
Reference:

Schmidt, C., 2013. As threats to corals grow, hints of resilience emerge. *Science*, 339, 1517-1519.

**Page 28, lines 27-31**

There seems to be an implication here that carbon dioxide is primarily responsible for most of the observed warming since 1950. But this is grossly misleading.

A recent study (Bond et al., 2013) has shown that black carbon (soot) emissions have a positive forcing that is equivalent to about 70% that of carbon dioxide (about 3 times more than generally realized). Wigley and Santer (2012) show that about 10% of the warming since 1950 has been due to ENSO. Taken in combination, these results imply that CO2 is responsible for less than one-third of the warming since 1950.




*CAPTION: Relative warming influence of the major anthropogenic emissions (whiskers are the 90% uncertainly bounds) (from IPCC AR4, and Bond et al., 2013).*

Recommendation: Change text to reflect that most the warming in the past 50 years can be explained by things other than carbon dioxide.

References:


Bond, T.C., et al., 2013. Bounding the role of black carbon in the climate system: A scientific assessment. *Journal of Geophysical Research*, in press.

Wigley, T.M.L., and B.D. Santer, 2012. A probabilistic quantification of the anthropogenic component of twentieth century global warming. *Climate Dynamics*, doi: 10.1007/s00382-012-1585-8.


**Page 28, lines 38-41**

**"**Natural variations can be as large as human-induced climate change over timescales of up to a decade or two at the global scale."

While this is no doubt true, for periods longer than about 15 years, it is contrary to climate model projections. For example, not a single run from any of the CMIP3 models under the A1B scenario has a two-decade long trend in global average annual surface temperature that is less than or equal to zero with a start date at the beginning of the model run.

Why discuss the period 1998-2007? It is now 2013, and data is available at least through 2012. In fact, the trend is global average surface temperature from 1998-2012 is lower than it was from 1998-2007. As this current period of minimal surface temperature rise continues to grow, it increasingly challenges the climate model projections (your

references on line 43 and page 28, line 1 can confirm this—for instance, see where a 16-year temperature change of 0.072°C (the change in the HadCRUT4 from 1997-2012) falls in the distribution of Knight et al., 2009.)

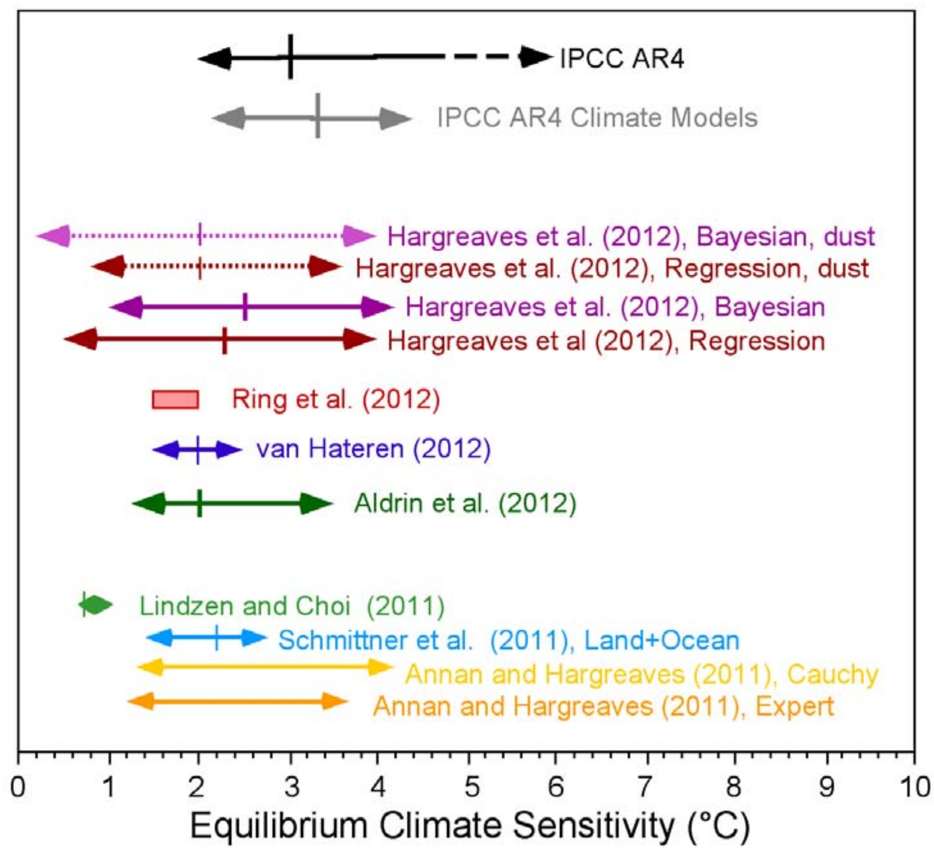
Recommendation: Change the text to say that the current (and growing) period of the general lack of warming is increasingly challenging climate model projections.

**Page 31, Lines 15-18**

"The magnitude of climate change beyond the next few decades depends primarily on the amount of heat-trapping gases emitted globally, and how sensitive the climate is to those emissions."

This is perhaps the generally most glaring problem with the entire NCADAC draft—that is, the failure to address the growing number of recently published findings that suggest that the climate sensitivity is much beneath those contained in the climate models that the projections throughout this entire report have been made.

The following figure shows the extent of the problem.




*CAPTION: Climate sensitivity estimates from new research published since 2010 (colored), compared with the range given in the IPCC* Fourth Assessment Report *(black).*

*The arrows indicate the 5 to 95% confidence bounds for each estimate along with the mean (vertical line) where available.*

The mean equilibrium climate sensitivity of the climate models from the CMIP 3 is 3.3°C. The “best estimate” of the equilibrium climate sensitivity in the IPCC AR4 is 3.0°C. The mean equilibrium climate sensitivity from a collection of recent papers (illustrated in the above figure) lies close to 2.0°C—or about 40% lower than that from the climate models used in this report.

This has major implications for the projected climate changes and their associated impacts as described in the NCADAC draft report—as such, it is not an issue which can be swept under the rug.  Instead, new estimates of climate sensitivity must be prominently discussed and the implications of a 40% lower equilibrium climate sensitivity incorporated throughout the report.

Recommendation: Discuss the new estimates of climate sensitivity and for each and every graphic in NCA report illustrating future projected change include a companion graphic showing the projection assuming a 40% lower climate sensitivity.

References:

Aldrin, M., et al., 2012. Bayesian estimation of climate sensitivity based on a simple climate model fitted to observations oh hemispheric temperature and global ocean heat content. *Environmetrics*, doi:10.1002/env.2140.

Annan, J.D., and J.C. Hargreaves, 2011. On the generation and interpretation of probabilistic estimates of climate sensitivity. *Climatic Change,* 104, 324-436.

Hargreaves, J.C., et al., 2012. Can the Last Glacial Maximum constrain climate sensitivity? *Geophysical Research Letters*, 39, L24702, doi:10.1029/2012GL053872.

Lindzen, R.S., and Y-S. Choi, 2011. On the observational determination of climate sensitivity and its implications. *Asia-Pacific Journal of Atmospheric Sciences,* 47, 377-390.

Ring, M.J., et al., 2012. Causes of the global warming observed since the 19th century. *Atmospheric and Climate Sciences*, 2, 401-415, doi:10.4236/acs.2012.24035.

Schmittner, A., et al., 2011. Climate sensitivity estimated from temperature reconstructions of the Last Glacial Maximum, *Science,* 334, 1385-1388, doi: 10.1126/science.1203513.

van Hateren, J.H., 2012. A fractal climate response function can simulate global average temperature trends of the modern era and the past millennium. *Climate Dynamics*, doi:10.1007/s00382-012-1375-3.

**Page 35, lines 8-23**

This bit about the 80% increase since 1980 is cherry-picking through and through. The temperature increase in the U.S. has not been linear, but a combination of multi-decadal ups and downs. As such, it is hard to know even how to go about performing such a calculation.

Probably the best way to do this would be to simply take the overall rate of temperature rise (0.13°F/decade) and multiply it by the number of decades between 1980 and now (3.3) and then divide by the overall temperature change (1.5°F). When you do this, you get 29% of the overall rise has occurred since 1980. Since 29% is nowhere close to being "more than 80%," clearly this is not how the NCADAC authors made their determination.

Another way to do it would be to find the maximum amount of temperature rise that occurred at any time before 1980 and then determine how much more the temperature since 1980 has risen above that amount. For example, from 1895 through 1940, the U.S. annual average temperature increased at a rate of 0.27°F per decade for 4.5 decades for a total rise of 1.2°F. That only leaves 0.3°F of the total overall rise of 1.5°F left over. So the maximum proportion of temperature rise that could have occurred since 1980 is 20%. Again, 20% is nowhere close to being more than 80%, so clearly this is not how it was done.

Another way would be to calculate the linear temperature rise between 1895 and 1979, subtract that from the 1.5°F total rise and assign whatever is left over to the period 1980 to 2012. When you do this, you get that 77% of the rise occurred since 1980. At least this is starting to get close to the USGCRP number.

Or, you could calculate the linear rise from 1980 to 2012 and compare this to the total rise. When you do this, you find that the rise from 1980 to 2012 was 1.58°F. Or, 105% of the total rise! 105% is definitely more than 80%, so maybe that is what they did.

Or perhaps the NCADAC authors did something completely different. Who knows?

Now, before we go any further, let's get something straight—*none of these methods for determining the proportionate amount of warming is statistically sound* because the nature of temperature rise in the U.S. during the last 118 years is not strictly linear.

Instead, there are multi-decadal periods of rising and falling temperatures. So attempting to describe the proportional change over some period of time is cherry-picking by design. As we show in the examples above, you have a wide variety of answers at your disposal depending on your analysis method. The NCADAC authors clearly wanted to choose a method that produced the appearance of a lot of rise since 1980 (and thereby completely disregarding a more rapid warming from 1910-1940).

It is unclear to us why such an unscientific motive should exist at the NCADAC. This needs to be rectified immediately.

Recommendation: Entirely remove any references to increases in temperature relative to the overall change as such statements are statistically unsound.

**Page 39, line 22-23**

**"**and length of the ragweed pollen season (Ziska et al. 2011)."

Why on earth did you cherry pick ragweed to single out from virtually every single other plant which benefits from a longer growing season? Is it because there is some implication that ragweed doing better is actually a negative (for reasons to reconsider this see our Comment on Page 336, lines 1-12)—contrary to other kinds of plants doing better?

If this is the case, are we to take it that you desire to provide at least one example of a scientific research result which describes an impact counter to what you consider the prevailing one?

Recommendation: Keeping the precedent set here, provide examples of positive impacts for all the environmental changes you negatively describe throughout the report.

**Page 39, lines 23-32**

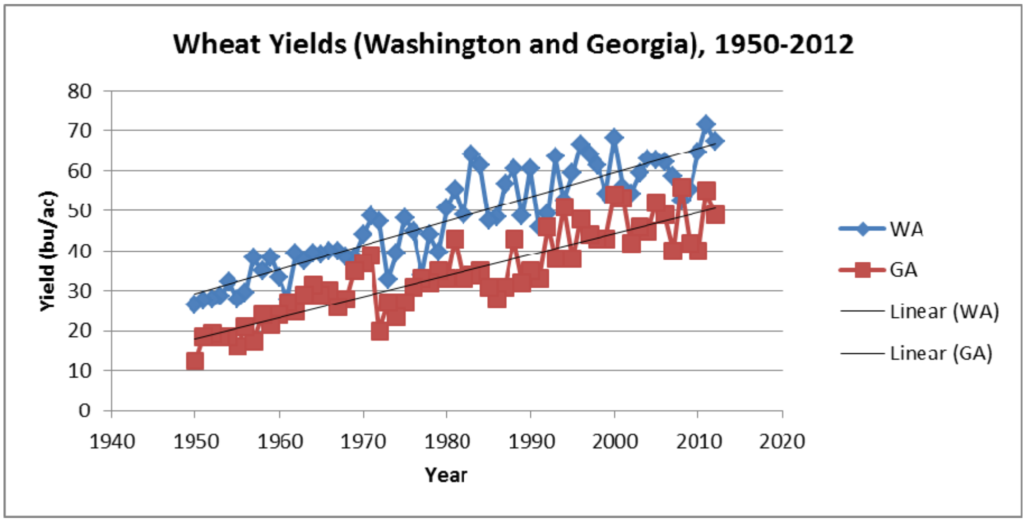
"A longer growing season can mean greater evaporation and loss of moisture through plant transpiration associated with higher temperatures so that even with a longer frost-free season, crops could be negatively affected by drying."

There is a lot wrong with this sentence.

First of all, you seem to have left out any reference to overwhelming and widely accepted scientific evidence that increased atmospheric carbon dioxide levels increase the water use efficiency of plants.

In lines 23-26 you set forth a hypothesis that longer growing seasons could negatively affect crops. In lines 28-32, you describe differential changes to the lengths of the growing season across the country. So this sets up a good test of whether crops are negatively affected. For example, in the figure below I plot the annual wheat yields from Washington state where the growing season increased by (according to Figure 2.9) some 18 days, and the wheat yields in Georgia where the growing season increased by only 5 days. According to the hypothesis that you all laid out above, wheat yields in Washington should have declined relative to wheat yields in Georgia. But, it turns out they are practically indistinguishable (actually linear trend in increasing yield is slightly greater in Washington than in Georgia) over the past 60+ years. So, it appears that your

hypothesis that increased growing season will lead to reduced crop yields is readily falsifiable.




*CAPTION: Annual statewide average wheat yields from Washington and Georgia, 1950-2012 (data source: National Agricultural Statistics Service, USDA)*

Recommendation: Remove the contention that longer growing seasons will negatively impact crops.

**Page 43, lines 3-7**

**"**However, if emissions of heat-trapping gases continue their upward trend, clear patterns of precipitation change are projected to emerge."

Is there any way to assess the validity of these projections? Otherwise, are they not pure speculation? How do the models perform over the 20th century? What is the size of natural variability? Is it possible that the precipitation changes depicted in Figure 2.11 already exceed those projected to occur in Figure 2.12 and Figure 2.13 (for example, based on a visual averaging of the projections depicted in Figure 2.12, it looks like the observed changes in the upper Midwest already exceeded those projected to occur by the end of the 21st century)?

To help better assess this, and to give the reader a sense of the size of natural variability vs. model projections, either the observed changes should be shown for each season, or the model projections shown for the annual change. As it stands now, you show observed changes in *annual* precipitation and model projections for *seasonal* precipitation. Why? You did not do this in section on "Recent U.S. Temperature Trends,"—there you showed annual observed changes and annual projections.

Additionally, to better allow the reader to understand the size of the "noise" of natural variability compared with the "signal" of AGW, you should include some information as

to whether the projected changes in Figure 2.12 are expected to emerge from the noise of natural variability by the verifiable time period (i.e. 2070-2099). Otherwise, the projected changes, even if correct, will be largely undetectable and potentially inconsequential during the projected time period. To help with that task, we have prepared the following Table showing how many years it will take before the projected precipitation change will be larger than natural variability.

To prepare it, we looked only at the high-confidence state-season combinations (i.e. those which are hatched in Figure 2.12). From that Figure, we estimated the percentage change in projected seasonal precipitation for each state so identified (dividing the total percentage change from Figure 2.12 by the time over which it occurred, 155 years (the difference in midpoints from the projected period 2070-2099 to the base period 1901-1960 in Figure 2.12), gave us the projected precipitation change per year, in percent). We then calculated the standard deviation (in percentage of the mean) around the observed (1901-2011) precipitation data for each state identified above (using data from McRoberts and Nielsen-Gammon, 2011). Dividing the observed standard deviation by the projected change per year gave us the time (in years) until the projected change exceeded 1 standard deviation of observed variability. Those numbers populate our Table.

There were 84 separate season-state combinations where confidence is large. In nine of these, the predicted change has already emerged from the noise (some 70 years ahead of time—not sure that is good or bad for the models). Of those nine, eight of them are precipitation increases, with the most in spring. Farmers should do handstands over that.

There were 75 cases where the observed change to date is still less than the model projected change by the 2070-2099 averaging period. During the summer, the average time for the projected changes to emerge from the natural noise (i.e. exceed 1 standard deviation) is 520 years. In the winter, it is 330 years. Averaged across all seasons it will take approximately 297 years before a state's projected precipitation changes emerge from background variability—and this is only for those states where the models agree (i.e. hatched areas in Figure 2.12)—for the majority of the rest of the country, the time for emergence from the noise is longer still.

This needs to be made readily apparent to the reader of the NCA.

| State | DJF | MAM | JJA | SON |
|---|---|---|---|---|
| AL | n/a | **211** | **444** | Achieved |
| AZ | **1109** | **221** | n/a | n/a |
| AR | n/a | **1146** | 127 | n/a |
| CA | n/a | 332 | 2111 | 467 |
| CO | **417** | **229** | | n/a |
| CT | 183 | n/a | n/a | n/a |
| DE | **275** | n/a | n/a | n/a |
| FL | n/a | 145 | 139 | **265** |
| GA | n/a | Achieved | n/a | Achieved |

| | | | | |
|---|---|---|---|---|
| ID | **285** | Achieved | **380** | n/a |
| IL | 76 | 76 | n/a | n/a |
| IN | **258** | 76 | n/a | n/a |
| IA | 197 | Achieved | n/a | n/a |
| KS | Achieved | n/a | **1017** | n/a |
| KY | **695** | n/a | n/a | n/a |
| LA | n/a | 168 | **230** | n/a |
| ME | 140 | 80 | n/a | n/a |
| MD | **259** | n/a | n/a | n/a |
| MA | 85 | n/a | n/a | n/a |
| MI | 94 | 97 | **263** | Achieved |
| MN | 151 | 88 | n/a | n/a |
| MS | n/a | **391** | n/a | n/a |
| MO | n/a | n/a | **306** | n/a |
| MT | **473** | 15 | **833** | n/a |
| NE | **463** | n/a | n/a | n/a |
| NV | n/a | **419** | **370** | n/a |
| NH | 97 | 41 | n/a | n/a |
| NJ | **221** | n/a | n/a | n/a |
| NM | **1660** | **246** | n/a | n/a |
| NY | 112 | 120 | n/a | n/a |
| NC | n/a | n/a | n/a | n/a |
| ND | 122 | 99 | n/a | n/a |
| OH | **266** | 121 | n/a | n/a |
| OK | n/a | **629** | **313** | n/a |
| OR | **444** | n/a | **249** | n/a |
| PA | 154 | 182 | n/a | n/a |
| RI | 131 | n/a | n/a | n/a |
| SC | n/a | n/a | n/a | n/a |
| SD | **207** | Achieved | n/a | n/a |
| TN | n/a | n/a | n/a | n/a |
| TX | **1217** | **206** | n/a | n/a |
| UT | n/a | **450** | n/a | n/a |
| VT | 45 | 58 | n/a | n/a |
| VA | **319** | n/a | n/a | n/a |
| WA | **356** | Achieved | **200** | n/a |
| WV | **330** | n/a | n/a | n/a |
| WI | 144 | 85 | n/a | n/a |
| WY | **249** | n/a | n/a | n/a |
| **AVG** | **340** | **228** | **520** | **366** |

*TABLE: Years until projected change (in Figure 2.12) exceeds one standard deviation (calculated using the 1896-2011 data) from the 1991-2011 average value (calculated using McRoberts and Nielsen-Gammon, 2011). A "n/a" indicates that no consistent projection was made, "achieved" means that the projected change has already been exceeded (that is, the change from 1901-1960 to 1991-2011 was larger than the climate*

*model projected change from 1901-1960 to 2070-2099). Highlighted values indicate two centuries or more.*

Also it would be instructive to present the observed time series of U.S. precipitation changes along with the time series of the same quantity from climate models. I would suggest doing this on seasonal basis as well as an annual one. This would again allow the reader to get an idea about the ability of climate models to simulate precipitation on scales of the U.S. or smaller. The 2000 National Assessment Report included such a comparison. Not sure why it was removed in this Assessment (other than perhaps that it doesn't show favorable results for the models).

Recommendation: Produce verification statistics for the model precipitation fits of 20th century precipitation changes for seasons and subregions of the U.S. Also include figures showing the climate model precipitation changes across the U.S. during the 20th century for each season and compare with the observed patterns of change.

Reference:

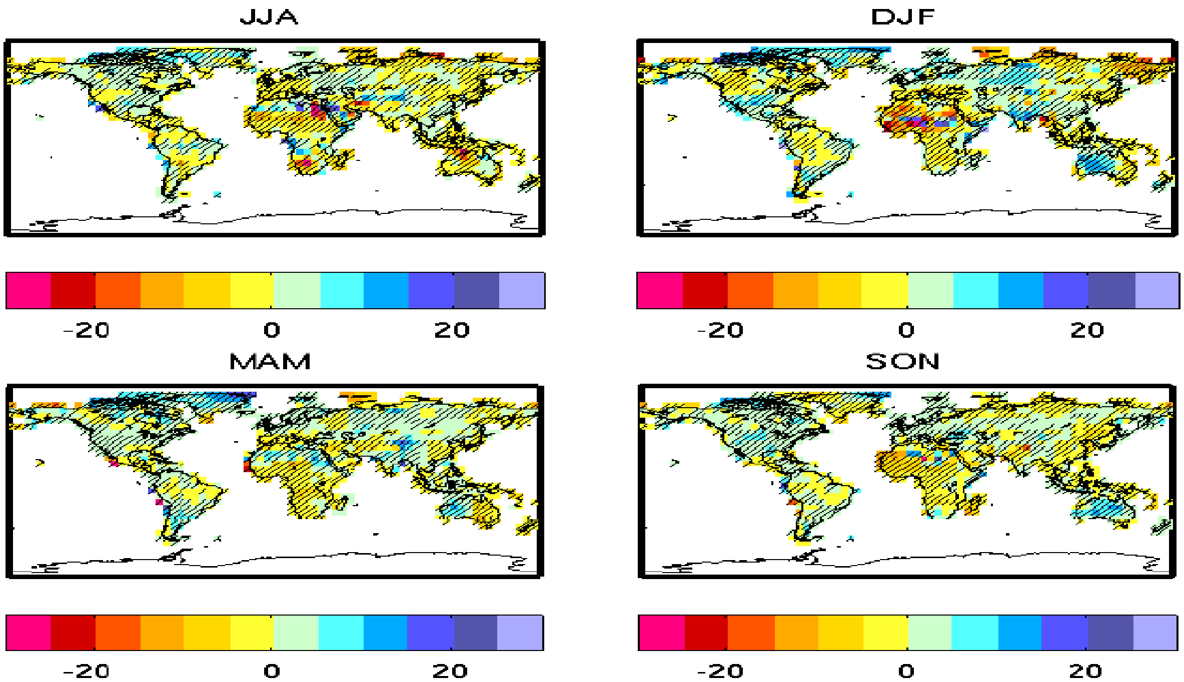
McRoberts, D.B. and J.W. Nielsen-Gammon, 2011. A New Homogenized Climate Division Precipitation Dataset for Analysis of Climate Variability and Climate Change. *Journal of Applied Meteorology and Climatology*, 50**,** 1187-1199.

**Page 43, lines 18-19**

Just how much confidence does the scientific literature support for patterns and magnitude of precipitation changes driven by anthropogenic climate change over the U.S.? For example, isn't it well established that the observed changes are several times larger than the model projected changes over the 20th century, especially on a local/regional basis? Again, graphic showing modeled changes over the 20th century in the U.S. would be instructive. How much of the modeled changes over the U.S. are driven by anthropogenic climate alterations?

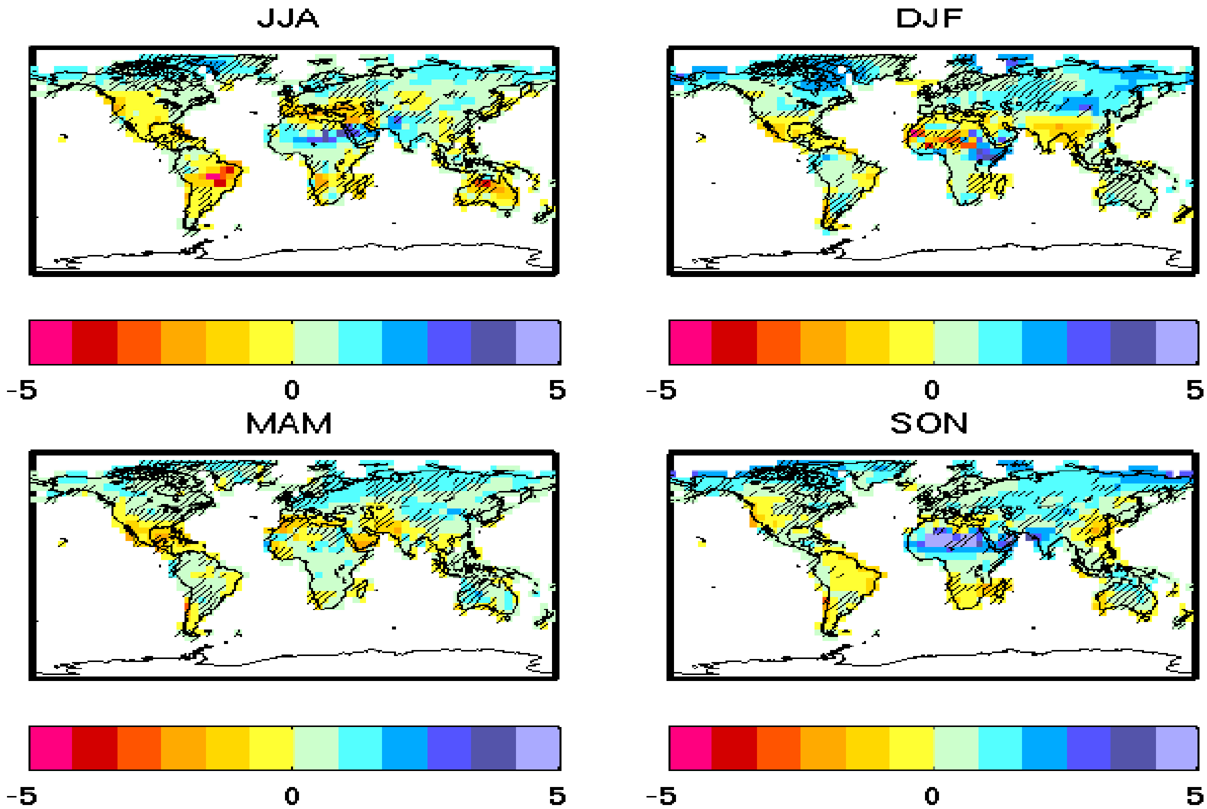
Findings in Polson et al. (2013) suggest the answer to the above question is "not much." The first figure below depicts the observed changes in precipitation by season over land areas (including the U.S) from 1950-2005 from several different observed datasets (from Polson et al., 2013). The hatches show where all the datasets give the same sign of the trend. Over most of the U.S. during most seasons, there is general agreement. The second figure shows the projections of the multi-model mean. The hatched areas are where the multi-model mean and the observed datasets all agree as to the sign. In this case, over much of the U.S., over most seasons, there is *not* agreement. And this test only assesses the sign of the trend, not the magnitude.

**Observations, 1951 - 2005**



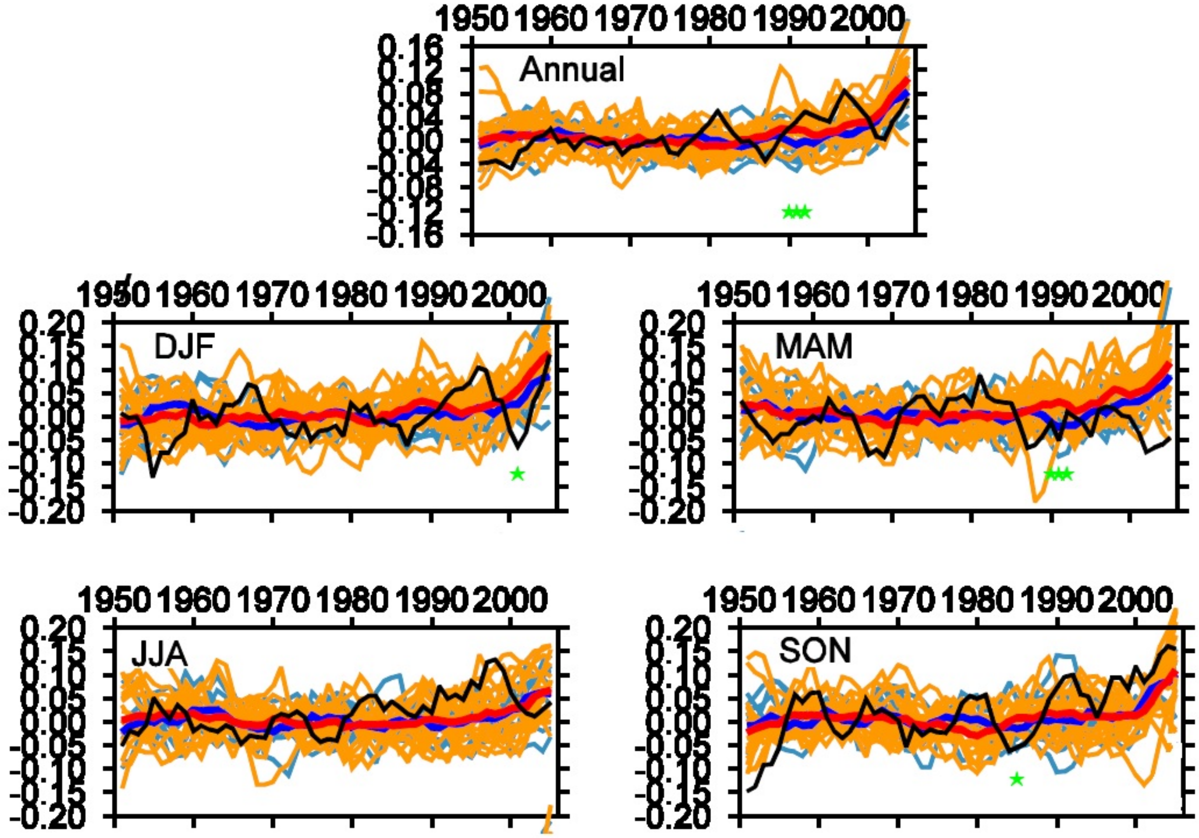

*CAPTION: Percentage change in precipitation per decade for 1951-2005 for DJF, MAM, JJA and SON. Hatched grid-boxes show where the sign of the change is consistent across all observation datasets with data available for that grid-box. (source: Polson et al., 2013).*

**Models, 1951-2005**




*CAPTION: Percentage change in precipitation per decade for the ALL forced multi-model mean for 1951- 2005 for DJF, MAM, JJA and SON. Hatched grid-boxes show where the sign of the change is consistent across all four observation datasets and the multi-model mean. Note the smaller scale of change patterns as multi-model mean changes show a much reduced influence of internal climate variability. (source: Polson et al., 2013)*

Looking over latitude bands, Sarojini et al. (2012) show that there is no anthropogenic signal in precipitation over land areas with observations in the 30 to 60 N latitude band for any season during the period 1951-2005. Instead, the slight upward trend in modeled precipitation averaged in this latitude band (where the U.S. largely falls) is driven by natural signals (see figure below).




*CAPTION: Precipitation anomalies in the 30N to 60N latitude band from climate models (masked to match observations) driven by natural (blue) and natural+anthropogenic (red) forcings. Observed changes are in black. Green stars indicate times when there is a statistically significant difference between the NAT and the ALL forcings. Green stars are largely absent indicating that there is no statistically significant difference between NAT and ALL forcings in the modeled precipitation history (1950-2005) of the latitude band which contains the much of the U.S. (Sarojini et al., 2012).*

The results of Sarojini et al. (2012) indicate that there is no anthropogenic "fingerprint" against which to test the ability of climate models' simulation of anthropogenic changes and it is thus impossible to assess the reliability of future projections from changes in anthropogenic forcings.

As mentioned in the NCA text, global climate models are too coarse to capture fine topographic details, especially in mountainous terrain. A good example of this is a paper by Gao et al., (2012) which shows that regional climate models with finer scale terrain show less precipitation decline in the Southwest U.S. that GCMs do.

Recommendation: Until the climate models' ability to accurately portray observed precipitation trends across the U.S. is demonstrated, and the anthropogenic climate change signal identified, that future model-based precipitation projections be removed

from the NCA, or at least talked about in a general sense indicating that natural variability is much larger than any hypothesized anthropogenic climate change signal both on local as well as regional scales. It is not that I don't think, that all else being equal, higher temperatures should generally lead to more precipitation on a global scale, but that all else is not equal and the magnitude of natural variability likely will overwhelm any anthropogenic climate change signal for many, many years into the future. This should be the point of emphasis for the reader.

References:

Gao, Y., et al., 2012. Moisture flux convergence in regional and global climate models: Implications for drought in the southwestern United States under climate change. *Geophysical Research Letters*, 39, L09711, doi:10.1029/2012GL051560.

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

Sarojini, B.B., et al., 2012. Fingerprints of changes in annual and seasonal precipitation from CMIP5 models over land and ocean. *Geophysical Research Letters*, 39, L21706, doi: 10.1029/2012GL053373.

**Page 45, lines 17-19**

"The patterns of precipitation change in the newer CMIP5 simulations are essentially the same as in the earlier CMIP3 and NARCCAP simulations used in impact analyses throughout this report, increasing confidence in our scientific understanding."

Agreement among models *is not grounds for scientific confidence* unless the models can be shown to replicate observations. As we pointed out above, this has not been shown to be the case.

Recommendation: Either provide demonstration that models reliably replicate observations, or drop the discussion about confidence in future projections. For example, Polson et al. (2013) show that the models don't do a very good job in just getting the sign of the observed precipitation change right, much less the magnitude. That does *not* instill confidence in future projections or our scientific understanding.

Reference:

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

**Page 47, lines 6-7**

It is pretty much a fact of climatology that at any location, increases in annual precipitation are driven more by increases in precipitation amounts than by increases in the number of precipitation events. This has the associated consequence that more annual precipitation is accompanied by increases in heavy precipitation events. Such a concept is demonstrated in Michaels et al., 2004, which also shows that fixed bin approaches, like those in the NCA, over emphasize the relative changes in heavy/extreme precipitation. In fact, Michaels et al., 2004 showed that despite an increase in the annual amount of precipitation falling on the wettest day of the year, the percentage of annual precipitation falling on the wettest day of the year was unchanged averaged across the country as a whole, with variations in subregions.

In this section, there is a lot of emphasis on changes in extreme events and virtually no mention that this is largely a natural accompaniment to changes in total annual precipitation.

The tone of the NCA makes it seem like more precipitation is a bad thing. Is this generally the case (i.e., is there some documentation that shows more precipitation is less beneficial than less precipitation)?

Reference:

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.

**Page 47, Line 13**

Add a sentence to the end of this paragraph that attributes much of the changes in heavy precipitation in recent decades to changes in ENSO (Higgis and Kousky, 2013).

Reference:

Higgis, R.W., and V.E. Kousky, 2013. Changes in observed and daily precipitation over the United States between 1950-79 and 1980-2009. Journal of Hydrometeorology, 114, 105-121, DOI: 10.1175/JHM-D-12-062.1.

**Page 53, lines 14-15**

As we have discussed (for example, see our Comment on Page 43, lines 18-19), there is not a basis for confidence in the “projections” of future summer precipitation trends.

**Page 54, lines 2-3**

As we have commented on previously (for example, see our Comment on Page 43, lines 18-19) agreement among computer models for future precipitation conditions across the U.S. (when they display an inability to correctly capture past conditions), is not grounds for rising confidence.

**Page 55, lines 9-21**

You need to be more careful in your discussion regarding the size of any human-caused climate signal in flood event vs. other types of human-caused signals in flood events (such as surface changes, river course modifications, dykes, etc.). There is a lot of literature on the subject, yet none of it is reflected here.

The role that climate change may play in impacts from flooding, over and above that of the natural climate, human changes to waterways and watersheds, and changes in the population living in flood plains and other at risk areas, is difficult to ascertain (Pielke Jr., 1999). However, research studies that have investigated streamflow trends, rather than precipitation trends, have found increases in low to moderate streamflows and little overall changes in high streamflow (Lins and Slack, 1999; Douglas et al., 2000; McCabe and Wolock, 2002; Lins and Cohn, 2003)—the category associated with flooding events. This has been attributed to the seasonality of the observed increases in precipitation, which have been characterized by increases in autumn (the general time of low streamflow) and little change to spring precipitation (the general time of high streamflow) (Small et al., 2006).

Studies that have looked specifically at trends in annual peak streamflow find mixed results and inconsistent associations with atmospheric carbon dioxide levels or climate change (Villarini et al., 2009; Hirsch and Ryberg, 2012). Studies that examine trends in damage from flood events generally conclude that changes to population and wealth in vulnerable areas dominate over changes in the climate (Downton, et al., 2005; Changnon, 2003).

Observed climate complexity makes it difficult to identify any changed climate signal in flood trends. Such difficulties will persist into the future and likely worsen with increased development in flood prone regions.

If you don't believe us, here is what the IPCC SREX had to say:

> "There is limited to medium evidence available to assess climate-driven observed changes in the magnitude and frequency of floods at regional scales because the available instrumental records of floods at gauge stations are limited in space and time, and because of confounding effects of changes in land use and engineering. Furthermore, there is low agreement in this evidence, and thus overall low confidence at the global scale regarding even the sign of these changes."

References:


Changnon, S.A., 2003. Shifting economic impacts from weather extremes in the United States: A result of societal changes, not global warming. *Natural Hazards Review*, 29, 273-290.

Douglas, E.M., R.M. Vogel, and C.N. Kroll, 2000. Trends in floods and low flows in the United States: Impact of spatial correlation. *Journal of Hydrology*, 240, 90-105.

Downton, M.W., 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, doi:10.1061/(ASCE)1527-6988(2005)6:1(13).

Hirsch, R.M., and K.R. Ryberg, 2012. Has the magnitude of floods across the USA changed with global CO2 levels? *Hydrological Sciences Journal*, 57:1, 1-9, http://dx.doi.org/10.1080/02626667.2011.621895.

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

Lins, H.F., and T.A. Cohn, "Floods in the Green House: Spinning the Right Tail," in Thorndycraft, V.R., G. Benito, M. Barriendos, and M.C. Llasat, 2003: *Palaeofloods, Historical Floods and Climatic Variability: Applications in Flood Risk Assessment (Proceedings of the PHEFRA Workshop, Barcelona, 16-19 October 2002)*, http://www.ica.csic.es/dpts/suelos/hidro/images/chapter_40_phefra.pdf.

McCabe, G.J., and D.M.A. Wolock, 2002. A step increase in streamflow in the conterminous United States. *Geophysical Research Letters*, 29(24), 2185-2188.

Pielke, R.A., Jr., 1999. Nine fallacies of floods. *Climatic Change*, 42, 413-438.

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

Villarini, G., et al., 2009. On the stationarity of annual flood peaks in the continental United States during the 20th century. *Water Resources Research*, 45, W08417, doi:10.1029/2008WR007645.


**Page 56, lines 1-5**

Isn't it inappropriate to include the Figure from Hirsch and Ryberg, 2012 in support of your assertion that flood increases in some regions of the U.S. are related to human-caused climate change? Here is how Hirsch and Ryberg (2012) described their findings:

> "What these results do indicate is that except for the decreased flood magnitudes observed in the SW there is no strong empirical evidence in any of the other 3 regions for increases or decreases in flood magnitudes in the face of the 32% increase in GMCO2 that has taken place over the study period."

Perhaps you ought to make note of that.

Reference:

Hirsch, R.M. and K.R. Ryberg, 2012. Has the magnitude of floods across the USA changed with global $CO_2$ levels? *Hydrological Sciences Journal*, 57**,** 1-9 doi: 10 10.1080/02626667.2011.621895

**Page 56, lines 7-8**

You write that "Precipitation has already declined in some areas within the Southwest and the Rocky Mountain states"

What do you mean by "already?" Are you implying here that declines in "some areas of the Southwest and Rocky Mountain states" are due to anthropogenic climate changes? What about in "some" other areas of the Southwest and Rocky Mountain states where precipitation has increased (See your Fig. 2.11)? Are precipitation *increases* signs of anthropogenic climate changes? Or are "some" areas getting wetter due to natural variability while "some" nearby areas are getting drier because of AGW?!? If this this is the case, then as they say, extraordinary claims require extraordinary proof, so let's see it.

**Page 56, lines 8-10**

"…and decreases in precipitation are projected to intensify in those areas and spread northward and eastward in summer (see Key Message 5)."

Is the decrease in precipitation projected to "intensify" in those areas of the Southwest and Rocky Mountain states which are currently seeing precipitation increases (see Figure 2.11)? Do the climate models that you are relying on to project decreases in precipitation in the future accurately capture the current precipitation increases experienced in some portions of the Southwest and Rocky Mountain states (see Figure 2.11). Are you all relying on a "gut" feeling that climate models that do not capture 20th century behavior (in fact get the sign wrong in some regions of the Southwest and Rocky Mountain states, see Polson et al., 2013) will start producing accurate projections for the 21st century? Is the signal-to-noise ratio of precipitation in the Southwest and Rocky Mountain states and

areas "northward" and "eastward" in the summer large enough to identify the projected changes even if they were to happen within the projected timeframe?

Recommendation: These questions must be answered and the answered used to modify the existing NCA text.

**Page 56, lines 10-11**

"However, even in areas where precipitation does not decrease, projected higher air temperatures will cause increases in surface evaporation and loss of water from plants, leading to drier soils."

How many variables did you leave out in getting from "higher air temperatures" to "drier soils" (hint: see NCA page 110 lines, 9-14 and page 111 lines 1-15)? Did you consider higher atmospheric CO2 concentration improves the water use efficiency of plants? Did you consider changes to the wind speed, humidity, or radiation? See Sheffield et al., (2012) as to why you should.

Recommendation: Remove this gross oversimplification or else describe the complex situation more thoroughly, explaining that temperature increases do not necessarily lead to increased water loss.

Reference:

Sheffield, J., et al., 2012. Little change in global drought over the past 60 years. *Nature*, 491, 435-438, doi:10.1038/nature11575.

**Page 57, lines 1-12**

Admitting, as you do, that the PDSI "may overestimate the magnitude of drought increases" seems reason enough not to include Figure 2.21 and any other subsequent quantitative discussions of future drought change based on the PDSI.

Here is how Sheffield et al. (2012) described the continued use of the PDSI in assessing change in drought:

> Despite the long-standing consensus that the underlying science for temperature-based estimates of PE is flawed, compounded by the results of this and other studies that the flaws are manifested in errors in the estimations of the impact of warming on drought and hydrology in general, the reasons for the long and continued use of the PDSI_Th for climate studies in essentially its original form are a curiosity.

As it now stands, the NCA remains such a "curiosity."

Further, Hoerling et al., 2012 specifically point to the future drought values in Wehner et al. 2011—the source for Figure 2.21—as being too sensitive to changes in temperature and being "unreliable."

Recommendation: Remove Figure 2.21 as it is provides an "unreliable" projection of future drought in the U.S. and Mexico.

Reference:

Hoerling, M., et al., 2012. Is a Transition to Semi-Permanent Drought Conditions Imminent in the U.S. Great Plains? *Journal of Climate*, 25, 8380-8386, doi:10.1175/JCLI-D-12-00449.1.

Sheffield, J., et al., 2012. Little change in global drought over the past 60 years. *Nature*, 491, 435-438, doi:10.1038/nature11575.

**Page 59, lines 1-9**

After line 9, add something like the following:

"But whether or not any detectable human influence is ever identified for these types of extreme weather events, the largest factor behind increases in the impacts from these types of events is human demographic changes such as population size, wealth, and location (e.g., Pielke, Jr., 2008; Simmons et al., 2012)

References:

Pielke, Jr., R. A., Gratz, J., Landsea, C. W., Collins, D., Saunders, M. A., and Musulin, R., 2008. Normalized Hurricane Damages in the United States: 1900-2005. *Natural Hazards Review*, 9, 29-42.

Simmons, K.M., et al., 2012. Normalized tornado damage in the United States: 1950-2011. *Environmental Hazards*, http://dx.doi.org/10.1080/17477891.2012.738642

**Page 59, lines 17-40**

Why does this discussion only include data "since the 1970s"?

Why is there no discussion about the most important characteristics of Atlantic hurricanes for U.S. interests—the number and intensity of storms that make landfall along the U.S. coast?

Why is there no discussion of how human-caused climate changes may impact the number and intensity of hurricanes making landfall in the U.S.?

Are the taxpayers really getting their money's worth from the USGCRP when the NCA neglects any mention of the most important aspects for the future impacts of hurricanes in the in the U.S.?

Reliable data on U.S. landfalling storms pre-dates the 1970s and in fact, extends back into the 19th century. There is no overall trend in the number of landfalls (e.g., Wang and Lee, 2008; Pielke Jr., 2009; Villarini et al., 2010) nor is there a trend in the intensity of U.S. landfalling storms (Landsea et al., 2005 and updates). In fact, we are currently experiencing the longest trend since reliable records began between major hurricane landfalls (Wienkle et al., 2012).

And, despite the lack of mention in the NCA, there is a lot of evidence that suggests that AGW will act to *reduce* the frequency of landfalling hurricanes along the U.S. coasts.

For example, Murakami and Wang (2010) compared the tracks of Atlantic basin tropical cyclones generated from a high resolution general circulation model (MRI/JMA AGCM v3.1) for a 25-yr simulation of the present day with those of the future under the SRES A1B emissions scenario. They found a significant eastward shift in the tropical cyclone genesis region in the Atlantic Ocean. This eastward shift had the impact of decreasing the frequency of storms which tracked into the U.S. Southeast Atlantic and Gulf coasts and reducing the probability of landfall, while only slightly increasing the influence of tropical cyclones on the northeastern U.S. In follow-up work using a newer version of the high resolution climate model (MRI/JMA AGCM v3.2), Murakami et al. (2012), find that overall, the frequency of tropical cyclones approaching the U.S. coastline declines by nearly 20% while the average maximum intensity of storm approaching the coast increases by less than 0.5 m/s.

In other work, Wang et al. (2008) established that the size of the Atlantic Warm Pool (AWP) plays a strong role in hurricane activity in the Atlantic Ocean. The size of the AWP is influenced by annual-to-multiannual ENSO variability, the multi-decadal variability of the Atlantic Multidecadal Oscillation (AMO) and a general overall "global warming" (which leads to a larger AWP). Larger AWPs are associated with more intense Atlantic hurricanes. However, in a follow-up study, Wang et al. (2011) investigated the relationship between AWP size and U.S. landfalling hurricanes. Wang et al. (2011) found that while large AWPs were associated with more storms, large AWPs also altered atmospheric steering currents such the storms which did form had a tendency to recurve northwards and remain out to sea without making landfall in the U.S. Conversely, in years with small AWPs—a condition not favored by global warming—storms were steered more towards the southeastern U.S. Atlantic coast and Gulf of Mexico. Wang and Lee (2008) wrote:

> "A secular warming of sea surface temperature occurs almost everywhere over the global ocean. Here we use observational data to show that global

> warming of the sea surface is associated with a secular increase of tropospheric vertical wind shear in the main development region (MDR) for Atlantic hurricanes. The increased wind shear coincides with a weak but robust downward trend in U.S. landfalling hurricanes, a reliable measure of hurricanes over the long term."

The lack of an observed long-term trend in the frequency and/or magnitude of U.S. hurricane landfalls, and the recent lack of Category 3 or stronger U.S. landfalling hurricanes is consistent with these findings for a world with increased AGW.

Recommendation: It is either oversight or willful neglect that the topic of future U.S. hurricane landfalls under anthropogenic climate change was not covered in the NCA. In either case, it is imperative that this deficit be remedied.

References:


Landsea, C. W., 2005. Hurricanes and global warming. *Nature*, 438. doi:10.1038/nature04477.

Murakami, H., and B. Wang, 2010. Future Change of North Atlantic Tropical Cyclone Tracks: Projection by a 20-km-Mesh Global Atmospheric Model. *Journal of Climate*, 23, 2699–2721. doi:10.1175/2010JCLI3338.1.

Murakami, H., et al., 2012. Future Changes in Tropical Cyclone Activity Projected by the New High-Resolution MRI-AGCM. *Journal of Climate*, 25, 3237–3260. doi: 10.1175/JCLI-D-11-00415.1.

Pielke, R. A., Jr., 2009. United States hurricane landfalls and damages: Can one- to five-year predictions beat climatology? *Environmental Hazards*, 8, 187–200.

Villarini, G., G.A. Vecchi, and J.A. Smith, 2012: U.S. landfalling and North Atlantic hurricanes: statistical modeling of their frequencies and ratios. *Monthly Weather Review,* 140, 44-65.

Wang, C., and S.-K. Lee, 2008. Global warming and United States landfalling hurricanes, *Geophysical Research Letters*, 35, L02708, doi:10.1029/2007GL032396.

Wang, C.L., S-K. Lee, and D.B. Enfield, 2008. Atlantic Warm Pool acting as a link between Atlantic Multidecadal Oscillation and Atlantic tropical cyclone activity. *Geochemistry, Geophysics, Geosystems*, 9, Q05V03, doi:10.1029/2007GC001809.

Wang, C., L. Hailong, S-K. Lee, and R. Atlas, 2011. Impact of the Atlantic warm pool on United States landfalling hurricanes. *Geophysical Research Letters*, 38, L19702, doi:10.1029/2011GL049265.

Weinkle, J., R. Maue, and R. Pielke, Jr., 2012. Historical global tropical cyclone landfalls. *Journal of Climate*, doi:10.1175/JCLI-D-11-00719.1.

**Page 60, lines 24-27**

Screen and Simmons (2013) do not provide overwhelming support for the Francis and Vavrus theory about blocking events.

For example, from Screen and Simmonds (2013):

> "It is of interest to compare our results to those of [Vavrus and Francis, 2012; FV12] for the case of meridional amplitude over the NAmAtl region. We find statistically insignificant positive trends in all seasons, in contrast to the comparatively larger (and significant) increases in JAS and OND suggested by FV12. These differences appears to relate to the precise metric analysed and can be understood using the idealised example above. FV12 effectively measure the poleward shift of the most northerly point on the wave (marked by the red arrow in Figure 4c) which is larger than the change in meridional amplitude (difference between the two black arrows in Figure 4c). Thus, we argue that the observed changes in the meridional extent of planetary-wave meanders are smaller than those implied by FV12. However, both studies agree on the sign of the meridional amplitude trends over NAmAtl, if not their magnitude or statistical significance."

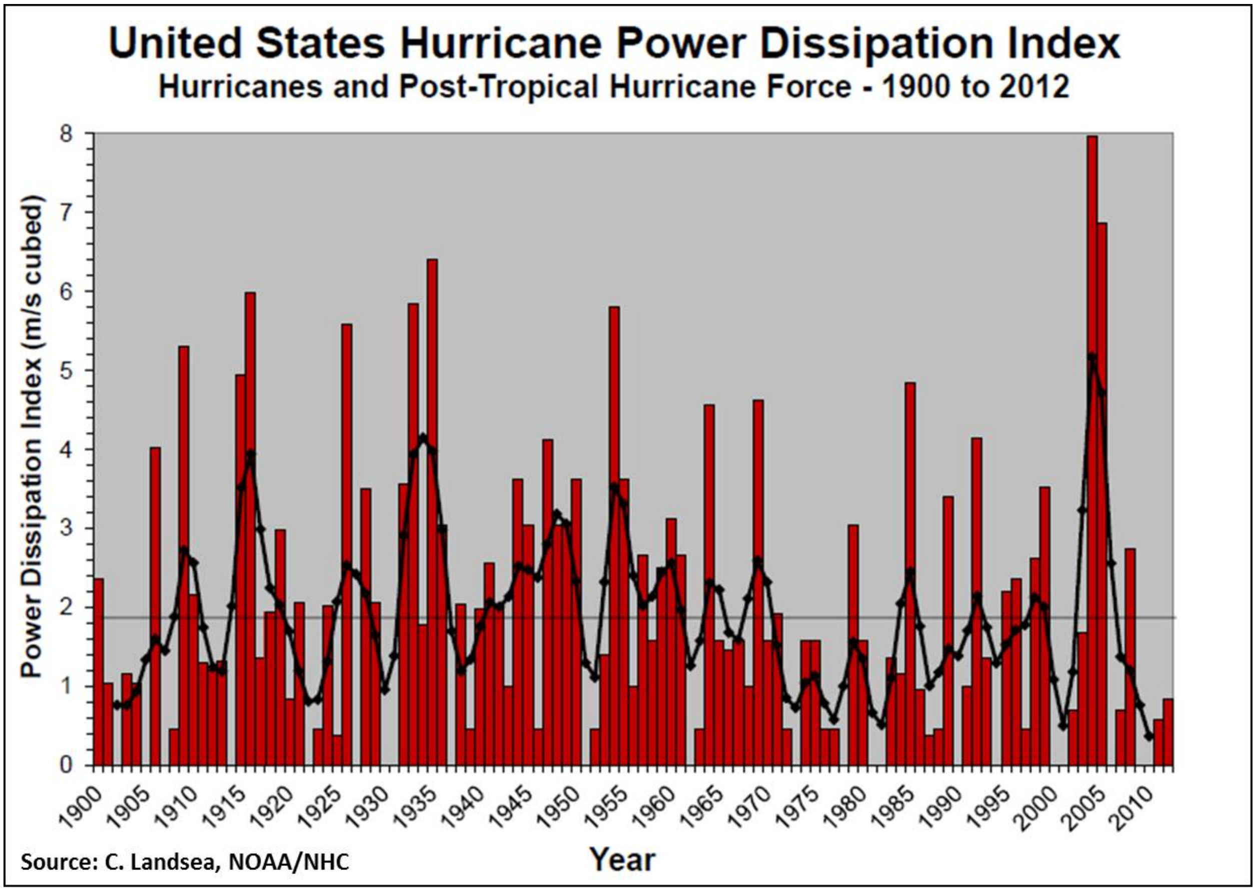
Recommendation: Clearly there is still a lot of work to be done on the topic, so I would suggest tempering your enthusiasm for the Francis and Vavrus result.

Reference:

Screen, J.A., and I Simmonds, 2013. Exploring links between Arctic amplification and mid-latitude weather. *Geophysical Research Letters*, doi: 10.1002/GRL.50174.

**Page 61, Figure 2.23**

Replace Figure 2.23 with one that is more germane to climate impacts in the United States—i.e. observed trend in hurricane intensity for U.S. landfalling hurricanes, shown below, or available here (http://rogerpielkejr.blogspot.com/2012/11/us-hurricane-intensity-1900-2012.html). Additionally, this figure does not suffer from the short -period bias that the current Figure 2.23 suffers from.




*CAPTION: Figure updated by C. Landsea from Landsea et al., 2005*

Reference:

Landsea, C. W., 2005. Hurricanes and global warming. *Nature*, 438, doi:10.1038/nature04477.

**Page 62, Figure 2.24**

This figure gives a misleading impression. Instead of showing changes in the percentage change in frequency of storms, show the projected change in the actual number of storms in each category along with the observed variability.  Also, include a figure showing the projected changes in the number of U.S. landfalling hurricanes in each category.

**Page 63, lines 11-14.**

"..faster still."

You mean the sea level rise during the short period (~20 years) satellite record is faster than the average tide gauge rise since 1880? That is hardly a fair comparison! You should compare the satellite rise with periods of similar length during in the tide gauge record. That would allow the reader to see the magnitude of the short-term variability in the sea level data set and judge the satellite-observed trend against this variability (for instance in the Church and White sea level dataset, there is a 20-yr period ending around 1930 during which the trend was greater than that currently observed in the satellite record).

References:

Church, J. A., and N. J. White, 2006. A 20th century acceleration in global sea-level rise, *Geophysical Research Letters*, 33, L01602, doi:10.1029/2005GL024826.

Nerem, R. S., D. Chambers, C. Choe, and G. T. Mitchum, 2010. Estimating Mean Sea Level Change from the TOPEX and Jason Altimeter Missions. *Marine Geodesy,* 33 (1), 435, and updates available at http://sealevel.colorado.edu/.

**Page 63, line 24.**

To the end of the sentence, "It is not clear, however, whether these statistical relationships will hold in the future" add the phrase "or that they are appropriate in modeling past behavior, thus calling their reliability into question (Gregory et al., 2012)."

Reference:

Gregory, J., et al., 2012. Twentieth-century global-mean sea-level rise: is the whole greater than the sum of the parts? *Journal of Climate*, doi:10.1175/JCLI-D-12-00319.1, in press.

Relevant excerpt from Gregory et al. (2012):

> "The implication of our closure of the [global mean sea level rise, GMSLR] budget is that a relationship between global climate change and the rate of GMSLR is weak or absent in the past. The lack of a strong relationship is consistent with the evidence from the tide-gauge datasets, whose authors find acceleration of GMSLR during the 20th century to be either insignificant or small. It also calls into question the basis of the semi-empirical methods for projecting GMSLR, which depend on calibrating a relationship between global climate change or radiative forcing and the rate of GMSLR from observational data (Rahmstorf, 2007; Vermeer and Rahmstorf, 2009; Jevrejeva et al., 2010)."

**Page 65, Figure 2.26**

Does the data in this figure square with the text on page 63, lines 15-17 stating "Even the most sophisticated climate models, which explicitly represent Earth's physical processes, cannot simulate recent rapid changes in ice sheet dynamics, and thus tend to underestimate sea level rise"?

Figure 2.26 makes it seem like the observed sea level rise falls well within the model bounds and doesn't obviously suggest that models "tend to underestimate sea level rise."

Perhaps the description on page 63 is inaccurate?

**Page 66 lines 6-**7

What is the significance of "since the early 1970s"? Is there no good Great Lakes ice coverage prior to then (e.g., Bai et al., 2012, show Great Lakes ice coverage back to 1963)? If not, then, since you tie ice loss to temperature, you could examine the regional winter temperature history prior to the early 1970s. The 1970s were a relative low point for the winter temperature in the Great Lakes region—thus making the change "since the early 1970s" seem larger than if the change were measured from average Lake ice conditions. Remember that 1976-7, 77-8, and 78-9 were the three coldest consecutive winters in the region in the entire NCDC record. So starting from near then will certainly give a big drop in ice coverage. Perhaps you could discuss the influence of ENSO and NAO on the Lake ice coverage (Bai et al., 2012)?

Reference:

Bai, X., et al., 2012. Interannual variability of Great Lakes ice cover and its relationship to NAO and ENSO. *Journal of Geophysical Research* 117, C03002, doi:10.1029/2010JC006932.

**Page 66, Figure 2.27**

What's the point of the two photos in the lower panel of Figure 2.27? Are you implying that Lake Superior was completely ice covered in all Marchs prior to 2003 and has lost all of its March ice cover in the past 10 years? Or are they just two random photos in time? Were there no Marchs prior to 2003 that had ice free conditions? What about 1987, or 1983, or 1964? What if you showed largely ice free conditions of Lake Superior in March 1987 and ice covered conditions in March 2003 with the caption "Satellite images show Lake Superior in a high ice year and an earlier low ice year." That would be accurate as well, correct? Are your cherries better than mine?

Recommendation: Remove the satellite photos and extend the Great Lakes ice coverage time series back into the early 1960s and include the mean ice coverage line (see Bai et al, 2012). Also include a time series of the Great Lakes basin winter temperature series back to 1895 to give the reader an idea of the character of the variability in the record.

Reference:

Bai, X., et al., 2012. Interannual variability of Great lakes ice cover and its relationship to NAO and ENSO. *Journal of Geophysical Research-Oceans*, 117, C03002, doi:10.1029/2010JC006932

**Page 67, lines 4-7**

The Francis and Vavrus stuff needs to be tempered by the recent findings from Screen and Simmonds (2013) that there are no significant changes in blocking in the North Atlantic sector. See our additional comment, on page 60, lines 24-27.

Reference:

Screen, J.A., and I Simmonds, 2013. Exploring links between Arctic amplification and mid-latitude weather. *Geophysical Research Letters*, doi: 10.1002/GRL.50174.

**Page 69, lines 1-5**

This section on Greenland ice loss is incomplete. For instance, no mention is made of the findings of Moon et al. (2012) which includes statements like (which leave a markedly different impression about future sea level rise than the NCA):

> "Finally, our observations have implications for recent work on sea level rise. Earlier research used a kinematic approach to estimate upper bounds of 0.8 to 2.0 m for 21st-century sea level rise. In Greenland, this work assumed ice-sheet–wide doubling of glacier speeds (low-end scenario) or an order of magnitude increase in speeds (high-end scenario) from 2000 to 2010. Our wide sampling of actual 2000 to 2010 changes shows that glacier acceleration across the ice sheet remains far below these estimates, suggesting that sea level rise associated with Greenland glacier dynamics remains well below the low-end scenario (9.3 cm by 2100) at present. Continued acceleration, however, may cause sea level rise to approach the low-end limit by this century's end."

Recommendation: Include a more thorough assessment of recent scientific findings concerning ice loss in Greenland which, together indicate that sustained period of rapid ice loss is unlikely and that the sea level contribution during the 21$^{st}$ century is likely to be small.

Reference:

Moon, T., I. Joughin, B. Smith, and I. Howat, 2012. 21st-century evolution of Greenland outlet glacier velocities. *Science*, 336, 576-578, doi:10.1126/science.1219985

**Page 69, lines 9-13**

We suggest you temper your methane findings—too many people are exaggerating the impact of methane releases from the Arctic (see http://www.realclimate.org/index.php?p=10412). You need to be careful not to find yourself among them.

**Page 70, lines 7-8**

"…and scientists are unsure whether and how quickly ocean life could adapt to such rapid acidification."

There is much more to the ocean acidification story being told in the NCADAC.
The scientific literature in this area has been expanding rapidly, and when evaluated in its entirety reveals a future that does not so full of negative impacts as the NCA implies. Negative outcomes are based primarily upon abiotic physical-chemical reactions that do not take account of the processes of life, which can greatly modify simply inorganic chemical processes. Many of the experiments examining this issue that actually do deal with living creatures are often of very short duration and do not account for longer-term adaptation, acclimation, or evolution.

Longer-term studies often demonstrate the ability of marine species to adapt to changing conditions. For example, a reconstruction of seawater pH spanning the period 1708-1988, based on the boron isotopic composition ($\delta^{11}$B) of a long-lived massive *Porites* coral from Flinders Reef in the western Coral Sea of the southwestern Pacific (Pelejero et al., 2005) indicated that there has been no notable trend toward lower $\delta^{\mathbf{11}}$B values over the 280-year period. Instead, they say "the dominant feature of the coral $\delta^{11}$B record is a clear interdecadal oscillation of pH, with $\delta^{11}$B values ranging between 23 and 25 per mil (7.9 and 8.2 pH units)." In addition, they calculated changes in aragonite saturation state from the Flinders pH record that varied between ~3 and 4.5, which values encompass, in their words, "the lower and upper limits of aragonite saturation state within which corals can survive." Yet in spite of this fact, they determined that "skeletal extension and calcification rates for the Flinders Reef coral fall within the normal range for *Porites* and are not correlated with aragonite saturation state or pH."

A study of historical calcification rates determined from coral cores retrieved from 35 sites on Australia's Great Barrier Reef, found that there was a statistically significant correlation between coral calcification rate and local water temperature, such that a 1°C

increase in mean annual water temperature increased mean annual coral calcification rate by about 3.5% (Lough et al., 1997). Nevertheless, it was reported that there were "declines in calcification in *Porites* on the Great Barrier Reef over recent decades." The researchers were quick to note, however, that their data depicted several extended periods of time when coral growth rates were either above or below the long-term mean, cautioning that "it would be unwise to rely on short-term values (say averages over less than 30 years) to assess mean conditions." Notably, they reported that "a decline in calcification equivalent to the recent decline occurred earlier this century and much greater declines occurred in the 18th and 19th centuries," long before anthropogenic $CO_2$ emissions made a significant impact on the air's $CO_2$ concentration. In fact, the researchers report that "the 20th century has witnessed the second highest period of above average calcification in the past 237 years."

Similar findings were reported by another research team that reconstructed a history of coral calcification rates from a core extracted from a massive *Porites* coral on the French Polynesian island of Moorea that covered the period 1801-1990 (Bessat and Buigues, 2001). They performed this work, they wrote, because "recent coral-growth models highlight the enhanced greenhouse effect on the decrease of calcification rate," as well as the similarly projected negative effect of $CO_2$-induced ocean acidification on calcification rate; and rather than relying on theoretical calculations, they wanted to work with real-world data, stating that the records preserved in ancient corals "may provide information about long-term variability in the performance of coral reefs, allowing unnatural changes to be distinguished from natural variability." Similar to other studies, they found that a 1°C increase in water temperature increased coral calcification rate at the site they studied by 4.5%, which result stands in stark contrast to the 6-14% decline in calcification that had earlier been computed should have occurred over the past 100 years, based solely on physical-chemical considerations (Kleypas et al., 1999). In addition, they observed patterns of "jumps or stages" in the record, which were characterized by an increase in the annual rate of calcification, particularly at the beginning of the past century "and in a more marked way around 1940, 1960 and 1976," stating once again that their results "do not confirm" those predicted by the purely physical-chemical model upon which the ocean acidification hypothesis is ultimately based.

In another study devoted to corals that involves a much longer period of time another research team determined the original growth rates of long-dead Quaternary corals found in limestone deposits of islands in the Wakatobi Marine National Park of Indonesia, after which they compared them to the growth rates of present-day corals of the same genera living in the same area ( et al., 2006).This work revealed that the Quaternary corals grew in a comparable environment to modern reefs -- except, of course, for the air's $CO_2$ concentration, which is currently higher than it has been at any other time throughout the entire Quaternary, which spans the past 1.8 million years. Most interestingly, therefore, their measurements indicated that the radial growth rates of the modern corals were 31% greater than those of their ancient predecessors in the case of *Porites* species, and *34%* greater in the case of *Favites* species. Similar findings are ubiquitous, showing increasing rates of coral calcification in the face of rising temperatures and atmospheric $CO_2$

concentrations (e.g. Clausen and Roth, 1975; Coles and Jokiel, 1977; Kajiwara et al., 1995; Nie et al., 1997; Reynaud-Vaganay et al., 1999; Reynaud et al., 2007).

Recommendation: The ocean acidification sections needs to be reworked so as to reflect more long-term real-world studies that indicate that marine species are not as susceptible to ocean acidification as is implied in the current NCA.

References:

Bessat, F. and Buigues, D. 2001. Two centuries of variation in coral growth in a massive *Porites* colony from Moorea (French Polynesia): a response of ocean-atmosphere variability from south central Pacific. *Palaeogeography, Palaeoclimatology, Palaeoecology,* 175, 381-392.

Clausen, C.D. and Roth, A.A. 1975. Effect of temperature and temperature adaptation on calcification rate in the hematypic *Pocillopora damicornis*. *Coral Reefs,* 33, 93-100.

Coles, S.L. and Jokiel, P.L. 1977. Effects of temperature on photosynthesis and respiration in hermatypic corals. *Coral Reefs,* 43, 209-216.

Crabbe, M.J.C., Wilson, M.E.J. and Smith, D.J. 2006. Quaternary corals from reefs in the Wakatobi Marine National Park, SE Sulawesi, Indonesia, show similar growth rates to modern corals from the same area. *Journal of Quaternary Science,* 21, 803-809.

Kajiwara, K., Nagai, A. and Ueno, S. 1995. Examination of the effect of temperature, light intensity and zooxanthellae concentration on calcification and photosynthesis of scleractinian coral *Acropora pulchra*. *Journal of the School of Marine Science and Technology,* 40, 95-103.

Kleypas, J.A., Buddemeier, R.W., Archer, D., Gattuso, J-P., Langdon, C. and Opdyke, B.N. 1999. Geochemical consequences of increased atmospheric carbon dioxide on coral reefs. *Science,* 284, 118-120.

Lough, J.M. and Barnes, D.J. 1997. Several centuries of variation in skeletal extension, density and calcification in massive *Porites* colonies from the Great Barrier Reef: A proxy for seawater temperature and a background of variability against which to identify unnatural change. *Journal of Experimental and Marine Biology and Ecology,* 211, 29-67.

Nie, B., Chen, T., Liang, M., Wang, Y., Zhong, J. and Zhu, Y. 1997. Relationship between coral growth rate and sea surface temperature in the northern part of South China Sea. *Science in China Series D,* 40, 173-182.

Pelejero, C., Calvo, E., McCulloch, M.T., Marshall, J.F., Gagan, M.K., Lough, J.M. and Opdyke, B.N. 2005. Preindustrial to modern interdecadal variability in coral reef pH. *Science,* 309, 2204-2207.

Reynaud-Vaganay, S., Gattuso, J.P., Cuif, J.P., Jaubert, J. and Juillet-Leclerc, A. 1999. A novel culture technique for scleractinian corals: Application to investigate changes in skeletal $\delta^{18}O$ as a function of temperature. *Marine Ecology Progress Series,* 180, 121-130.

Reynaud, S., Ferrier-Pages, C., Meibom, A., Mostefaoui, S., Mortlock, R., Fairbanks, R. and Allemand, D. 2007. Light and temperature effects on Sr/Ca and Mg/Ca ratios in the scleractinian coral *Acropora* sp. *Geochimica et Cosmochimica Acta,* 71, 354-362.

## Chapter 3 Water Resources

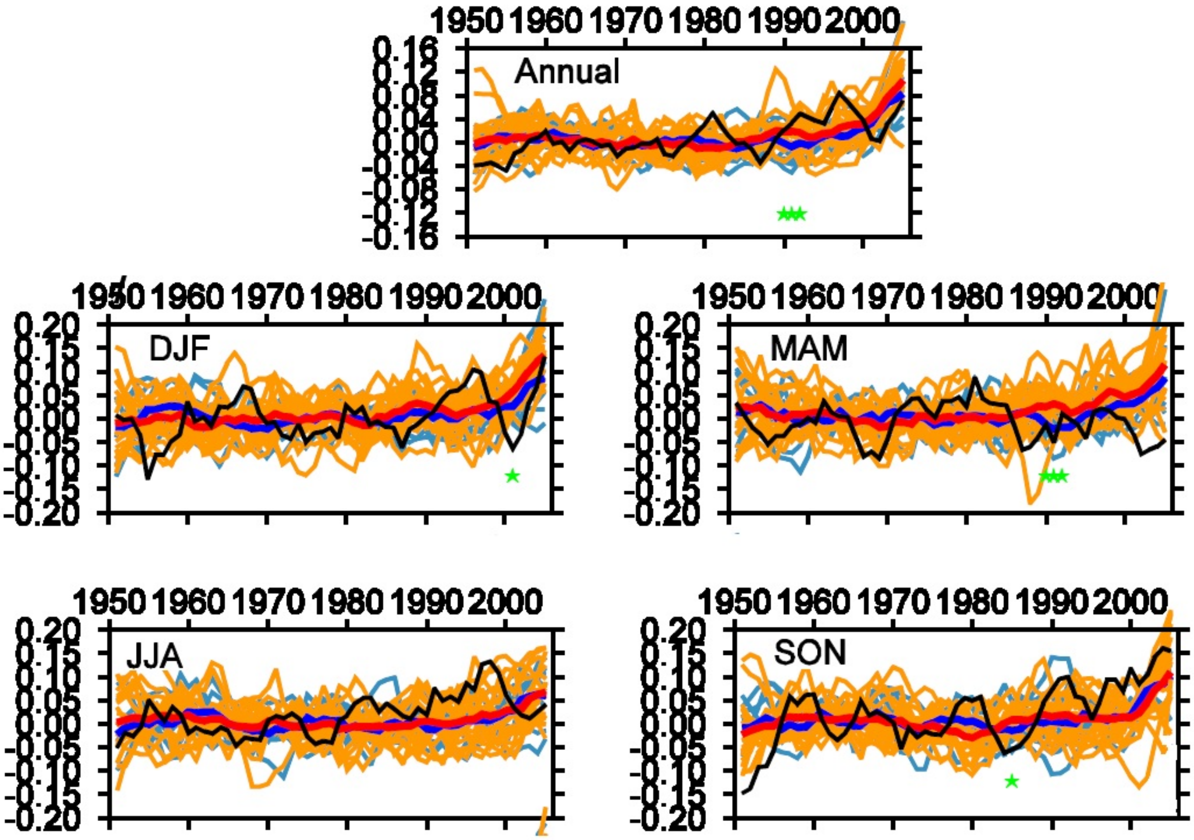
### General Comment

While Zhang et al. (2007) concluded globally that they had detected an anthropogenic influence on the overall latitudinal patterns of precipitation trends (although not in the magnitude of the trends) using the CMIP 3 models, in the latitude band that included the majority of the United States population, a mismatch between model projections and precipitation trends was found (model predicted a downwards trend while observations show and upwards trend).

*CAPTION: 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).*

More recently, Sarojini et al. (2012) examined the performance of the CMIP5 models and found much the same as Zhang et al. (2007). Of most relevance to the NCA, Sarojini et al. (2012) found little if any AGW signal in the observed annual of seasonal precipitation trends in land areas in the latitude band which contains the majority of the U.S.

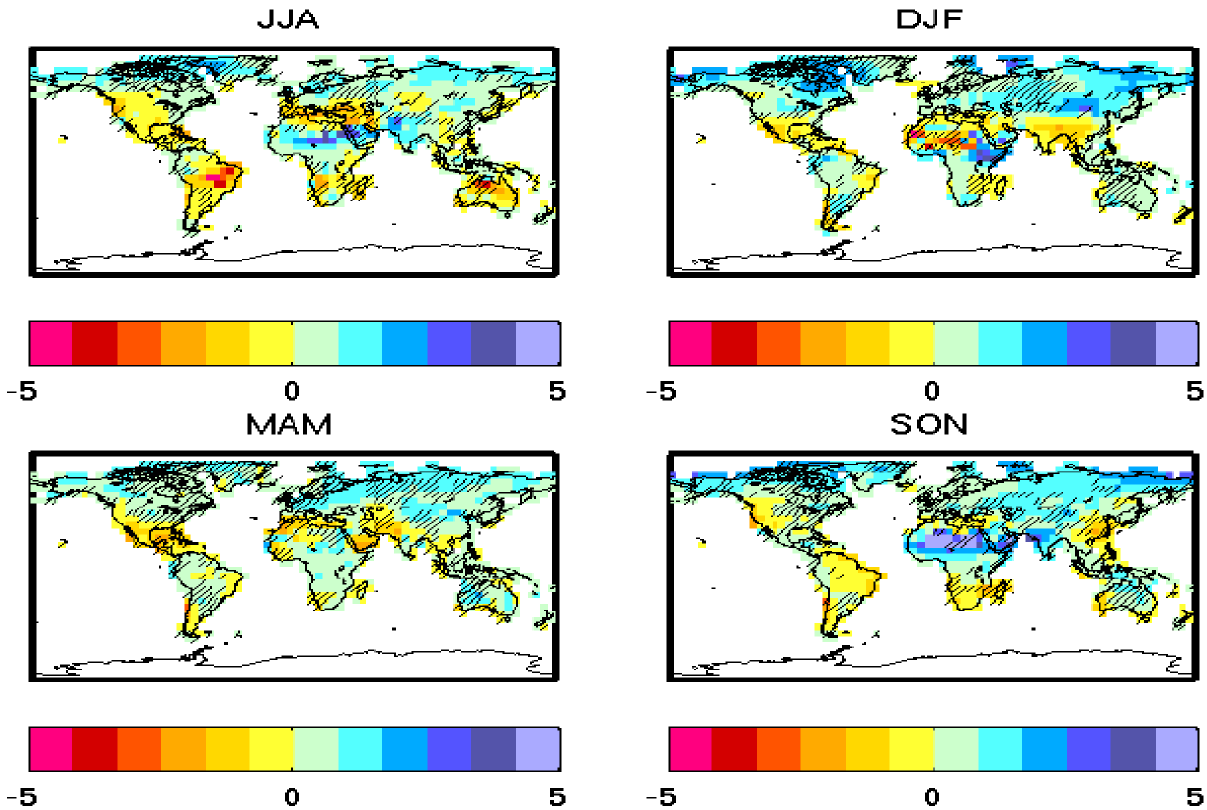



*CAPTION: Precipitation anomalies in the 30N to 60N latitude band from climate models (masked to match observations) driven by natural (blue) and natural+anthropogenic (red) forcings. Observed changes are in black. Green stars indicate times when there is a statistically significant difference between the NAT and the ALL forcings. Green stars are largely absent indicating that there is no statistically significant difference between NAT and ALL forcings in the modeled precipitation history (1950-2005) of the latitude band which contains the much of the U.S. (Sarojini et al., 2012).*

The Zhang et al. (2007) results have a high degree of internal consistency. Where large increases (high northern latitudes and southern tropics) or large decreases (tropics) are predicted, the signs of the modeled and observed trends are in agreement. In the intermediate zones, where projected changes are small, the signs are of the two are different.

Sarojini et al. (2012) find that the natural influence on precipitation over land areas with observations in the 30 to 60N latitude band is indistinguishable from the natural+anthopogenic signal indicating that anthropogenic forcing has played no detectable role in the evolution of seasonal precipitation in this latitude band either in seasonal or annual totals.

Obviously, forecasts that differ in sign from what is observed, or are indistinguishable from natural changes possess no skill and should not be used in a National Assessment. And while Zhang et al. (2007) and Sarojini et al. (2012) looked only at latitude bands, Polson et al. 2013 showed more specific results. Across the U.S., Polson et al. (2013) find that for most of the country during all seasons, that the sign of the observed precipitation changes (since 1950) differ from the sign of the climate model projected changes over the same period.




*CAPTION: Percentage change in precipitation per decade for the ALL forced multi-model mean for 1951- 2005 for DJF, MAM, JJA and SON. Hatched grid-boxes show where the sign of the change is consistent across all four observation datasets and the multi-model mean. Note the smaller scale of change patterns as multi-model mean changes show a much reduced influence of internal climate variability. (source: Polson et al., 2013)*

It is impossible to present reliable future projections for precipitations changes across the U.S. (seasonal or annual) from a collection of climate models which largely cannot even get the sign (much less the magnitude) of observed changed correct.

As a consequence, unless/until the GCMs can be demonstrated to accurately capture observed characteristics (spatial and temporal patterns and magnitudes) of precipitation changes across the U.S., all discussion from the NCA concerning future patterns of precipitation change should be removed.

References:

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013: Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

Sarojini, B.B., et al., 2012. Fingerprints of changes in annual and seasonal precipitation from CMIP5 models over land and ocean. *Geophysical Research Letters*, 39, L21706, doi: 10.1029/2012GL053373.

Zhang, X., et al., 2007. Detection of human influence on twentieth-century precipitation trends, *Nature*, 448, 461–465, doi:10.1038/nature06025.

**Specific Comments**

**Page 107, lines 13-14.**

The quote from Cousteau immediately reveals the political bias of the editors and writers. Try to be a *little* subtle and leave it out.

**Page 107, lines 18-20**

**Key Message 1**

There is general mismatch between the seasonal/annual patterns of observed precipitation changes and climate model expectations of those same changes across the U.S. during the past 50 to 100 years (see our General Comment of this Chapter for more detail) (Zhang et al., 2007; Sarojini et al., 2012; Polson et al., 2013). Given this mismatch, it is imprudent to discuss projections of future changes to the seasonal/annual pattern of precipitation change made by these very same climate models.

References:

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

Sarojini, B.B., et al., 2012. Fingerprints of changes in annual and seasonal precipitation from CMIP5 models over land and ocean. *Geophysical Research Letters*, 39, L21706, doi: 10.1029/2012GL053373.

Zhang, X., et al., 2007. Detection of human influence on twentieth-century precipitation trends, *Nature*, 448, 461–465, doi:10.1038/nature06025.

**Page 107, lines 21-23**

**Key Message 2**

Projections of summer droughts are typically too sensitive to projected changes in temperature (Hoerling et al., 2012; Sheffield et al., 2012), so care must be taken to insure that the techniques used in such drought projections do not suffer from such over sensitivity.

Further, seasonal projections of precipitation changes across the U.S. made by general circulation models are unreliable (Zhang et al., 2007; Sarojini et al., 2012; Polson et al., 2013) and regional climate models with higher spatial resolution indicate less drying in the Southwest than coarse resolution GCMs (Gao et al., 2011; Gao et al., 2012).

References:

Gao, Y., J. Vano, C. Zhu, and D. P. Lettenmaier. 2011. Evaluating climate change over the Colorado River basin using regional climate models. *Journal of Geophysical Research* 116, D13104, doi:10.1029/2010JD015278.

Gao, Y., et al. 2012. Moisture flux convergence in regional and global climate models: Implications for drought in the southwestern United States under climate change. *Geophysical Research Letters* 39, L09711, doi:10.1029/2012GL051560.

Hoerling, M., et al., 2012. Is a Transition to Semi-Permanent Drought Conditions Imminent in the U.S. Great Plains? *Journal of Climate*, 25, 8380-8386, doi:10.1175/JCLI-D-12-00449.1.

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

Sarojini, B.B., et al., 2012. Fingerprints of changes in annual and seasonal precipitation from CMIP5 models over land and ocean. *Geophysical Research Letters*, 39, L21706, doi: 10.1029/2012GL053373.

Sheffield, J., et al., 2012. Little change in global drought over the past 60 years. *Nature*, 491, 435-438, doi:10.1038/nature11575.

Zhang, X., et al., 2007. Detection of human influence on twentieth-century precipitation trends, *Nature*, 448, 461–465, doi:10.1038/nature06025.

**Page 107, lines 24-26**

**Key Message 3**

There is general mismatch between the seasonal/annual patterns of observed precipitation changes and climate model expectations of those same changes across the U.S. during the past 50 to 100 years (Zhang et al., 2007; Sarojini et al., 2012; Polson et al., 2013).

Given this mismatch, it is imprudent to discuss projections of future changes to the seasonal/annual pattern of precipitation change made by these very same climate models.

Further, floods are impacted by many factors besides climate or climate change. And the influence of these other factors must be accounted for before projections of climate change impacts are discussed (e.g. Pielke Jr., 1999).

References:

Pielke, R.A., Jr., 1999: Nine fallacies of floods. *Climatic Change*, 42, 413-438.

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

Sarojini, B.B., et al., 2012. Fingerprints of changes in annual and seasonal precipitation from CMIP5 models over land and ocean. *Geophysical Research Letters*, 39, L21706, doi: 10.1029/2012GL053373.

Zhang, X., et al., 2007. Detection of human influence on twentieth-century precipitation trends, *Nature*, 448, 461–465, doi:10.1038/nature06025.

**Page 107, lines 27-29**

**Key Message 4**

"Expected" precipitation changes are derived from simulations of precipitation and precipitation-related variables for the future. In science, "change" means *a significant difference*. So it is worthwhile asking, at what future (or present) point are changes observed or projected to appear? To help answer that question for you (since you all did not address it) we have prepared a Table showing the number of years before your projected changes in precipitation rise above the level of background natural variability.

(see our Comment for Page 43, lines 3-7). Please refer to that Table in your discussions of “expected” changes.

Also, there are no “expected” changes in “land use” detailed in this chapter. Text needs to be added in the appropriate place rather than just including some vague reference to other sections of the report.

Concern exists that groundwater resources in the United States will be negatively impacted by climate change because of increased drought and reduced recharge, increased pumping to keep up with increased potential evapotranspiration rates, and even the intrusion of brackish water related to ongoing sea level rise. There is no question that coherence exists between climate variables and groundwater resources with the interactions occurring at a variety of timescales (Ghanbari and Brave, 2010). Some scientists have found that climate change will be amplified in terms of groundwater response (Ng et al., 2010), while others have found only small responses to climate change scenarios (Scibek et al., 2007). Most investigators agree that groundwater response will strongly depend on local soils, land cover, geology, topography, regional climate, and existing groundwater conditions. Furthermore, groundwater may also respond to a decrease in transpiration from plants associated with elevated atmospheric carbon dioxide concentrations.

There is little doubt that groundwater resources of the future will be far more related to human management strategies than by changes in climate. Given the natural variability in climate, the complex response of groundwater to variations in climate, and the enormous impact on groundwater from pumping, groundwater impacts related to human-induced climate change will likely be undetectable for many decades to come (Hulme et al., 1999).

References:

Ghanbari, R.N. and H.R. Bravo, 2010. Coherence among climate signals, precipitation, and groundwater. *Ground Water*, 49, 476–490.

Hulme, M., E.M. Barrow, N.W. Arnell, P.A. Harrison, T.C. Johns, and T.E. Downing, 1999. Relative impacts of human-induced climate change and natural climate variability. *Nature,* 397, 688-691.

Ng, G.-H. C., D. McLaughlin, D. Entekhabi, and B. R. Scanlon, 2010. Probabilistic analysis of the effects of climate change on groundwater recharge. *Water Resources Research*, 46, W07502, doi:10.1029/2009WR007904.

Scibek, J., D.M. Allen, A.J. Cannon, P.H. Whitfield, 2007. Groundwater–surface water interaction under scenarios of climate change using a high-resolution transient groundwater model. *Journal of Hydrology,* 333, 165–181.

**Page 107, lines 30-32**

**Key Message 5**

Note on sea level rise: According to the IPCC *Fourth Assessment Report*, "Global average sea level rose at an average rate of 1.8 [1.3 to 2.3] mm per year over 1961 to 2003. The rate was faster over 1993 to 2003: about 3.1 [2.4 to 3.8] mm per year. Whether the faster rate for 1993 to 2003 reflects decadal variability or an increase in the longer-term trend is unclear." An update to the satellite sea level rise record (Nerem et al., 2010) through 2012 shows that the decadal rate of sea level rise has been slowing. From 2002 to 2012 the rate of sea level rise was 2.7 mm per year (0.09 inches per year). This slowdown in the rate of global sea level rise suggests that the faster rate of rise noted by the IPCC from 1993 to 2003 was influenced in part by short-term natural variability characteristic of the 20$^{th}$ century sea level rise record, rather than a full indication of the increase in the long-term rate of sea level rise.

The current rate of sea level rise, 2.7 mm per year, is equivalent to approximately 1 inch per decade—a rate which adaptive and protective responses can keep up with.

Note with regard to coastal wetlands: Some of the most extensive wetlands in the U.S are located on the northern Gulf Coast, where sea level rise from non-climatic processes greatly exceeds that from climate change. Consequently, any attempts to mitigate climate change by emissions reductions repeatedly referred to in the overall report will have little effect.

The Gulf Coast is a region which has been experiencing a long-term relative sea level rise that is some 2 to 5 times greater than that of the global average rate of sea level. The rate of relative sea level rise recorded at Galveston, Texas since 1908 has averaged 6.39 mm per year (2.1 feet per century). At Grand Isle, Louisiana the relative level rise has averaged 9.24 mm per year (3.03 feet per century) since measurements began there in 1947. The global average sea level rise during the 20$^{th}$ century has averaged 1.8 mm per year (0.59 feet per century), indicating that the bulk of relative sea level rise being felt in this region is from land subsidence which is not likely to be mitigated by emissions restrictions.

Reference:

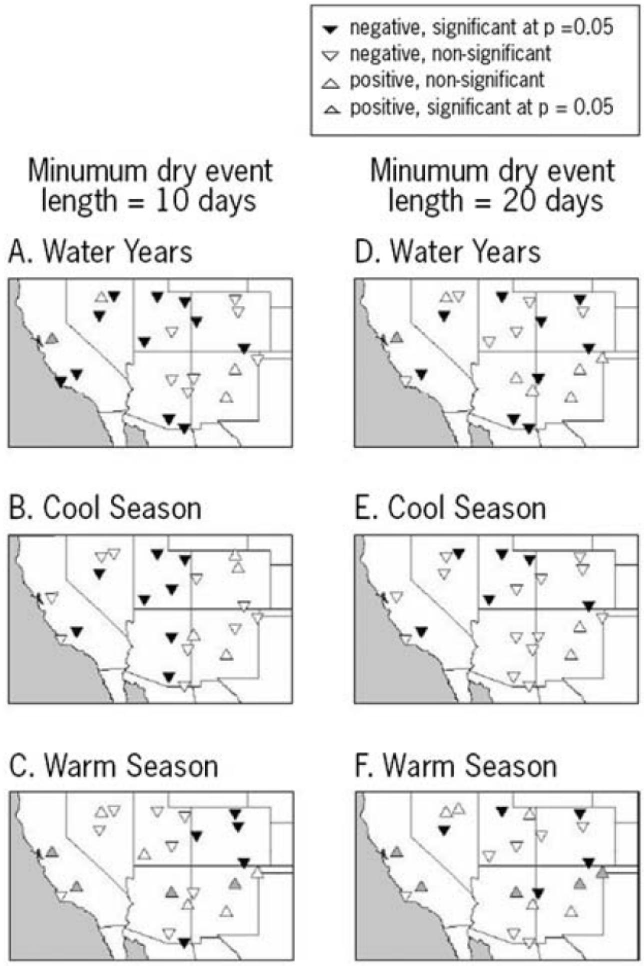
Nerem, R. S., D. Chambers, C. Choe, and G. T. Mitchum, 2010. Estimating Mean Sea Level Change from the TOPEX and Jason Altimeter Missions. *Marine Geodesy,* 33 (1 ), 435, and updates available at http://sealevel.colorado.edu/

**Page 108, lines 2-6**

**Key Message 7**

This statement is woefully incomplete. Whatever changes are occurring are largely driven by consumption. As NCA Figure 2.11 shows, precipitation in recent decades is *higher* in much of the Southwest, the Southeast, and the Great Plains, while it is lower in Hawaii. Nor has the temperature changed enough in the SE and GP regions to counter the precipitation increases.

In the Southwestern United States, where dry periods have been a concern since 2000, the differences between water use and supply is greater than in any other region of the United States. Despite the recent dryness, a large majority of reporting stations in the region exhibit negative trends in dry event length; i.e., the time interval between precipitation events has generally been declining since 1951. Not surprisingly, much of the variability in dry event length is related to El Niño variability, which is strongly coupled to southwestern U.S. precipitation.




*CAPTION: In general, the length of dry events have been declining in the western U.S. (downward triangles) (from McCabe et al., 2010).*

This key finding and the associated text needs to contain the following text: "The majority of these water supply changes are driven by consumption and withdrawal, as precipitation has generally increased, with the exception of a significant drop in Hawaii".

References:

Lins, H. F., and E. Z. Stakhiv, 1998. Managing the nation's water in a changing climate. *Journal of the American Water Resources Association*, 34, 1255–1264, doi:10.1111/j.1752-1688.1998.tb05429.x.

McCabe, G. J., et al., 2010. Variability and trends in dry day frequency and dry event length in the southwestern United States. *Journal of Geophysical Research*, 115, D07108, doi:10.1029/2009JD012866.

**Page 108, lines 7-8**

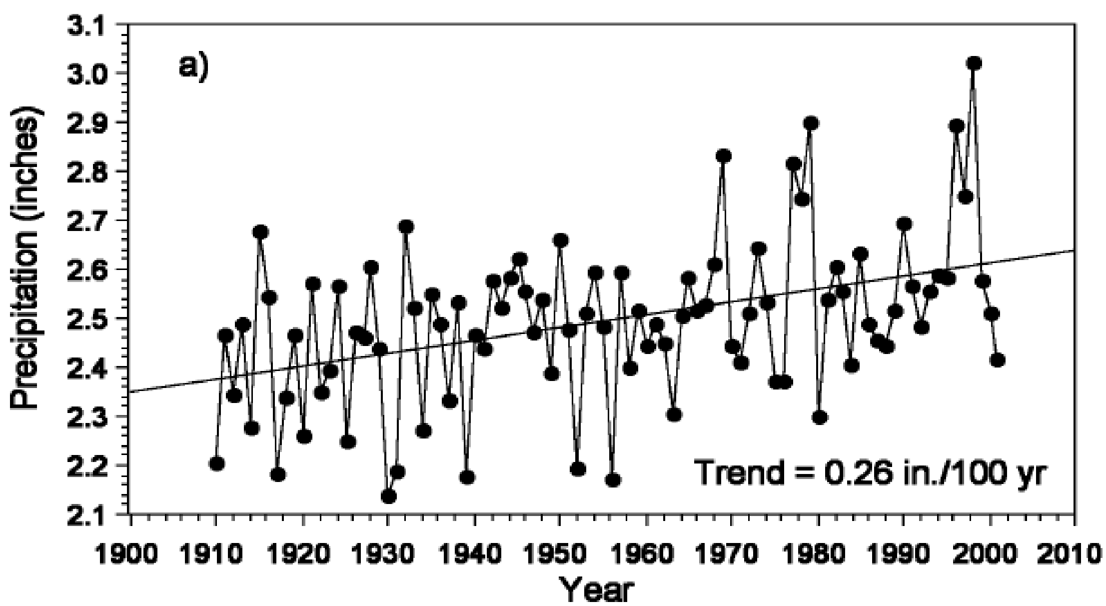
**Key Message 8**

The draft Assessment often conflates the *existence* of a change with a bad outcome from change. The statement "Increasing flooding risk affects……in the U.S." is meaningless unless "affects" is quantified, and there is a very extensive literature, centered on Pielke Jr. and Sr., but also on Chagnon Sr. and Jr., demonstrating little if any net cost increase after allowing for inflation and property value changes.

From Changnon et al., (1997):

> Frequent and extremely damaging severe weather conditions in the United States during 1991–94 caused $40 billion in insured losses, creating major impacts and eliciting diverse responses in the weather insurance industry. Population, one reason for the growing national sensitivity to storm damage, explained much of the increase in the number of catastrophes (property losses > $10 million) as well as the increases in the amount of losses. The largest increases in storms occurred in areas experiencing the greatest population growth (west, southwest, south, and southeast). Shifts in atmospheric variables (particularly in the frequency of extratropical cyclones) explained most of the 1949–94 fluctuations found in the intensity of catastrophic storms (losses divided by storm frequency)…

Nationally, there has been a significant increase in rainfall measured on the heaviest precipitation-day of the year, but the magnitude is very small—about 0.26 inches in a century (Michaels et al., 2004). While this trend is statistically significant, it is so small as to likely be operationally unimportant.




*CAPTION: Annual average precipitation amount falling on the wettest day of the year across the U.S. (source: Michaels et al., 2004).*

References:

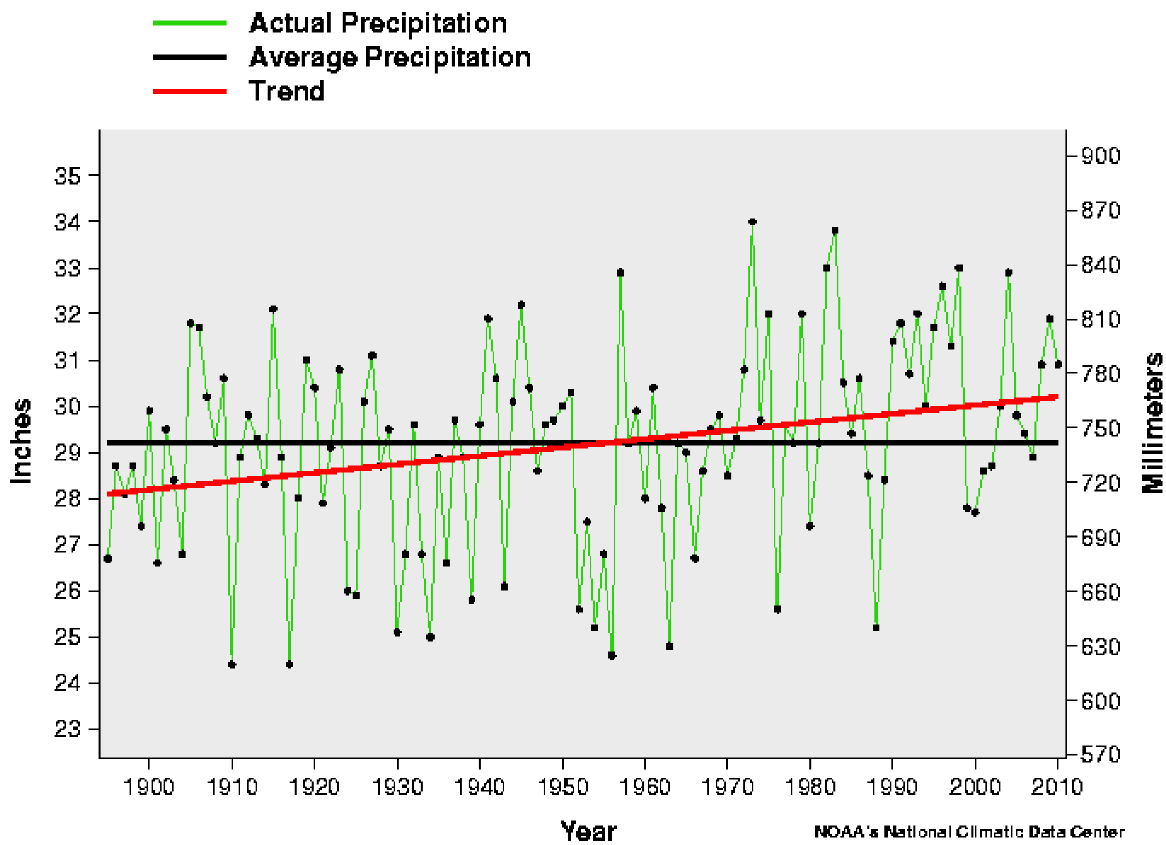
Changnon, S.A., et al., 1997. Effects of Recent Weather Extremes on the Insurance Industry: Major Implications for the Atmospheric Sciences. *Bulletin of the American Meteorological Society*, 78, 425-435.

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.

**Page 108, lines 29-35**

With respect to annual precipitation in the U.S.:

The statement about "recent decades" is true but neglects the fact that the 7% increase observed nationally does not really show an acceleration in recent decades, but instead reflects a situation similar to what has occurred with precipitation on the heaviest day of the year (Michaels et al., 2004).




*CAPTION: Average annual precipitation averaged across the U.S. (source: NCDC).*

**Page 108, lines 34-35**

"Increases in the north and decreases in the Southwest are projected to continue in this century (Orlowsky and Seneviratne 2012)."

What about those place in the Southwest which have seen precipitation increases (See NCA Figure 2.11). Are the increases in those places due to AGW or natural variability? Are the decreases in other locales in the Southwest due to AGW or natural variability? Do climate models get the pattern correct? Will the climate models get the pattern correct 10 years from now? 50 years? 100 years? How do you know?

It is imperative that you quantify natural variability and show the model projections against that natural variability (after that is, you have demonstrated the ability for the climate models to accurately capture the temporal and spatial history of precipitation patterns across the U.S.—a demonstration which is currently lacking in the NCA).

**Page 108, lines 37-38**

"Further, the volume of precipitation from the heaviest daily events has increased across the U.S."

This is true. In fact, this was demonstrated in Michaels et al., 2004. What was also demonstrated in Michaels et al., 2004 was that when the volume of precipitation delivered on wettest days of the year was divided by the total annual volume of precipitation, that there was no overall increase in the percentage of precipitation falling on the wettest days of the year. The basic conclusion is that as precipitation increases, so too does the amount falling in heavy events. Unless you all provide a cost-benefits analysis that shows that in net, the observed precipitation changes are negative (which is probably going to be challenging since there is no climate change signal in flood damages, for example, according to the IPCC SREX "The absence of an attributable climate change signal in losses also holds for flood losses")—arguably the biggest source of negative impacts from additional precipitation—then you should stop overemphasizing the data on heavy precipitation amounts. Fine, heavy events are increasing. So what? It turns out that it is thus far impossible to identify, much less demonstrate, that they result in a net negative when all effects of precipitation changes are considered.

Reference:

IPCC, 2012.*Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation*. A Special Report of Working Groups I and II of the Intergovernmental Panel on Climate Change [Field, C.B., V. Barros, T.F. Stocker, D. Qin, D.J. Dokken, K.L. Ebi, M.D. Mastrandrea, K.J. Mach, G.-K. Plattner, S.K. Allen, M. Tignor, and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, UK, and New York, NY, USA, 582 pp.

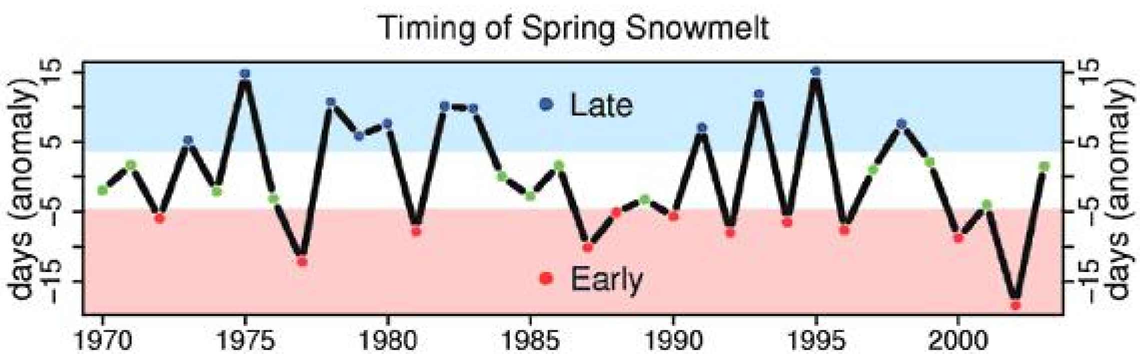
Michaels, P.J., et al., 2004. Trends in precipitation on the wettest days of the year across the contiguous USA. *International Journal of Climatology*, 24, 1873-1882.

**Page 109, lines 7-10**

Incomplete literature.

This brief section is a "high-visibility" aspect of U.S. climate change and should therefore be much more complete in its literature citations.

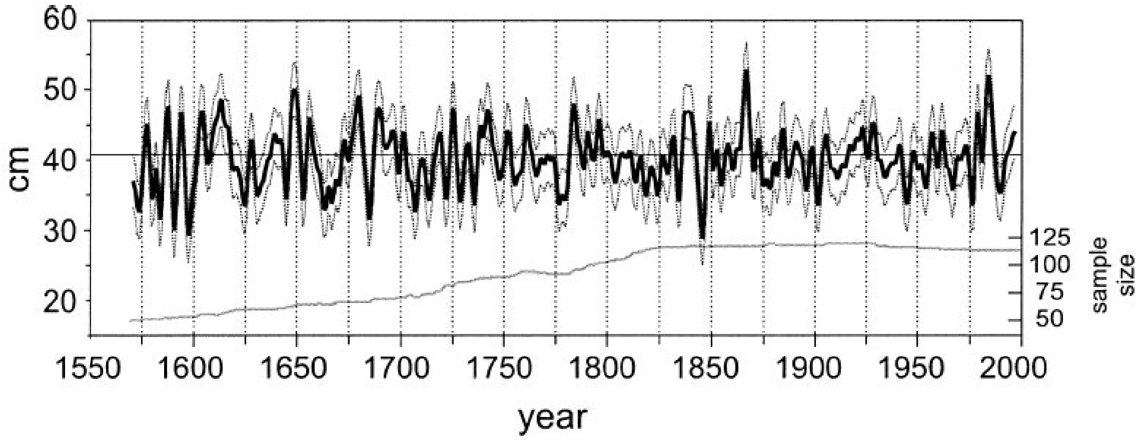
In a paper relating wildfire to snowmelt, Westerling et al. (2006), showed considerable year to year variability in the timing of snowmelt. They also found no significant trend whatsoever over the past four decades.




*CAPTION: Timing of spring snowmelt (the more negative the value, the earlier in the year the spring snowmelt occurred) (source: Westerling et al., 2006).*

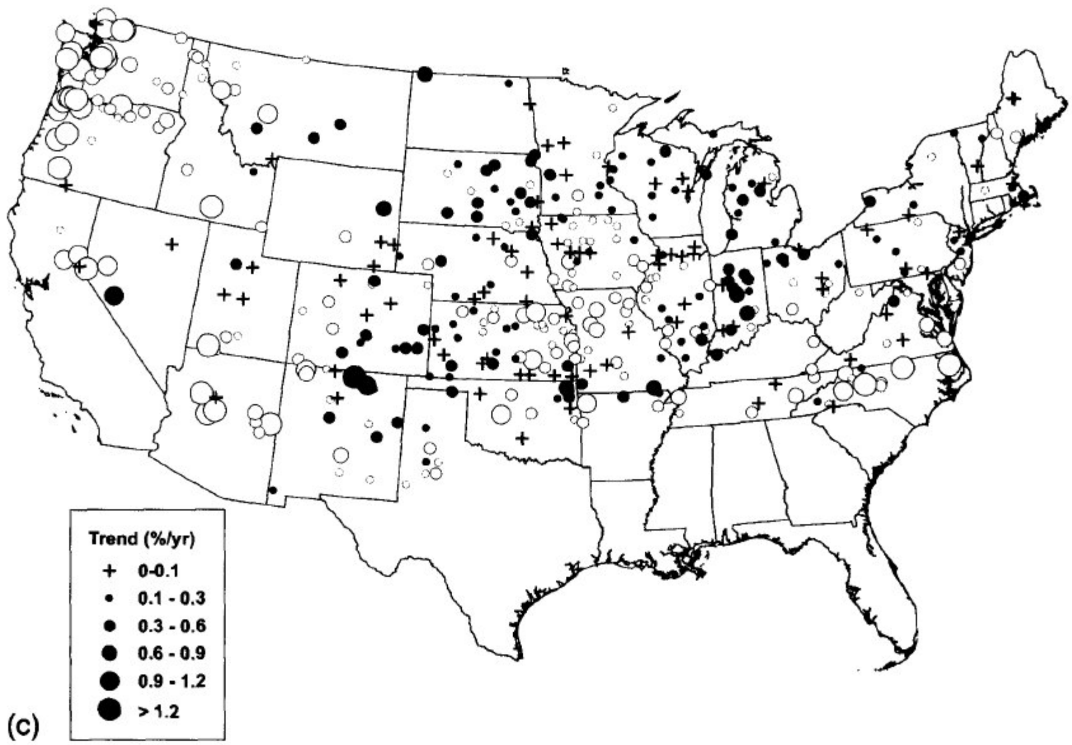
Longer tree ring records in the West have been shown to be highly related to snow water equivalent in snowpack of the area. Given a long-term tree ring record, a proxy time series of water in the snowpack can be generated. One such record from the Gunnison River basin of western Colorado is well suited for such a reconstruction (Woodhouse et al., 2003), and the record does show a substantial decline in the most recent few decades.

But, when viewed over the time frame of 430 years, the recent change appears to be well within the range of natural variability and does not seem exceptional at all.

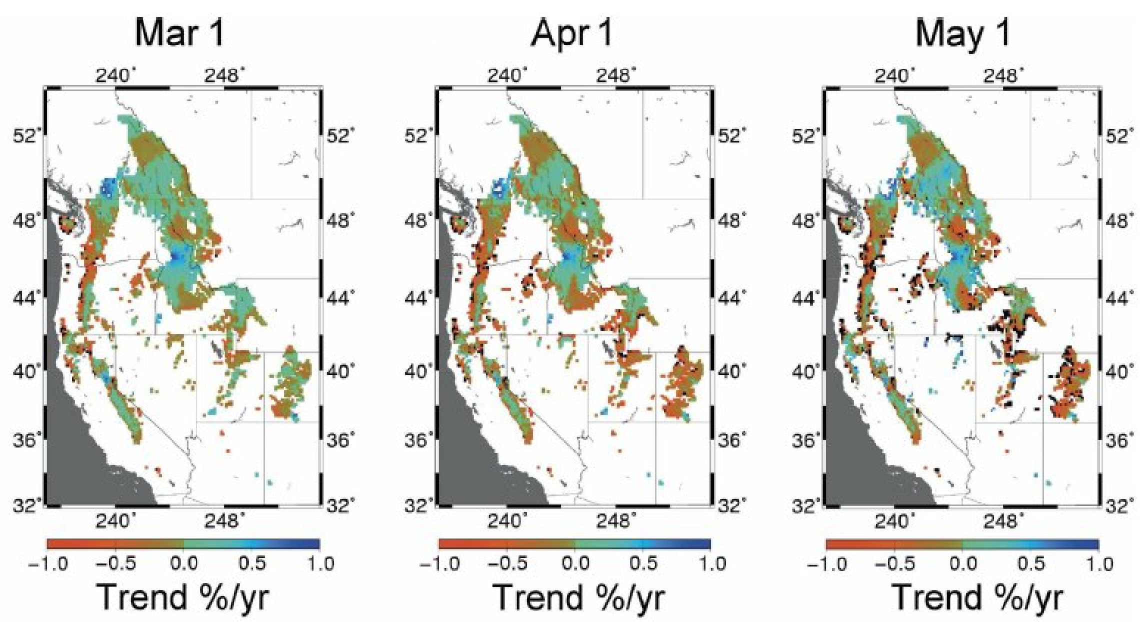


*CAPTION: Full reconstruction of Gunnison, Colorado snow water equivalent, smoothed with a 5-weight binomial filter (heavy line), and error bars (thin lines), 1571–1997. The thin line at the bottom of the graph indicates the change in total number of samples in the four chronologies used in the reconstruction over time (right-hand y axis) (source: Woodhouse et al., 2003).*

For the United States as a whole, the amount of snow has not changed significantly, nor have characteristics of snowfall such as the onset or duration of snowfall (Bartlett et al., 2005). Although the overall snowfall is largely unchanged, many investigators report an increase in snowfall in the Great Lakes area and a reduction in snowfall in the Northwest. The identification of trends in snowfall is difficult given many inconsistencies that badly contaminate long term records. Furthermore, examples can be found of nearby stations having remarkably different trends in snowfall through time that point to problems with the records as opposed to any realistic change in climate (Kunkel et al., 2009).




*CAPTION: Snowfall trend maps for 1930-31 to 2006-07. Trends are given as a percentage of the 1937-38 to 2006-07 snowfall mean per year. Closed circles: positive trends; open circles and stippling: negative trends (source: Kunkel et al., 2009).*




*CAPTION: Relative trends (% yr_1) in simulated snow water equivalent for three calendar dates for the period from 1916 to 2003 (source: Hamlet et al., 2005).*

The Mote (2006) citation only invites criticism. Despite his claims, his selective use of data—especially the endpoint—has caused great controversy and it should be avoided. Rather, Stoelinga et al. (2010) provides a much more comprehensive analysis.

They note that trend in snowpack in the entire record (1930-2007) is marginally statistically insignificant. If it were, it would be negative. Also, the trend beginning in 1976, which marks the start of the second global warming of the 20th century, is also insignificant. From 1950-1997 there was a significant decline in snowpack that was largely related to climate patterns over the North Pacific Ocean that have no obvious relationship to global climate change. After removing this factor, the trend in snowpack in the entire record remains marginally statistically insignificant.

Climate models, coupled to the observed relationship between lower-atmospheric temperature and snowpack, project a 9% decline between 1985 and 2025, considerably lower than the 29% forecast by the Washington Climate Impacts Group (Elsner et al., 2009). It is noteworthy that this NCA, similar to the last one, continues to rely heavily on it (not surprising, given the senior lead author for the Northwest section!).
Given the year-to-year noise in snowpack data, it is not clear whether a 9% decline would be significantly significant. In other words, the decline may not be scientifically distinguishable from no trend.

Streamflow in the Northwest is largely modulated by snowmelt. Unfortunately, streamflow data therefore have largely the same year-to-year variation as the snowpack does. Consequently, a prominent study of changes in the timing of Northwest peak streamflow used by the NCA used a statistical criterion for significance that does not meet normal scientific specification (Stewart et al., 2005). The NCA concluded the trends would continue, despite the fact that they cannot be distinguished from no trend whatsoever in reality.

References:

Bartlett, M.G.,D.S. Chapman, and R.N. Harris, 2005. Snow effect on North American ground temperatures, 1950-2002. *Journal of Geophysical Research,* 110, F03008, 10.1029/2005JF000293.

Elsner, M. M., et al., 2009. Implications of 21st century climate change for the hydrology of Washington State. The Washington Climate Change Impacts Assessment, J. Littell et al., Eds., Climate Impacts Group, University of Washington, 69–106.

Hamlet, A.F., P.W. Mote, M.P. Clark, and D.P. Lettenmaier, 2005. Effects of Temperature and Precipitation Variability on Snowpack Trends in the Western United States. *Journal of Climate,* 18, 4545-4561.

Kunkel, K.E.,M.A. Palecki, L. Ensor, K.G. Hubbard, D.A. Robinson, K.T. Redmond, and D.R. Easterling, 2009. Trends in twentieth-century U.S. snowfall using a quality-controlled dataset. *Journal of Atmospheric and Oceanic Technology,* 26, 33-44.

Stewart, I.T., D.R. Cayan, and M.D. Dettinger, 2005. Changes Toward Earlier Streamflow Timing across Western North America. *Journal of Climate*, 18**,** 1136-1155

Stoelinga, M.T., M.D. Albright, and C.F. Mass, 2010. A new look at snowpack trends in the Cascade Mountains. *Journal of Climate*, 23, 2473-2491.

Westerling, A.L., H.G. Hidalgo, D.R. Cayan, and T.W. Swetnam, 2006. Warming and earlier spring increases western U.S forest wildfire activity. *Sciencexpress*, July 6, 2006.

Woodhouse, C.A., 2003. A 431-yr reconstruction of western Colorado snowpack from tree rings. *Journal of Climate,* 16, 1551-1561.

**Page 110, lines 3-8**

Highly unlikely emissions scenario.

The NCA's repeated use of scenario A2 (high emissions), while it creates flashy graphics, has become inappropriate. Any statements that "we are currently above A2 emissions" are misleading because in our era A1B emissions actually exceed those for A2.

A2 was generated prior to the shale gas revolution, which has already driven energy-related U.S. emissions back to 1992 levels. The large price reductions that caused this means that the world is likely to follow suit (even if some nations are foolish enough to forbid hydrofracking). Consequently we are likely even come in below A1B, the "midrange" scenario. It is simply misleading to continue to use A2.

**Page 110, lines 9-14; Page 111, lines 1-15**

In your discussion on evapotranspiration, you left out any reference to overwhelming and widely accepted scientific evidence that increased atmospheric carbon dioxide levels increase the water use efficiency of plants. This section is incomplete without such a discussion.

**Page 111, lines 20-24**

Regarding future prediction of soil moisture in the Southwest, your assessment seems to leave out results from Gao et al. (2011) that the enhanced resolution of Regional Climate Models allowed them to better simulate the snow accumulation and ablation at high elevations of the Southwest and consequently "runoff in the Colorado River Basin is less susceptible to a warming climate in RCMs than in GCMs"; results from Gao et al. (2012) that showed that the ability of RCMs to better resolve transient eddies and their

interactions with mountains allows RCMs to capture the response of transient flux convergence to changes in stability, and consequently "that limitations in how GCMs represent terrain and its effects on moisture convergence have important implications for their ability to project future drying in the [Southwest] where mountains play an important role in the regional water cycle; and results from Lo and Famiglietti (2013) that showed that irrigation of California's Central Valley increased summertime precipitation, soil moisture, and run-off over the Southwest.

As to the Southeast, apart from its drought projections being deemed "unreliable" (Hoerling et al., 2012), Wehner et al. (2011) finds the increase in future drought to be primarily located in the western portions of the Upper Midwest—not the Southeast. Just drop this Wehner et al. (2011) reference here, and anywhere in the NCA that it is referenced—unless you are discussing how future climate impacts are often exaggerated. Further, it is not clear to me that the Georgakakos and Zhang (2011) work referenced is peer-reviewed and I have been unable to track down a copy to see the techniques it employs so I am less than confident as to its results.

Recommendation: The discussion on future soil moisture trends in the Southwest needs to be updated with reference to the most current literature. The reference to Wehner et al. (2011) concerning Southeast drought needs to be dropped. And projections of future drought in the Southeast need to be better documented.

References:

Gao, Y., J. Vano, C. Zhu, and D. P. Lettenmaier. 2011. Evaluating climate change over the Colorado River basin using regional climate models. *Journal of Geophysical Research* 116, D13104, doi:10.1029/2010JD015278.

Gao, Y., et al. 2012. Moisture flux convergence in regional and global climate models: Implications for drought in the southwestern United States under climate change. *Geophysical Research Letters* 39, L09711, doi:10.1029/2012GL051560.

Hoerling, M., et al., 2012. Is a Transition to Semi-Permanent Drought Conditions Imminent in the U.S. Great Plains? *Journal of Climate*, 25, 8380-8386, doi:10.1175/JCLI-D-12-00449.1.

Lo, M-H., and J.S. Famiglietti. 2013. Irrigation in California's Central Valley strengthens the southwestern U.S. water cycle. *Geophysical Research Letters* 40, doi:10.1002/GRL.50108.

**Page 111, line 25**

The projections of future stream run-off and streamflow are based strongly on climate model projections of future precipitation changes. As we have discussed previously, until/unless climate models can be shown to be able to reliably capture observed

precipitation changes across the U.S., future projections should not be attempted, much less discussed. For example, over many regions of the Southwest, climate models expected precipitation decline in the latter half of the 20th century, yet precipitation increase were observed instead (Polson et al., 2013). Perhaps the same will hold true in the future.

Reference:

Polson, D., G. Hegerl, X. Zhang, and T. Osborn, 2013. Causes of Robust Seasonal Land Precipitation Changes. *Journal of Climate*, doi:10.1175/JCLI-D-12-00474.1, in press.

**Page 112, Figure 3.2, lines 1-7, figure caption.**

What emissions scenario is this? If it is A2, see the previous comment (Page 110, lines 3-8).

**Page 114, lines 13-28**

Need to add text at end of paragraph:

"There is little doubt that groundwater resources of the future will be far more related to human management strategies than to changes in climate. Given the natural variability in climate, the complex response of groundwater to variations in climate, and the enormous impact on groundwater from pumping, groundwater impacts related to human-induced climate change will likely be undetectable for many decades to come."

Reference:

Hulme, M., E.M. Barrow, N.W. Arnell, P.A. Harrison, T.C. Johns, and T.E. Downing, 1999. Relative impacts of human-induced climate change and natural climate variability. *Nature,* 397, 688-691.

**Page 113, lines 26-28**

Projections of future drought conditions across the US suffer from several limitations; 1) they are based upon future precipitation projections from climate models whose reliability in reproducing observed changes in precipitation across the U.S. cannot be established; 2) they are based on temperature projections from climate models, which on average, have an equilibrium climate sensitivity that is some 40% greater than the mean value from recent estimates appearing in the peer-reviewed literature; 3) they are generally too sensitive to changes in temperature (Hoerling et al., 2012; Sheffield et al., 2012).

Recommendation: Discuss the caveats in summer drought projections for the U.S.

References:

Hoerling, M., et al., 2012. Is a Transition to Semi-Permanent Drought Conditions Imminent in the U.S. Great Plains? *Journal of Climate*, 25, 8380-8386, doi:10.1175/JCLI-D-12-00449.1,

Sheffield, J., et al., 2012. Little change in global drought over the past 60 years. *Nature*, 491, 435-438, doi:10.1038/nature11575.

**Page 113, lines 32-40; Page 114, lines 1-8**

The NCA seems to rely on Hirsch and Ryberg (2012) a lot in this section. Thus it is worth bearing in mind these words from the conclusions of Hirsch and Ryberg (2012):

> "What these results do indicate is that except for the decreased flood magnitudes observed in the SW there is no strong empirical evidence in any of the other 3 regions for increases or decreases in flood magnitudes in the face of the 32% increase in GMCO2 that has taken place over the study period."

Additionally, Villarini et al., 2019 examined annual maximum peak discharge from 572 stations in the eastern U.S. with at least 75 years of record and concluded:

> "Trend analyses for the 572 eastern United States gaging stations provide little evidence at this point (2009) for increasing flood peak distributions associated with human-induced climate change."

Future flooding in the U.S. depends on a large number of factors, that include seasonal changes in total precipitation and extreme precipitation events, changes in the frequency of landfalling tropical cyclones, and, perhaps above all, human-caused changes to the physical properties of watersheds and stream/river courses. It is not clear that all these factors have been appropriately included in the current NCA analysis and assessment.

References:

Hirsch, R.M., and K.R. Ryberg, 2012. Has the magnitude of floods across the USA changed with global CO2 levels? *Hydrological Sciences Journal*, 57:1, 1-9, http://dx.doi.org/10.1080/02626667.2011.621895.

Villarini, G., et al., 2009. On the stationarity of annual flood peaks in the continental United States during the 20th century. *Water Resources Research*, 45, W08417, doi:10.1029/2008WR007645.

**Page 115, lines17-19; Page 116, lines 1-2**

Citation in complete disagreement with attribution.

The text states, "These aquifers and wetlands…may be at particular risk due to the combined effects of…accelerating sea level rise and greater storm surges (Chang et al., 2011…).

Chang et al., (2011) actually says:

> Climate change effects are expected to substantially raise the average sea level. It is widely assumed that this raise will have a severe adverse impact on saltwater intrusion processes in coastal aquifers. In this study we hypothesize that a natural mechanism, identified here as the ''lifting process,'' has the potential to mitigate, or in some cases completely reverse, the adverse intrusion effects induced by sea-level rise.

**Page 119, lines 12-13**

Inaccurate analysis.

The frequency of a type of weather anomaly is proportional to reduced vulnerability, which is why, for example, heat-related mortality is virtually unknown in Tampa and Phoenix (Davis et al, 2003).

Reference:

Davis R.E., et al., 2003. Changing Heat-Related Mortality in the United States. *Environmental Health Perspectives*, 111, 1712–18.

**Page 120, line 115**

There is not one word about per-capita water withdrawal and use (the subject of the relevant sentence) in the reference Haley (2001). As Casey Stengel used to say, "you could look it up."

**Page 121, lines 4-5**

We repeat: The Assessment's repeated use of scenario A2 (high emissions), while it creates flashy graphics, has become inappropriate. Any statements that "we are currently above A2 emissions" are misleading because in our era A1B emissions actually exceed those for A2.

A2 was generated prior to the shale gas revolution, which has already driven energy-related U.S. emissions back to 1992 levels. The large price reductions that caused this means that the world is likely to follow suit (even if some nations are foolish enough to forbid hydrofracking). Consequently we are likely even to come in below A1B, the "midrange" scenario. It is simply misleading to continue to use A2, although it is good for your budget (and bad for the rest of us).

**Page 122, lines 1-6**

Overblown issue.

In its 2009 National Climate Assessment, the USGCRP could only find one report of a significant power plant shutdown because of low water. It that report it was appropriately sourced as "Bull et al., 2007."

Given the warmth and drought of some recent summers, it is obvious that most plants are designed with substantial latitude with regard to cooling water.

**Page 122, line 17**

San Francisco?

Sea-level rise on the West Coast is largely muted by tectonic activity, which is why it averages 1-2 mm/year in the area around San Francisco. Saying that, say, three times that will be a problem is a reversion to what Paul Waggoner used to call the "Dumb-Farmer Mistake," meaning that farmers are too stupid to adapt to change.

**Page 122, line 22**

"Economic conditions may constrain implementation [of adaptation]."

Upon what is this based? That certainly needs a reference!

**Page 123, lines 1-5, entire sidebar.**

Anecdotal.

This is remarkable. Anecdotes substitute for science in the National Assessment along with the conflation of one flood and one drought as evidence for pernicious climate change, i.e., at least drop the sophomoric "interestingly" in the fifth line from the bottom.

**Page 124, lines 1-12**

Neglected significant hydropower in NE US.

Quebec-Hydro generates a more power for the Northeast than we generate domestically, and its contribution is likely to grow because of the Regional Greenhouse Gas Initiative as well as the Assessment's forecast for increasing precipitation in southeast Canada (Figure 2.12).

**Page 124, line 37**

Inappropriate reference.

Although we will append a list of the nonstandard references to the end of this chapter review, we should point out that "Union of Concerned Scientists 2009" is particularly egregious and likely to result in some nasty op-eds.

**Page 126, line 4-6**

As we have discussed in several of our comments, precipitation projections need to be taken with a grain of salt, if not eliminated entirely from the NCA. As we have discussed in previous comments, heavy precipitation events have been shown to be impacted by factors besides AGW.  As we have discussed in previous comments, run-off and flooding magnitudes are greatly impacted by other man-made changes to the environment (e.g., increase in impervious surfaces, river channelization, etc.).

The degree to which climate change (as opposed to climate and other man-made environmental changes) will impact flooding must be discussed with these caveats prominently mentioned.

**Page 129, line 15-17**

Heavy precipitation is impacted by other human-caused environmental changes besides greenhouse gas emissions—a factors which seemed to be ignored in this section. Without direct mention of these other influences, the reader is left with the impression that you are discussing AGW. If so, then you should demonstrate that the magnitude of AGW impacts actually is detectable above all the other influences on heavy precipitation events.

**Page 149-164, References**

This chapter contains quite a number of grey literature and nonstandard references, even with a very forgiving screen.  As noted above, some could cause quite a lot of trouble.

Adams, A., D. Behar, K. Brooks, P. Fleming, and L. Stickel, 2012: Water Utility Climate Alliance's Technical Input to the 2012 NCA

Barsugli, J., C. Anderson, J.B. Smith, and J.M. Vogel, 2009: Options for improving climate modeling to assist water utility planning for climate change. Final Report., Water Utility Climate Alliance. [Available online at http://www.wucaonline.org/assets/pdf/pubs_whitepaper_120909.pdf]

Berry, L., 2012: Florida Water Management and Adaptation in the Face of Climate Change: A white paper on climate change and Florida's resources.

Brekke, L.D., 2011: *Addressing Climate Change in Long-Term Water Resources Planning and Management: User Needs for Improving Tools and Information.* DIANE Publishing

Brekke, L.D., J.E. Kiang, J.R. Olsen, R.S. Pulwarty, D.A. Raff, D.P. Turnipseed, R.W. Webb, and K.D. White, 2009a: *Climate change and water resources management: a federal perspective.* DIANE Publishing.

City of New York, 2012: PlaNYC Progress Report 2012. A Greener, Greater New York, City of New York. [Available online at http://nytelecom.vo.llnwd.net/o15/agencies/planyc2030/pdf/PlaNYC_Progress_Report_2012_W11 eb.pdf]

Foti, R., T.C. Brown, and J.A. Ramirez, 2012: *Vulnerability of future United States water supply to shortage.* U.S. Forest Service.

Heimlich, B.N., F. Bloetscher, D.E. Meeroff, and J. Murley, 2009: Southeast Florida's Resilient Water Resources: Adaptation to sea level rise and other climate change impacts, Florida Atlantic University. *Center for Urban and Environmental Solutions and Department of Civil Engineering, Environmental, and Geomatics Engineering*, [Available online at www.ces.fau.edu/files/projects/climate_change/SE_Florida_Resilient_Water_Resources.pdf]

Liverman, D., S. Moser, P. Weiland, L. Dilling, M. Boykoff, H. Brown, D. Busch, E. Gordon, C. Greene, E. Holthaus, D. Niemeier, S. Pincetl, J. Steenburgh, and V. Tidwell, 2012: Climate Choices for a Sustainable Southwest. *Assessment of Climate Change in the Southwest United* States*: a Technical Report Prepared for the U.S. National Climate Assessment. A report by the Southwest Climate Alliance*, G. Garfin, A. Jardine, R. Merideth, M. Black, and J. Overpeck, 1 Eds., Southwest Climate Alliance, pp. 684-734

Means, E., M. Laugier, J. Daw, L. Kaatz, and M. Waage, 2010a: Decision support planning methods: Incorporating climate change uncertainties into water planning, Water Utility Alliance. [Available online at http://www.wucaonline.org/assets/pdf/pubs_whitepaper_012110.pdf]

Transboundary Aquifer Assessment Act, 2009: Transboundary Aquifer Assessment Act (U.S. Public Law 109-448), signed December 22, 2009. *The intent is to provide scientific information useful to policymakers and water managers*

Union of Concerned Scientists, 2009: Climate Change in the United States: The Prohibitive Costs of Inaction. [Available online at http://www.ucsusa.org/global_warming/science_and_impacts/impacts/climate-costs-of-inaction.html]

## Chapter 4 Energy Supply and Use

### General Comments

This report suffers from the systematic asymmetry that has plagued National Assessments since the first one. This is largely reflected in emphasis on unlikely emissions pathways (namely A2, which will be obviated by global switching to gas for energy and transportation). It is evident from treatment of heating/cooling degree day issues, and completely ignoring the (unknown) adaptation multiplier with respect to climate change. This last factor makes reports like this seem hopelessly jejune, probably to the point that it will be counterproductive for those favoring expensive and expansive emissions policies.

The adaptation multiplier absence is pervasive in all of the Assessments. As an example, consider a simple agricultural model. It is a fact that bad weather affects regional crop yields, and it is certainly plausible (but by no means proven) that warmer temperatures will increase drought frequency and/or magnitude, but it is hardly proven because *all other factors are hardly equal*.

One is adaptation. Let's stipulate that it's getting warmer in the Midwest. Despite this, yields continue to increase at the historical rate established over a half-century ago. The increase is due to changing tillage technology, increasing fertilizer application, and faster improvement of genetic cultivars thanks largely to DNA splicing and plasmid insertions. Both tillage and genetics can respond to changing climate. For example, no-till is much more moisture efficient, especially in the black soils of the corn belt. I am sure that many of the major seed companies are experimenting with DNA manipulations that can result in fewer stomates or increased stomatal resistence, and other moisture-conserving strategies.

These adaptations, which result in increasing yields, are in part in response to perception that less moisture stress is desirable. Increasing temperature only spurs further innovation.

It is very difficult to quantify the positive impacts of climate change, but it is very clear, for example, that heat-related mortality is inversely proportional to heat wave frequency

and magnitude. Our cities have run this experiment for us, not even needing global warming, as they warm independent of that.

It's hard to believe that there are not similar factors at work at the energy/climate interface. Space cooling technology is becoming increasingly efficient. There's no a priori reason to assume that adaptive technologies do not exist with regard to the issue of cooling water for power plants, but there is very little mention of anything like this in this chapter or in the overall Assessment. It's conspicuously absent and serves to seriously compromise its quality and the public reception it will receive.

Another problem concerns both this chapter (and the overall Assessment's) over-reliance on emissions scenario A2. It is hard to believe that this is at all viable given the dramatic shifts to natural gas for electrical generation (and likely for much of the surface transshipment industry) that are occurring or will occur relatively soon.

Your predictable counter, that emissions are currently above A2, is irrelevant, as indeed A2 emissions for the current era are projected *lower* than A1B. Nice sleight of hand, but no dice.

Using unlikely "extreme" scenario or, increasingly unlikely "extreme" surface warming (see other parts of this review) additionally compromises this (and the previous) Assessments.

**Page 167, lines 13-16**

**Key Message 1**

Message is only partial

It would be appropriate to insert a few quantitative words here, and to at least acknowledge that there are unknown adaptive multipliers that may mean that even the *sign* of the net financial impacts of climate change may not be known. More details on this in our review of specific text.

**Page 167, lines 17-20**

**Key Message 2**

Lack of quantitative analysis, again.

National studies project that the demand for cooling energy is will increase from 5 to 20 percent per degree (C) of warming (US Climate Change Program, 2008), and the demand for heating energy to drop by 3 to 15 percent for the same change. These ranges reflect

different assumptions about factors such as the rate of market penetration of improved building equipment technologies.

While the vast majority of space cooling is provided by electricity, the recently exploited abundance of natural gas can certainly alter the current distribution. Indeed, gas air conditioners are now twice as efficient as they were 25 years ago, and inflation-adjusted hardware costs are clearly lower. It is truly odd that somehow this was not even considered worth mentioning.

An examination of population-weighted annual cooling degree days over the last 60 years show a marginally significant (p=.047) increase of 6%, and a marginally insignificant (p=.064) change in heating degree days (raw trend, -3.9%) (EIA, 2009). According to the Energy Information Administration, while the total number of households in the United States is expected to increase at a rate of 1.0 percent per year through 2035 and average house square footage is expected to increase at 0.7 percent per year, total energy consumed in BTUs per square foot is expected to decline by 1.3 percent per year (EIA, 2011). The positive efficiencies resulting from new technologies will therefore have more of an effect on energy consumption than any increases that might be caused by warming.

**Figure 1.9 Heating Degree-Days by Census Division**

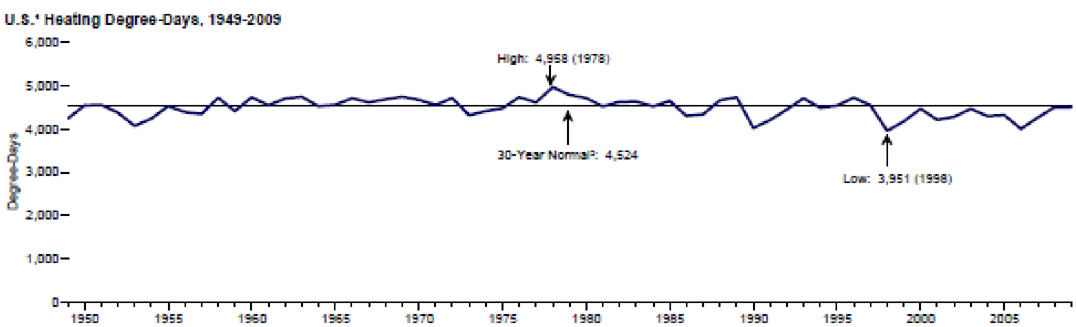
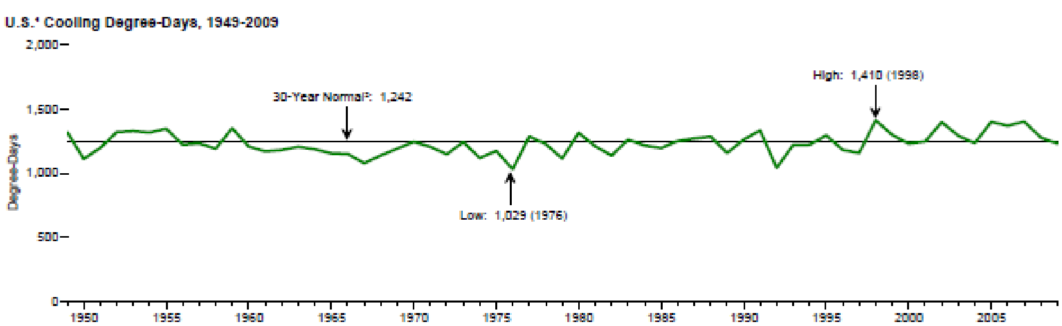



**Figure 1.10 Cooling Degree-Days by Census Division**




*CAPTION: There is a marginally significant increase in cooling degree-days and a marginally insignificant decrease in heating degree-days over the period of record (EIA, 2011).*

References:

Energy Information Administration, Annual Energy Review 2009, http://www.eia.gov/totalenergy/data/annual/pdf/sec1_22.pdf and http://www.eia.gov/totalenergy/data/annual/pdf/sec1_20.pdf

Energy Information Administration, Annual Energy Outlook 2011, http://www.eia.gov/analysis/projection-data.cfm#annualproj

U.S. Climate Change Science Program, Effects of Climate Change on Energy Production and Use in the United States, February 2008, http://www.climatescience.gov/Library/sap/sap4-5/final-report/sap4-5-final-all.pdf

**Page 167, lines 31-32**

*What* “specific risks to energy security”?

“Energy security” isn’t threatened by a Gulf hurricane or a Midwestern drought. The nation’s security is much stronger than that. And I don’t think it’s appropriate to raise the specter of the Russian Bear coming along to steal all of our oil in the ice-free Arctic summer.

Authors really need to re-think the whole “security” issue in a nation that could easily be a net exporter within ten years, thanks to hydrofracking and horizontal drilling.

**Page 168, lines 15-19**

One-sidedness.

This is the kind of paragraph that gives your detractors ammunition to accuse NCADAC of bias. There are other weather “extremes” that are hardly deleterious and certainly correlated with surface warming. Those would include very warm winters, unusually long growing seasons, etc.

**Page 168, lines 20-21**

Oooh! One inch of rain in a day is bad? We doubt that’s ever caused a significant flood.

**Page 168, lines 25-29**

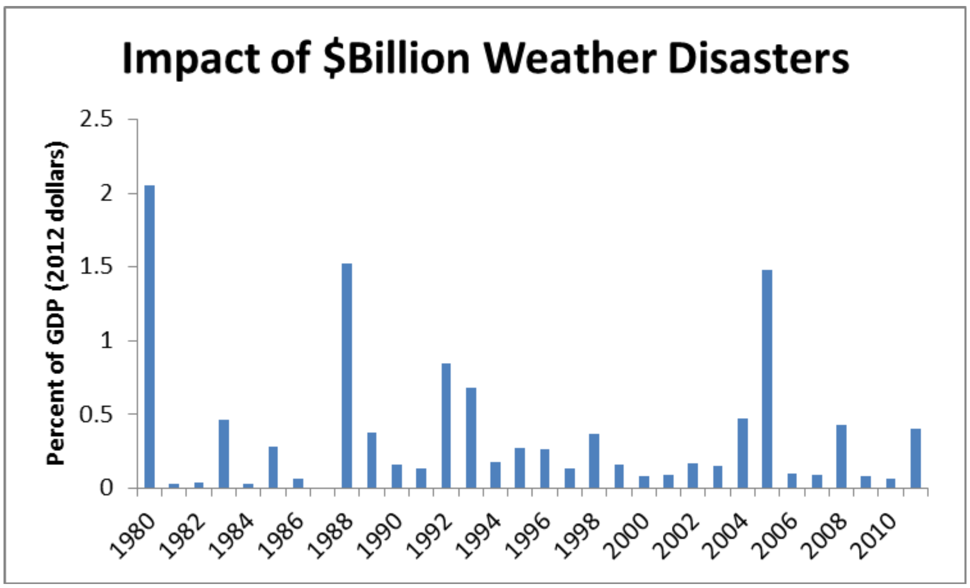
Did you notice how little the power went out in areas of the northern Mid-Atlantic where significant Sandy winds hit, if those same areas had been affected by the June 29, 2012 derecho? Trees don't regrow their branches that fast and the local power companies were under intense pressure to trim more aggressively around power lines. People adapt, and a perception of increased storminess (real or not) will lead to demands for cleaner power lines. It's the same notion as the fact that the warmest urban areas in the country (i.e. Tampa and Phoenix) have the lowest heat-related mortality. It's called "adaptation".

**Page 168, line 34**

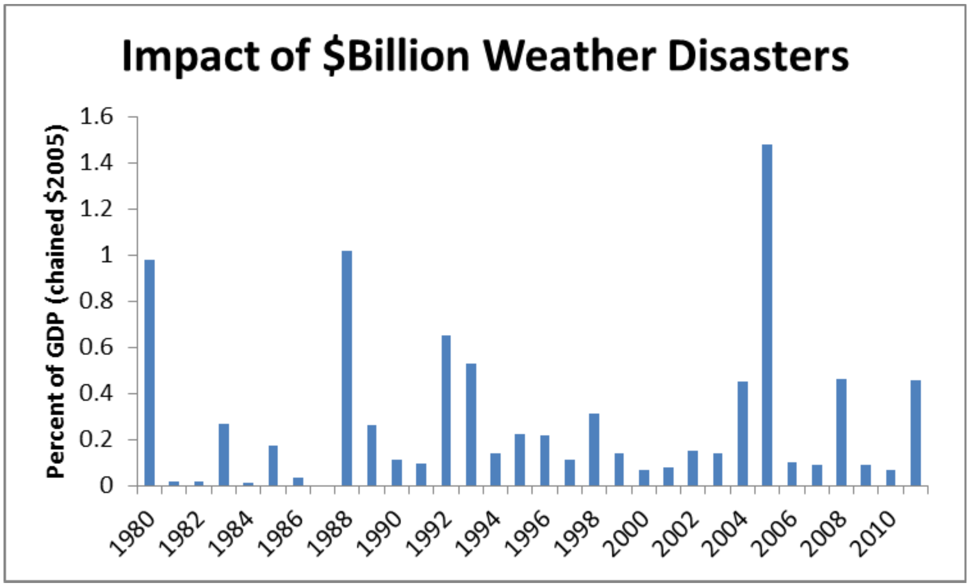
**"**Economic losses arising from weather and climate events are large and have been increasing."

*This statement is wrong*.

Normalized damages from weather events are not increasing as our figures below show.




*CAPTION: The annual total of damage from NOAA's identified billion-dollar weather events divided by GDP in billions of current (2012) dollars.*




*CAPTION: The annual total of damage from NOAA's identified billion-dollar weather events divided by GDP in billions of chained (2005) dollars. (2012 was not available at this writing. Even if it were high, it would have to exceed 2005 by a long shot on the bottom figure to show a (non-robust) trend.)*

So, even if you have increasing "extreme events" (that wouldn't apply to hurricanes or tornadoes, as you well know), there's no detectable effect. *Maybe* people adapt?

**Page 169, lines 1-5, Figure 4.1**

Nice rhetoric, a little short on facts.

Oil and natural gas disruptions from hurricanes in the Gulf of Mexico are a fact of business life. 2005 Hurricane Katrina was a memorable storm, a category 3 hurricane at landfall, and a category 5 in Gulf of Mexico. It was the second-costliest hurricane (adjusted for constant dollars and normalized for population) in U.S. history with catastrophic damages estimated at $81 billion to New Orleans and the Mississippi coast (NOAA, 2007).

Katrina was, in many ways, a worst-case hurricane for the oil-producing Outer Continental Shelf of the Gulf of Mexico, which is the source for about 30 percent of crude oil production in the United States and about 13 percent of natural gas production EIA). Refining operations in the Gulf were only shut-in for about a month.

Shortfalls were made up by refined product imports and crude purchases from the Strategic Petroleum Reserve (CRS, 2006). And, older oil and gas drilling and production equipment that had to be replaced resulted in newer infrastructure that is more resilient and likely to withstand future storms (Rigzone, 2010).

Future disruptions to natural gas supplies in the Gulf, if they were to occur regardless of cause, will most likely be countered by the economic production of shale gas onshore in the United States. The Energy Information Administration expects about 50% of total natural gas production to be from shale by 2035 (EIA, 2011).

References:

Congressional Research Service, Oil and Gas disruption from Hurricanes Katrina and Rita, April 6, 2006, http://www.au.af.mil/au/awc/awcgate/crs/rl33124.pdf

Energy Information Administration, Annual Energy Outlook 2011, Table A14, http://www.eia.gov/forecasts/aeo/pdf/tbla14.pdf

Energy Information Administration, EIA Special Report-Gulf of Mexico Fact Sheet, http://205.254.135.24/special/gulf_of_mexico/

National Oceanic and Atmospheric Administration, The Deadliest, Costliest and Most Intense United States Tropical Cyclones from 1851 to 2006, Technical Memorandum NWS TCP-5, April 2007, http://www.nhc.noaa.gov/pdf/NWS-TPC-5.pdf

Rigzone Analysis: Gas Prices Immune to Hurricane Disruptions Post-Katrina, Rita, August 31, 2010, http://www.rigzone.com/news/article.asp?a_id=98139

**Page 171, lines 1-17, Figure 4.3**

Where's the other half (HDD's)?
Not to sound like a broken record, but it does *not* help NCADAC's reputation to only harp on one type of impact. Show the analogous maps for the decline in HDD's.

(These figures also suffer from the oft-repeated problem that scenario A2 was developed before hydrofracking and horizontal drilling exploded, and it is no longer plausible—assuming the world responds to economic incentives and displaces coal and gasoline (where economical) with natural gas).

**Page 174, lines 2-5**

Inappropriate reference.

The NCA is not required to use references that are inappropriate. The cited study uses only A2 and A1Fi, which is even bigger than A2.

**Page 174, lines 30-31**

Real data argue otherwise.

McCabe et al. (2010) actually looked to see if the number of dry days was increasing in the Southwest, which has been trending towards increasing drought. Here's what they found:

> "trends in the fraction of dry days for water years, cool seasons, and warm seasons indicate that most trends are negative [i.e. towards more wet days]. For water years, 18 sites exhibit negative trends in the fraction of dry days, and eight of these trends are statistically significant at a 95% confidence level. In contrast, only four sites indicate positive trends in the fraction of dry days for water years, and none of these trends is statistically significant at p = 0.05. For the cool season, 19 sites exhibit negative trends (12 are statistically significant at p = 0.05), and only 3 sites indicate positive trends (none are statistically significant)."

The opposite to what is being forecast is what is occurring. You need to adjust the text to include this.

Reference:

McCabe, G. J., D. R. Legates, and H. F. Lins. 2010. Variability and trends in dry day frequency and dry event length in the southwestern United States, *Journal of Geophysical Research*, 115, D07108, doi:10.1029/2009JD012866.

**Page 175, lines 1-9, Figure 4.4**

This map is certainly wrong.

Disregarding the inappropriateness of scenario A2, Figure 2.12 from Chapter 2 is supposed to show the same thing—projected seasonal precipitation changes—although for a different period of comparison. Figure 2.12 shows the change from 2070-2099 compared to 1901-60. There is no way that a shorter period in Figure 4.4 (2041-2070 compared to 1971-2000) would have much larger precipitation changes. I don't know what is being used in Chapter 4, but it simply cannot be correct as the precipitation changes are far too large for a period that begins a mere 40 years after the base!

Oh. The Chapter 4 version is a model from a less complete source than the CMIP-5 family and shows the most extreme changes. Shades of the first (2000) Assessment, where the most extreme temperature and precipitation models were selected!

Doesn't the USGCRP ever learn? The use of those models is one reason that it ran afoul of the Data Quality act. Leaving this model in will invite the same.

**Page 176, line 8**

Grammar or meaning problem.

Don't get "are in counties with some type of water sustainability (EPRI 2011)." As written this would mean "are in counties with very dependable water."

**Page 176, lines 14-23**

Many misleading statements and incompletions.

Note on sea level rise:

Because a significant fraction of America's energy infrastructure is located near the coasts, sea level rise could be a concern. According to the IPCC *Fourth Assessment Report*, "Global average sea level rose at an average rate of 1.8 [1.3 to 2.3] mm per year over 1961 to 2003. The rate was faster over 1993 to 2003: about 3.1 [2.4 to 3.8] mm per year. Whether the faster rate for 1993 to 2003 reflects decadal variability or an increase in the longer-term trend is unclear." An update to the satellite sea level rise record (Nerem et al., 2010) through 2012 shows that the decadal rate of sea level rise has been slowing. From 2002 to 2012 the rate of sea level rise was 2.7 mm per year (0.09 inches per year). This slowdown in the rate of global sea level rise suggests that the faster rate of rise noted by the IPCC from 1993 to 2003 was influenced in part by short-term natural variability characteristic of the $20^{th}$ century sea level rise record, rather than a full indication of the increase in the long-term rate of sea level rise.

Add a 40% reduction in the estimate of the equilibrium climate sensitivity and rapid sea level rise is really out of the question.

The current rate of sea level rise, 2.7 mm per year, is equivalent to approximately 1 inch per decade—a rate which adaptive and protective responses can keep up with. There is no evidence for any effect of sea level rise on the US energy infrastructure.

The 6.6 foot reference should therefore be removed.

**Page 176, lines 24-30**

A note on the resilience of the Gulf Coast energy complex should be added:

> The resiliency of the U.S. oil and gas industry has allowed energy production to continue even after large hurricanes. The Gulf Coast is home to a significant portion of the U.S. oil and gas industries, representing nearly 30 percent of the nation's crude oil production and approximately 13 percent of its natural gas production (EIA). One-third of the national refining and processing capacity lies on coastal plains adjacent to the Gulf of Mexico. Several thousand offshore

drilling platforms, dozens of refineries, and thousands of miles of pipelines are vulnerable to damage and disruption due to the high winds and storm surge associated with hurricanes and other tropical storms. Powerful hurricanes (such as Katrina and Rita in 2005) temporarily halted all oil and gas production from the Gulf, disrupted nearly 20 percent of the nation's refinery capacity, and closed many oil and gas pipelines. Such low-frequency extreme events will always cause disruptions, but the economic history of the US shows that, in the large scope, they are inconsequential.

The diversification of supply points helps to cope with extreme events. As an example, in Katrina, most of the high-volume platforms that operate in deep waters and account for nearly half of the Gulf's offshore oil production escaped significant damage (CBO, 2005).

References:

Congressional Budget Office, Testimony of Douglas Holtz-Eakin, Director, Macroeconomic and Budgetary Effects of Hurricanes Katrina and Rita, October 6, 2005, http://www.cbo.gov/ftpdocs/66xx/doc6684/10-06-Hurricanes.pdf

Energy Information Administration, EIA Special Report-Gulf of Mexico Fact Sheet, http://205.254.135.24/special/gulf_of_mexico/

**Pages 178-179, Table 4.2**

Yay! It's about time that the NCA talks about what *will* happen, namely that there will be considerable sectoral adaptation. Of course, people in the energy business probably know what to do, so this Table really isn't necessary.

**Page 180, lines 9-11**

The biofuels industry is a complete waste of taxpayer money. It can't roll without a push from Washington, and it results in more GHG production than simply burning the energy equivalent of gasoline. Actually, if yields went down, that would probably force reconsideration of the wisdom of the "Saudi Arabia of corn burning" up 40% of it.

But yields are likely to increase given the long lead times for adaptation and genetic engineering. Failing that, sorghum will simply be substituted for corn as it is much more drought tolerant and has roughly the same nutritive value as corn. People adapt to perceived changes and trends. Don't you?

**Page 190-192, References**

The following are pretty much egregious nonstandard references. Using the Union of Concerned Scientists really invites criticism, IMHO:

Averyt, K., J. Fisher, A. Huber-Lee, A. Lewis, J. Macknick, N. Madden, J. Rogers, and S. Tellinghuisen, 2011: Freshwater use by US power plants: Electricity's thirst for a precious resource. A report of the Energy and Water in a Warming World initiative, Union of Concerned Scientists, Cambridge, MA

Burkett, V., 2011: Global climate change implications for coastal and offshore oil and gas development. Energy Policy

Entergy Corporation, 2012: Building a Resilient Energy Gulf Coast: Executive Report. [Available online at http://www.entergy.com/gulfcoastadaptation]

Rosenzweig, C., W. Solecki, R. Blake, M. Bowman, A. Castaldi, C. Faris, V. Gornitz, K. Jacob, A. LeBlanc, and R. Leichenko, 2009: Climate Risk Information. *New York City Panel on* Climate *Change*

Sathaye, J., L. Dale, P. Larsen, G. Fitts, K. Koy, S. Lewis, and A. Lucena, 2011: Estimating Risk to California Energy Infrastructure from Projected Climate Change

Wei, M., 2012: California's Carbon Challenge. Scenarios for Achieving 80% Emissions Reductions in 2050. [Available online at http://censeps.soe.ucsc.edu/sites/default/files/2011- wei.pdf]

Wilbanks, T., D. Bilello, D. Schmalzer, and M. Scott, 2012b: Climate Change and Energy Supply and Use

Wilbanks, T., G. Backus, S. Fernandez, P. Garcia, K. Jonietz, P. Kirshen, M. Savonis, B. Solecki, and L. Toole, 2012a: Climate Change Infrastructure, Urban Systems, and Vulnerabilities

## Chapter 5 Transportation

### General Comments

There seems to be an implication in this Chapter that the impacts from anthropogenic climate changes are leading to an overall decline in the U.S. transportation system. This idea is forwarded by statements such as found in Key Message 1 that "[anthropogenic climate changes] are reducing the reliability and capacity of the U.S. transportation system in many ways."

The reality of the situation is that the reliability and capacity of the U.S. transportation is increasing and expanding. Clearly improvements are outpacing any impacts from climate

change—if such negative impacts even exist at all (they are likely undetectable if other factors are accounted for and the proper normalization procedures are applied).

This fact ought to be better highlighted.

**Page 195, lines 14-17**

**Key Message 1**

As data from the Bureau of Transportation Statistics of the U.S. Department of Transportation (http://www.rita.dot.gov/bts/sites/rita.dot.gov.bts/files/publications/national_transportation_statistics/index.html#chapter_1) plainly shows, reliability and capacity of the U.S. transportation system is *expanding*—contrary to the primary claim made in this Key Message. Examples of this are provided in additional comments.

Recommendation: Remove this Key Message.

**Page 195, lines 18-20**

**Key Message 2**

In discussions of the projections of global sea level rise, it is imperative to discuss the impacts of new evidence that suggests that the equilibrium climate sensitivity is 40% lower than the GCM average used in the NCA projections. Almost certainly, models with a 40% lower equilibrium climate sensitivity will project less sea level rise and thus less impact on the U.S. transportation system.

See our Comment Page 31, Lines 15-18 for further details.

**Page 195, lines 21-22**

**Key Message 3**

"Extreme weather events currently disrupt transportation networks in all areas of the country; projections indicate that such disruptions will increase."

In most cases in which risks are increasing, actions are taken to mitigate those risks. This includes risks from climate and climate change.

In making your "projections" that such "disruptions will increase" how have you factored in modernization of the U.S. transportation system, upgrades, and adaptations that will

surely occur? If you did not take them into account, then you should modify your statement to something like:

“Under the unrealistic assumption that the U.S. transportation system remains static into the future, projections indicate that disruptions from extreme weather events will increase.”

**Page 196, lines 1-5**

You throw out carbon dioxide emissions numbers without any perspective whatsoever. In and of themselves, they are meaningless.

The U.S. transportation sector produced 1,849 million metric tons of carbon dioxide in 2009, 34% of the total U.S. energy-related emissions of carbon dioxide (EIA, 2011). The U.S. produced 18% of the global total carbon dioxide emissions from the consumption of energy in 2009 (EIA). The U.S. transportation sector was responsible for 6% of the global total. Emissions from the U.S. transportation section have been growing at an average rate of 24 million metric tons of $CO_2$ per year for the past two decades (although since peaking in 2007, they have been in decline). The growth of emissions in China has been at a rate of 253 million metric tons of $CO_2$ per year during the same period, or more than 10 times greater than the growth of emissions from the U.S. transportation sector. In fact, the average rate of emissions growth in China is so great that it adds new emissions equivalent to the total annual emissions from the U.S. transportation sector every 5 weeks.

Using the methodology of the United Nations’ Intergovernmental Panel on Climate Change, a complete cessation of emissions from U.S. transportation would reduce mean projected global warming approximately 0.11°F per 50 years, an amount too small to reliably measure. Clearly, emissions from the U.S. transportation sector play a minor and rapidly diminishing role in total global greenhouse gas emissions.

It is not climate *change*, but the vagaries of the climate itself that have the greatest impact on U.S. transportation. Climate change, to the degree that it is detectable and identifiable, contributes a mix of impacts, some positive and some negative, and the net impact has never been reliably quantified or monetized.

The impacts of climate and climate change are confused and thus used interchangeably, however, such usage is incorrect and misleading.

Recommendation: Drop the paragraph in its entirety or put the U.S. transportation emissions and their fractional impact of in the specific weather types that may impact U.S. transportation in its proper perspective.

Recommendation: Drop the paragraph in its entirety or put the U.S. transportation emissions and their fractional influence on the specific weather events that impact U.S. transportation in the proper perspective.

References:

Energy Information Administration, Emissions of Greenhouse Gases in the U. S., March 31, 2011

Energy Information Administration, International Energy Statistics, http://www.eia.gov/cfapps/ipdbproject/IEDIndex3.cfm?tid=90&pid=44&aid=8

**Page 196, lines 6-9**

You claim that "Transportation systems are already experiencing costly climate change related impacts."

Can you quantify the additional cost that human-related climate change caused the transportation system over and above the costs incurred by plain old climate variability? If not, how do you know the climate change related impacts were "costly"?

You claim that "Many inland states – for example, Vermont, Tennessee, Iowa, and Missouri – have experienced severe precipitation events and flooding during the past three years, damaging roads, bridges, and rail systems." We assume that you all think that these severe precipitation events were related to human-caused climate change? Otherwise why list them?

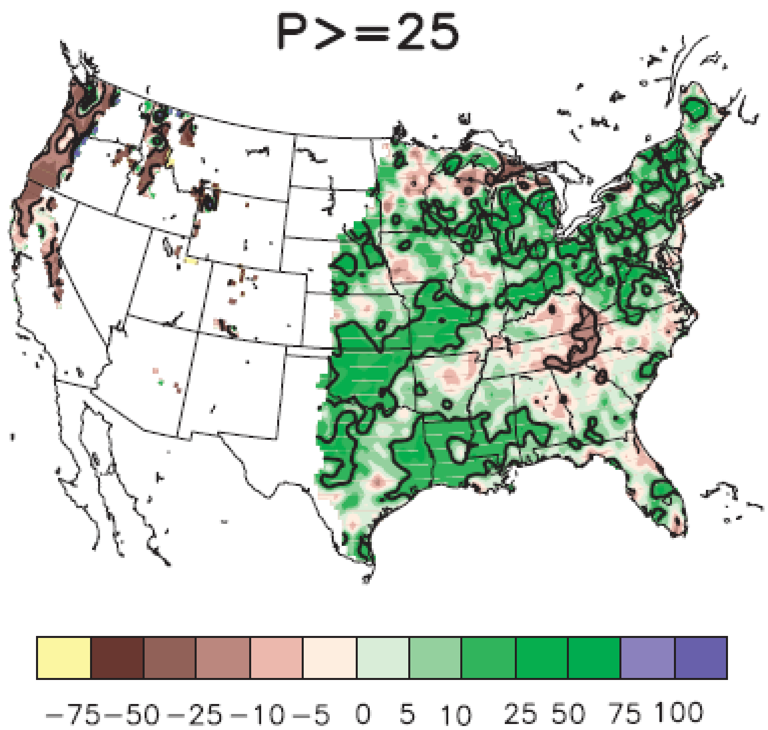
The Figure below is from Higgis and Kousky (2013) and shows the percent change in the annual number of daily precipitation events (1980-2009 minus 1950-79) for daily events which produced over 25mm of precipitation (their highest category).

We note that the number of daily precipitation events greater than 25mm has declined in much of Tennessee and parts of Iowa. So how is it that you suggest that heavy precipitation events there which lead to flooding were from human-caused climate change when the observed climate change is towards fewer heavy events there?

And as to the overall changes in heavy precipitation across the U.S., Higgis and Kousky (2013) find that they are strongly related to ENSO variability.

So we ask again, what portion of the impact to the US transportation infrastructure from those storms was attributable to human-caused climate change?

If you don't know, then we suggest that you drop these sentences.




*CAPTION: The percent change in the annual number of daily precipitation events (1980-2009 minus 1950-79) for daily events which produced over 25mm of precipitation (from Higgis and Kousky, 2013).*

Reference:

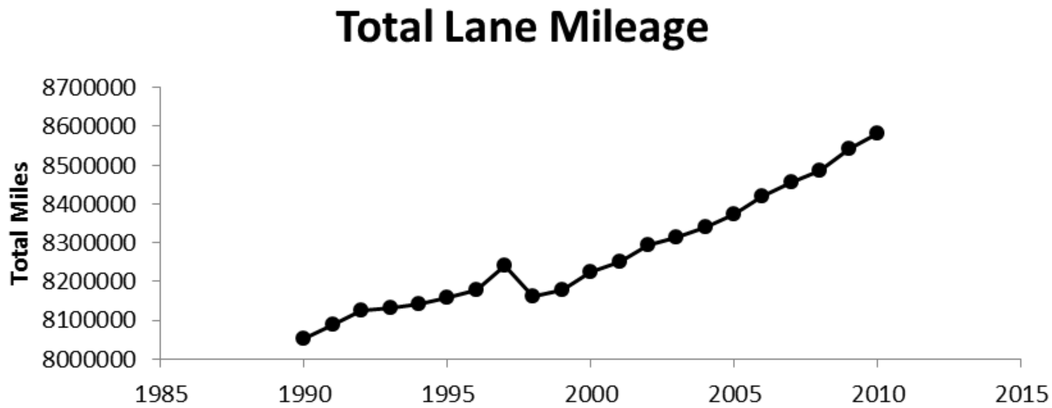
Higgis, R.W., and V.E. Kousky, 2013. Changes in observed and daily precipitation over the United States between 1950-79 and 1980-2009. *Journal of Hydrometeorology*, 114, 105-121, DOI: 10.1175/JHM-D-12-062.1

**Page 197, lines 2-5**

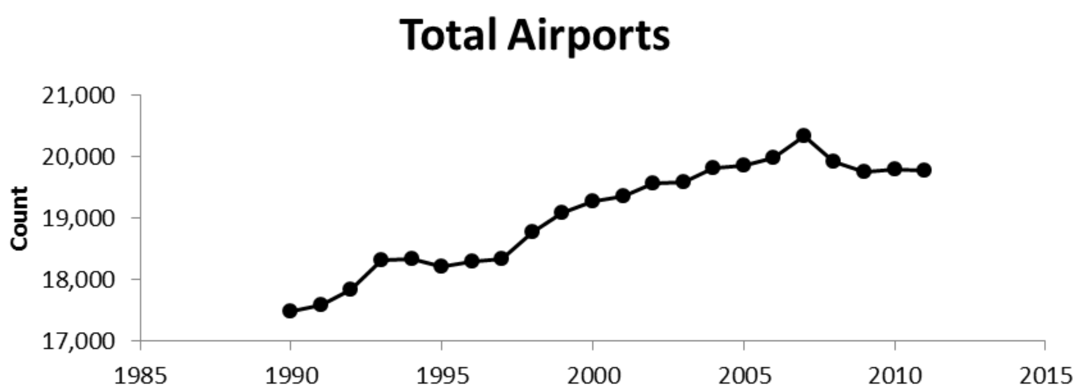
"…are reducing the reliability and capacity of the U.S. transportation system"

Do you have any data to back this up? Or did you just make this up?

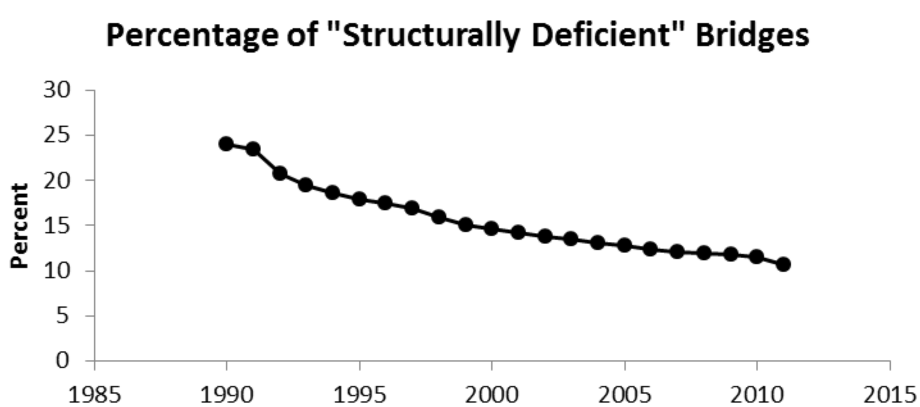
The Figures below are from data from the Bureau of Transportation Statistics of the U.S. Department of Transportation (http://www.rita.dot.gov/bts/sites/rita.dot.gov.bts/files/publications/national_transportation_statistics/index.html#chapter_1). Since 1990, in the United States, total roadway lane mileage has increased, the total number of airports has increased, the percentage of 'structurally deficient' bridges has decreased, and the percentage of 'unacceptable' roadway surface conditions has decreased.



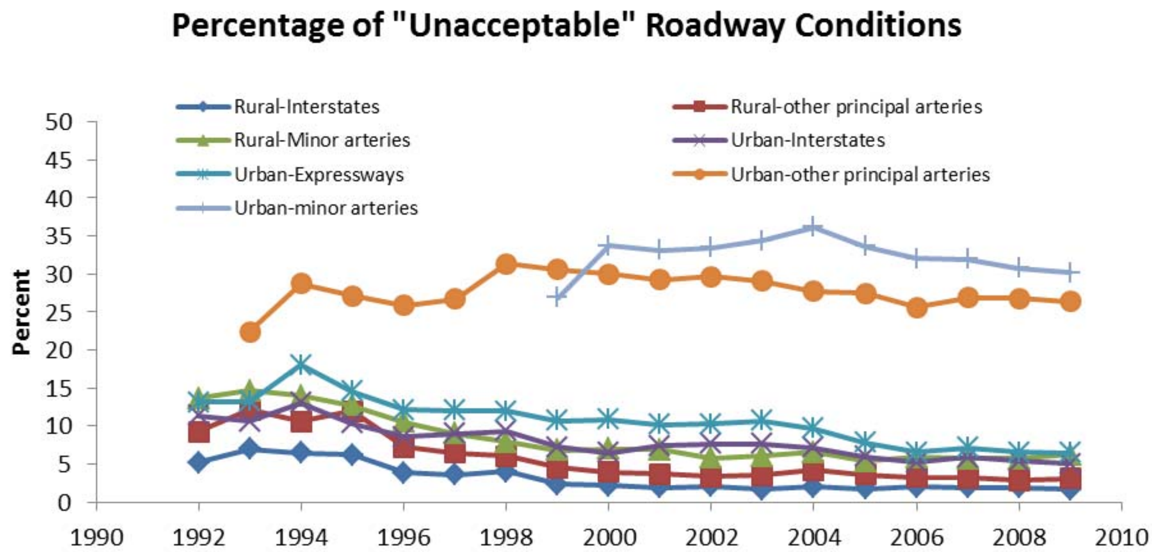

*CAPTION: Estimated U.S. roadway lane mileage (data source: U.S. Bureau of Transportation Statistics).*




*CAPTION: Number of U.S. airports (data source: U.S. Bureau of Transportation Statistics).*

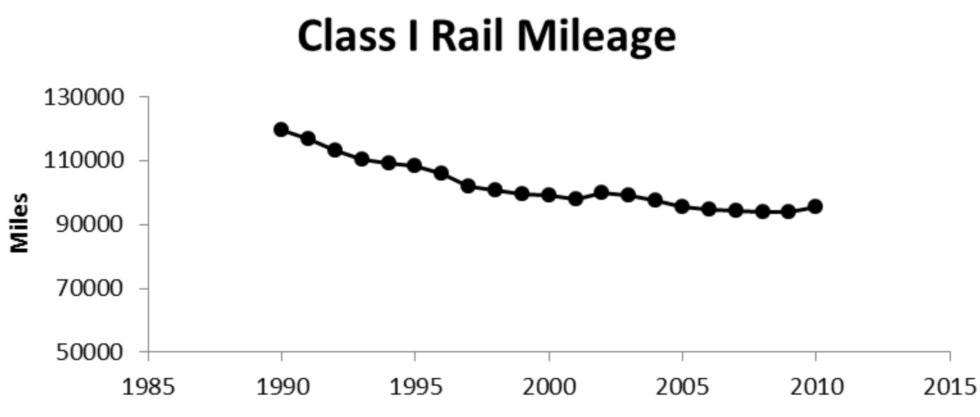



*CAPTION: Percentage of "structurally deficient" bridges on U.S. highways (data source: U.S. Bureau of Transportation Statistics).*

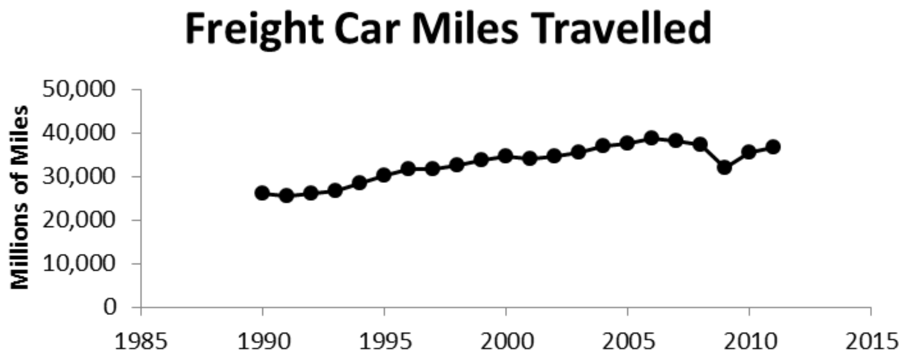



*CAPTION: Percentage of "unacceptable" roadway conditions by functional system (data source: U.S. Bureau of Transportation Statistics).*

Of the major transportation systems, only the number of Class 1 rail road miles has declined, that is due largely to abandonment of little-used lines and the proliferation of non class-1 (regional) railroads. The total number of freight car miles travelled has been increasing.




*CAPTION: Estimated mileage of Class 1 rail. Mileage excludes yard tracks, sidings, and parallel lines. (data source: U.S. Bureau of Transportation Statistics).*




*CAPTION: Freight car-miles travelled (data source: U.S. Bureau of Transportation Statistics).*

So clearly your proposition that the reliability and capacity of if the U.S. transportation system is reduced is being reduced by climate change is going to be a tough one for you all to demonstrate.

Recommendation: Drop the entire section on Reliability and Capacity Risk (as well as Chapter 5 "Key Point 1") because you do not demonstrate that it is true, in the face of the improving capacity and reliability of the U.S. transportation system.

**Page 197, lines 13-14**

Add a note that your climate models in general have a climate sensitivity that is about 40% higher than recent estimates and thus the future climate change projections from climate models are likely overestimates.

See our Comment Page 31, Lines 15-18 for further details.

**Page 197, line 27**

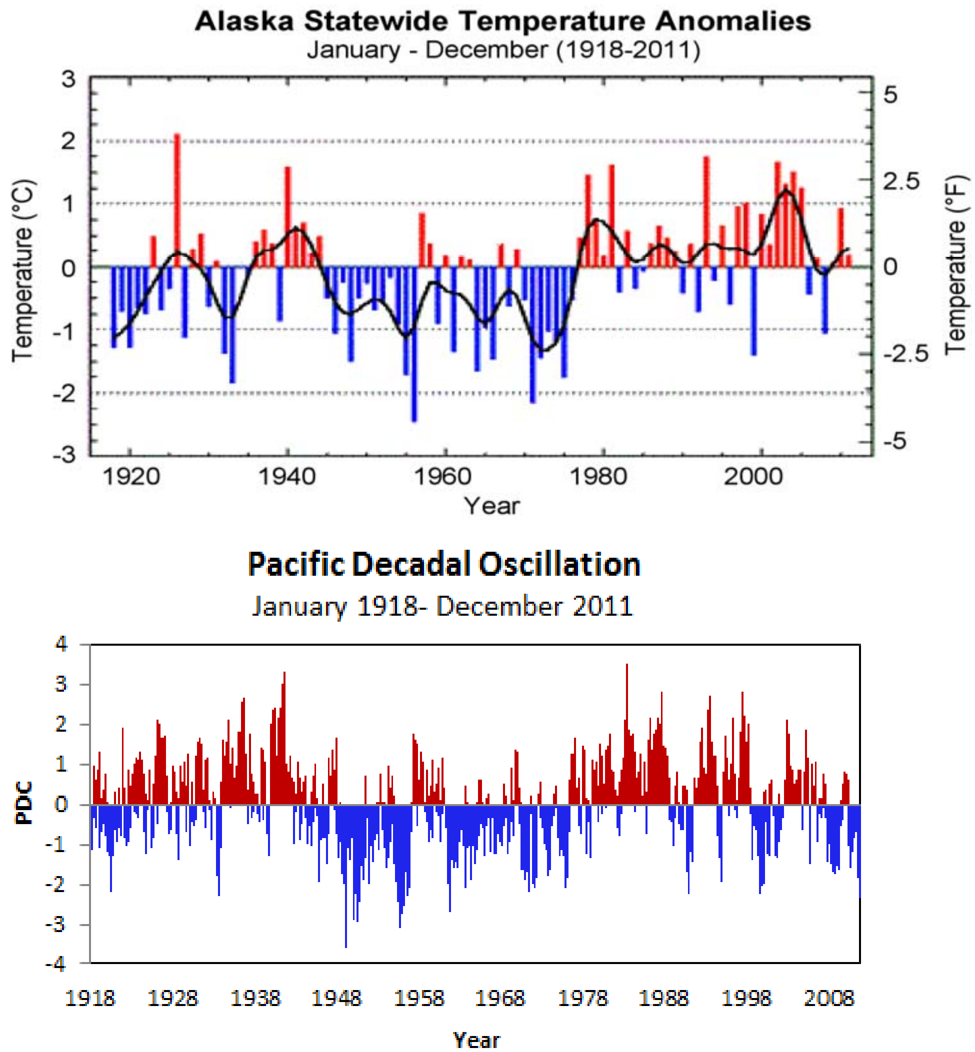
"Climate change is most severe at high northern latitudes."

What is the definition that you are using for "severe"? Is a change that exceeds some relative bounds developed for each location? Is it a change that exceeds some absolute bound for each location? Does "severe" mean bad? Can a "severe" change have positive results? On net, over the long term, is a greener Arctic a bad thing? You need to be more precise in your descriptions.

**Page 197, lines 28-29**

Your description of temperature change in Alaska lacks context and thus implies that the observed changes are the result of human-caused climate changes. They largely are not.

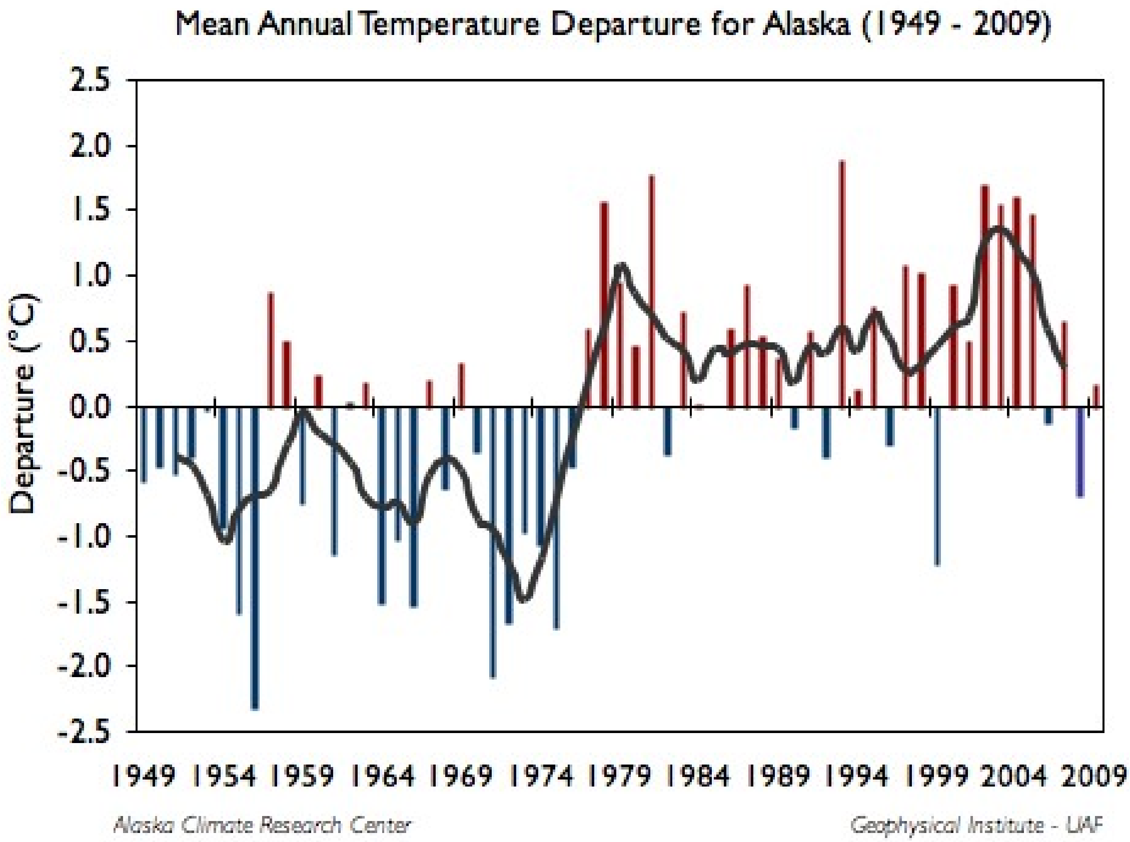
Our suggestion would be to discuss the various influences on observed temperatures in Alaska and show the magnitude of the influence of the PDO and include the figures below showing temperatures in Alaska beginning back in 1918, when NCDC begins their data.




*CAPTION: The top panel shows the statewide average temperature in Alaska from 1918 through 2011. The bottom panel shows the Pacific Decadal Oscillation (PDO) index over the same period. Notice that temperatures in Alaska largely reflect the PDO.*

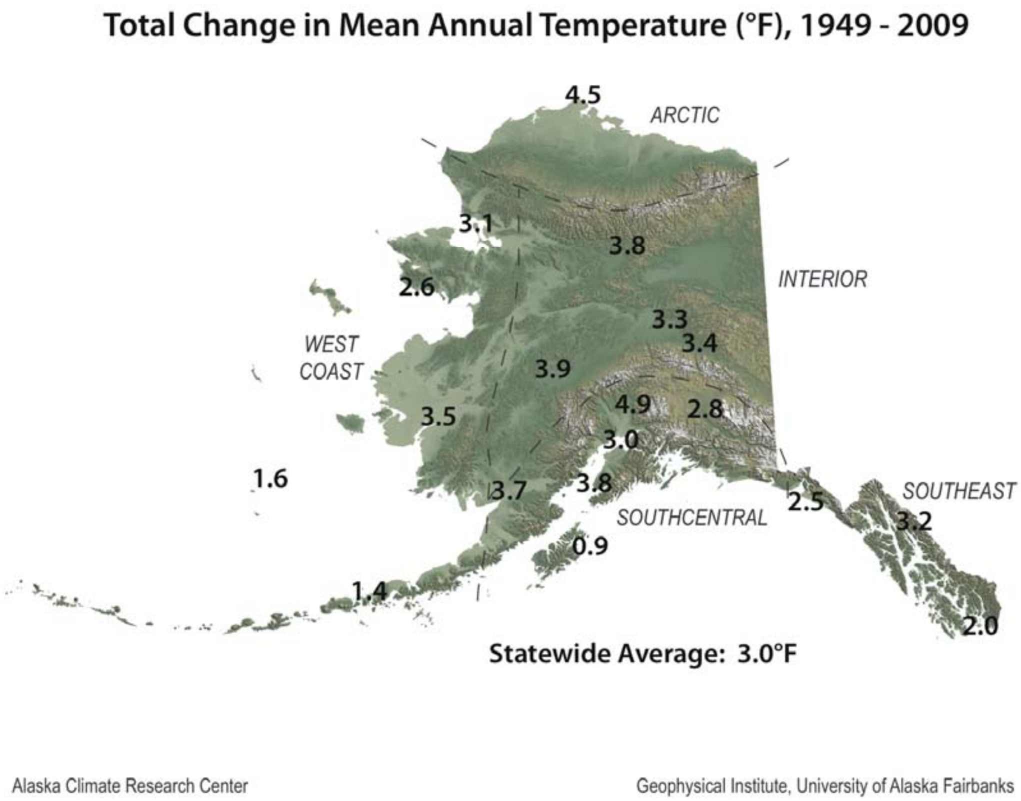
Alaskan climate change has been enigmatic and complex. One clear signal is that, in general (with one or two notable exceptions), the statewide temperature history is characterized by a step-change in 1976-77, which was recognized in hindsight (nearly twenty years later) as a sudden reorganization of pan-Pacific climate known as the Great Pacific Climate Shift (Miller et al., 1994). The ultimate cause of this change and the reasons for its persistence are currently not known.

The Pacific Climate Shift involved 40 physical variables, including the climatic pattern known as the Pacific Multidecadal Oscillation (Miller et al., 1994). As a result, statewide average records tend to show no net warming prior to or subsequent to this change.




*CAPTION: Annual average temperature history for Alaska since 1949 (source: http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html).*

The power of the Pacific Climate Shift is evident in this post-1948 temperature history and shows why reporting the trend since 1949 (as is done in the NCA) is a poor idea. As noted by the Alaska Climate Research Center, at the University of Alaska (Fairbanks), a plot of gross trends is inappropriate because of the step change-nature of the Alaskan climate history.




*CAPTION: Without the accompanying table (shown below), this map is very misleading about Alaskan climate change (source: http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html).*

It is very apparent that since the Pacific Climate Shift there is very little secular temperature change over all of Alaska with the exception of Barrow. The very large Autumn change there is almost certainly related to the decline of sea ice which has a strong local climate influence.

**Total Change in Mean Seasonal and Annual Temperature (°F), 1977 - 2008**

| *Region* | Location | Winter | Spring | Summer | Autumn | Annual |
|---|---|---|---|---|---|---|
| *Arctic* | Barrow | 1.5 | 4.2 | 1.8 | 8.3 | 4.0 |
| *Interior* | Bettles | 2.3 | -0.3 | -0.4 | 0.7 | 0.1 |
| | Big Delta | 3.6 | -1.5 | 0.1 | -0.3 | -0.2 |
| | Fairbanks | -1.0 | -1.7 | 0.6 | -1.4 | -1.3 |
| | McGrath | 0.7 | 1.7 | 2.3 | 1.1 | 1.2 |
| *West Coast* | Kotzebue | -2.1 | 1.0 | -0.5 | 1.7 | -0.2 |
| | Nome | -3.0 | -0.5 | -1.2 | 0.4 | -1.4 |
| | Bethel | -1.2 | 0.7 | 2.4 | 0.6 | 0.4 |
| | King Salmon | -1.3 | -1.3 | 1.3 | 0.6 | -0.6 |
| | Cold Bay | -1.3 | -1.9 | -0.1 | 0 | -1.1 |
| | St Paul | -2.9 | -1.3 | 0.1 | 0.2 | -1.0 |
| *Southcentral* | Anchorage | 0.6 | -1.4 | 0.3 | -0.1 | -0.6 |
| | Talkeetna | 3.8 | 1.9 | 2.8 | 2.4 | 2.2 |
| | Gulkana | 2.0 | -0.9 | 0.8 | -1.3 | -0.6 |
| | Homer | 0.3 | -0.7 | 1.1 | 0.1 | -0.2 |
| | Kodiak | -3.0 | -2.6 | -0.8 | -2.4 | -2.3 |
| *Southeast* | Yakutat | 1.5 | -1.8 | 0.4 | -0.5 | -0.5 |
| | Juneau | 3.7 | -1.8 | -0.1 | -0.5 | -0.1 |
| | Annette | 1.4 | -0.1 | 0.4 | 0.1 | 0.2 |
| | *Average* | 0.3 | -0.4 | 0.6 | 0.5 | -0.1 |

Alaska Climate Research Center Geophysical Institute, University of Alaska Fairbanks

*CAPTION: Total change in mean seasonal and annual temperature from stations in Alaska, 1977-2008 (the period since the Great Pacific Climate Shift).(Source: http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html).*

The remaining station that shows a significant increase since 1976 is Talkeetna, but there is likely some type of warming bias at the site when compared to other records that are "nearby" (in Alaskan terms), Gulkana and Anchorage International.

One interesting aspect of Alaskan temperatures south of the Brooks Range (which divides interior Alaska from the northern coastal plain) is that satellite (microwave)-sensed lower tropospheric temperature show a rise since the Pacific Climate Shift that is not detected by ground-based thermometers. Because both measurements (satellite microwave and thermometer) are presumably accurate, one is left to hypothesize a systematic discontinuity between the lower troposphere (satellite-sensed) and the boundary layer (thermometrically measured) temperature over Alaska.

References:

Alaska data: http://climate.gi.alaska.edu/ClimTrends/Change/TempChange.html

Miller, et al., 1994. The 1976-77 climate shift of the Pacific Ocean. *Oceanography* **7**, 21-26.

**Page 198, lines 1-13**

While thawing permafrost is a concern from rising temperatures—whether from the natural oscillations of the PDO, anthropogenic global warming, or some combination of these and other factors—the concern should not be overstated, in that the land-based transportation infrastructure of Alaska located in regions where thawing permafrost is sparse. Repairs and improvements can be made on a case by case basis and in association with other planned improvement and expansion projects. In the state of Alaska's long-range transportation policy plan adopted in 2008, concerns about thawing permafrost are rarely mentioned (except in association with planned improvement to the Dalton Highway) and concerns of climate change-related impacts to the state's transportation infrastructure play only a minor role in the overall long-range policy.

**Page 199, Figure 5.1**

You seem to be trying to confuse the reader between the impacts of climate and the impacts of climate change in this figure. To help clarify the difference, please include a map of the same region showing only the impact of "a storm surge similar to Hurricane Katrina." That way, through visually differencing the two maps, the reader can better understand the impact of the 30-inch sea level rise. As it is now, a cursory look at the figure leaves you with the impression that all of the inundation is due to climate change-related sea level rise, when, in truth, probably very little of it is due to that factor.

**Page 200, lines 15-16, page 201 Figure 5.2**

"Thirteen of the nation's largest airports have at least one runway with an elevation within 12 feet of current sea levels (Airnav LLC 2012)."

Uh, remind us one more time as to what your sea level rise projections were? We thought they were 1 to 4 feet, but if that is the case, then why did you provide a count of the airports within 12 feet of sea level?

**Page 201 Figure 5.2**

One of the airports in the list, Louis Armstrong International is listed as being 1.7 feet *below* sea level. Some portions of San Francisco International and Oakland International were actually part of the San Francisco Bay about 50 years ago. Portions of Reagan National Airport were once mudflats of the Potomac River, some runways at LaGuardia and JFK were also reclaimed from the water.

Areas that are currently below sea level or that were recently water now support major airports. If that situation didn't deter the current existence of the airports, why should it do so in the future?

Recommendation: Remove Figure 5.2 and related discussion in the text.

**Page 202, lines 11-15**

Replace these lines about inland waterways and flooding with the text below, a far more accurate portrayal of the situation:

> Oftentimes, there is confusion when attributing or associating major flood events on major river systems—such as the Mississippi/Missouri river floods of 1993 and 2008—to increases in precipitation extremes. These river systems are highly altered from there natural state by a variety of engineering schemes intended to "control" the rivers to enhance shipping commerce and protect riverfront communities from flooding. While the collection of levies, dykes, channel alterations, etc., have largely achieved this goal on a day to day basis, they oftentimes exacerbate conditions of extremely high flow. The increases in impervious surfaces and the channelization of the river flow (which keeps the rivers from overflowing into their natural flood plains) leads to confined flow and increasing flow speeds which can result in extremely high, erosive water levels and catastrophic flooding and concomitant disruption of transportation services and damages to transportation infrastructure, when the river level tops or breaks through existing protection structures. Certainly heavy and persistent rainfall is the instigator of major flooding events, but human alterations to the waterways and management decisions can exacerbate the magnitude and destructive potential of the flood events.

**Page 202, lines 20-23**

There is a body of scientific literature that projects that AGW will alter the storm track of Atlantic hurricanes such that it is shifted eastward resulting in a decrease in the number of U.S. landfalls. That literature seems not to have been taken into account in your description.

Instead of your few lines on hurricanes, we suggest the following, more thorough treatment:

> Projections of future changes in tropical cyclone (tropical storms and hurricanes) characteristics are neither overly large nor unambiguous. Globally, the frequency of tropical cyclones is expected to be decline slightly with increasing atmospheric greenhouse gas concentration

increases. Tropical cyclone intensity is expected to increase slightly. However, at the regional level, changes may depart from the global tendency. In the Atlantic basin, new research suggests that although there may be a tendency for a slight increase in both storm number and storm intensity, the preferred storm track may be shifted towards more storms out to sea in the central Atlantic and away from the continental U.S. (Wang et al., 2011). As the greatest hurricane-related impact to coastal transportation infrastructure occurs when hurricanes make a direct strike to the U.S., a future tendency for land-falling hurricanes (of any strength) to become less frequent would mitigate hurricane-related damages.

However, the projected *changes* to Atlantic tropical cyclone characteristics are neither certain nor large enough to warrant directed measures modifying the nation's transportation infrastructure. Instead, it should be recognized that there are large natural variations in hurricane characteristics that occur over timescales from years to decades. Tropical cyclones have been and will continue to be threats to coastal development. Periods characterized by lulls in Atlantic hurricane activity—such as the late 1970s, 1980s and early 1990s—underrepresent the true nature of the threat and encourage booms in coastal development and the accompanying transportation infrastructure. Active periods of Atlantic tropical cyclones, such as the 1940s and 1950s and again in the period since the mid-1990s serve as reminders of existing vulnerabilities.

A collection of some of the world's leading hurricane researchers issued the following statement that reflects the current thinking on hurricanes and their potential impact (Emanuel et al., 2006):

> …the 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* that demands our attention, but the vulnerability of existing and planned transportation infrastructure to the *existing* climate. The damage potential from on-going demographic changes in coastal locations far exceeds that from even the worst projections of climate change induced alterations to the characteristics of the storms themselves (Pielke Jr., 2007).

References:

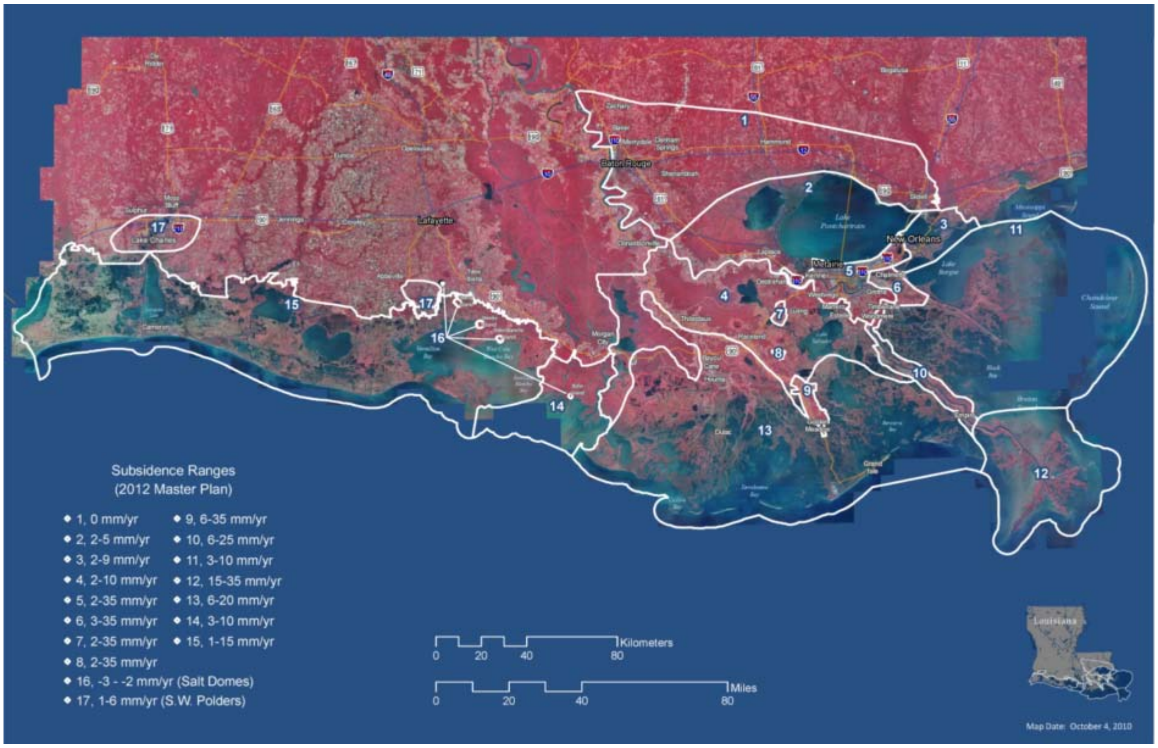
Emanuel, K., et al., 2006. Statement on the U.S. hurricane problem, http://wind.mit.edu/~emanuel/Hurricane_threat.htm

Pielke, Jr., R. A., 2007. Future economic damage from tropical cyclones: sensitivities to societal and climate changes. Philosophical Transactions of the Royal Society A, doi:10.1098/rsta.2007.2086

Wang, C., L. Hailong, S-K. Lee, and R. Atlas, 2011. Impact of the Atlantic warm pool on United States landfalling hurricanes. *Geophysical Research Letters*, 38, L19702, doi:10.1029/2011GL049265.

**Page 203, Figure 5.3**

Along with the current Figure 5.3 "Gulf Coast Transportation Hubs at Risk" add the figure (and caption) below to show the reader what the non-AGW rates of sea level rise are in the region.




*CAPTION: This map shows an estimate of the rate of land subsidence along the Louisiana coast—a sinking of the land not related to global sea level rise. Although the subsidence rate varies by a large amount along the Louisiana coastline, on average it is occurring at a rate about 5 times greater than the actual sea level is rising. What this means, is that in most areas along the Louisiana coast will experience sea level rise close to 4 feet from land sinking alone and irrespective of global warming-induced sea level rise. Therefore actions to mitigate the impact of a large sea level rise will be required, where necessary, from ongoing changes in the geography of the region. (figure from: Recommendations for Anticipation sea-level rise impacts on Louisiana Coastal Resources during Project Planning and Design. Draft report, 2012, Louisiana Applied Coastal Engineering and Science (LACES) Division, Coastal Protection and Restoration Authority of Louisiana, State of Louisiana. http://coastal.louisiana.gov/index.cfm?md=pagebuilder&tmp=home&pid=240)*

**Page 203-204, Box 2 Hurricane Sandy**

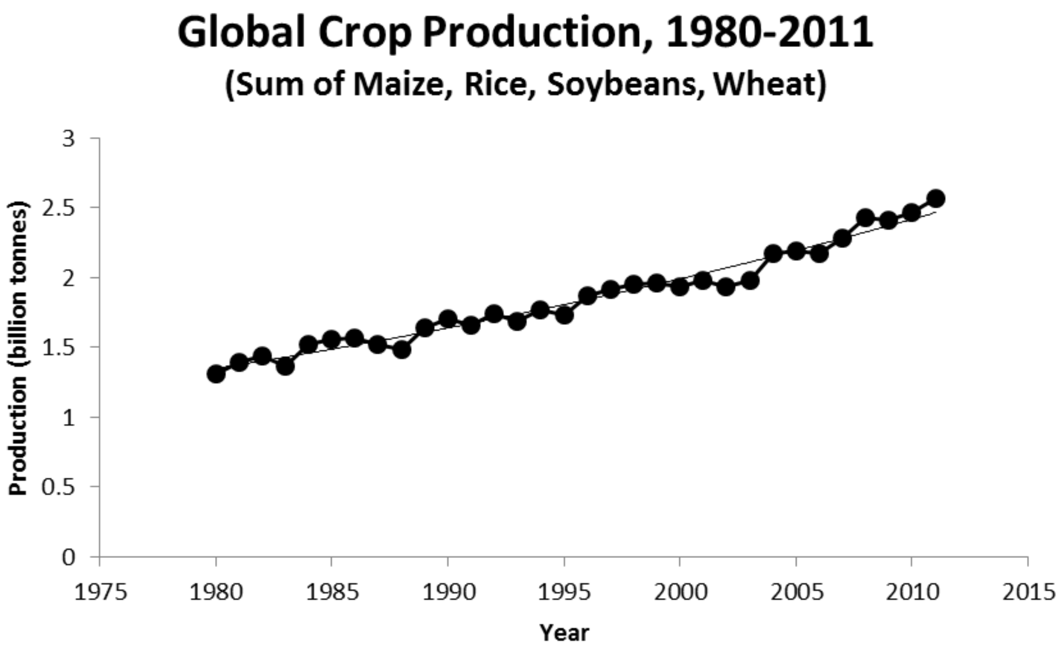
What exactly is the point of this Box? There is no definitive science that links the path or intensity of Sandy to AGW. The only robust impact of AGW in the region was a sea level rise of about 6 inches. So, please recast your Box 2 in terms of what damage the extra 6 inches of sea level caused—everything else is chalked up to a natural occurrence independent from AGW and need not be included in the NCA as it gives the reader a false sense of reality.

**Chapter 6 Agriculture**

**Page 228, lines 5-8**

"Climate change…will also alter the stability of food supplies and create new food security challenges"

Food supply is a function of production minus consumption. This statement is supported in the text by Lobell (2011), a paper (mis)titled "Climate Trends and Global Crop Production since 1980." The paper is actually about yield, which is the amount per planted or harvested acre. (Production is the product of yield multiplied by acreage.) So, while it is obvious that changes in temperature and precipitation have detectable effects on yield, what really matters is how much is produced, which is reproduced below.




*CAPTION: Global annual total production from maize, rice, soybeans, and wheat. Data source: Food and Agriculture Organization, United Nations, available at http://faostat.fao.org/site/567/DesktopDefault.aspx?PageID=567*

An exponential fit ($Y = 2283.3 - 2.32\ (\text{year}) + .00059(\text{year})^2$) to the global crop production is significantly better than a linear one, which demonstrates how insignificant the climatic component of global food production is.

What's missing here? There's simply little, if any, effect of year-to-year global climate variability. That's because the world food system is highly diverse, in terms of varieties grown and the climates in which they grow.

Further, there is a tremendous amount of reserve built into food supply because of the (stupid) diversion towards biofuels. In fact, the amount we divert to ethanol dwarfs the amount that is lost to climate. Lobell (2011) reported that, after allowing for the growth enhancement from atmospheric carbon dioxide, global average crop yields were reduced by a bit less than 1% (which is small compared to the amount that they increased because of technological advances) during the period 1980-2008.

But consider this. The U.S. produces about 36 percent of the world's corn. And about 40 percent of U.S. corn is used to produce ethanol for use as a gasoline substitute instead of being consumed by humans or animals. Globally, corn makes up 30 percent of total worldwide production of the four crops studied by Lobell's group.

And even this less than 1 percent impact was described by Lobell et al. as perhaps being "overly pessimistic" because it did not fully incorporate long-term adaptive farming responses to changing climate conditions (i.e., farmers are not as dumb as statistical models make them out to be).

What this means is that even under overly pessimistic scenarios, we still currently burn more than 4 times as much grain as climate change has taken away. Thinking about this in future terms, if we observe *twice* as much climate change from 2010 through 2038 as we did from 1980 to 2008 (Lobell's study period), all we would have to do is stop burning *half* as much ethanol as we do now to make up for the *entire* global climate-related crop reduction.

Therefore, climate is an irrelevant overlay on world food supply for the foreseeable future. If we really need the food, just stop the stupid conversion to ethanol.

Obviously there is a lot of wastage in the world food supply from this stupid policy and any statement about the "stability of food supply" and "food security" challenges, is a result of the farm lobby and not climate. Why is this missing from the NCA?

Reference:

Lobell, D.B., W. Schlenker, and J. Costa-Roberts, 2011. Climate trends and global crop production since 1980. *Science*, 333**,** 616-620.

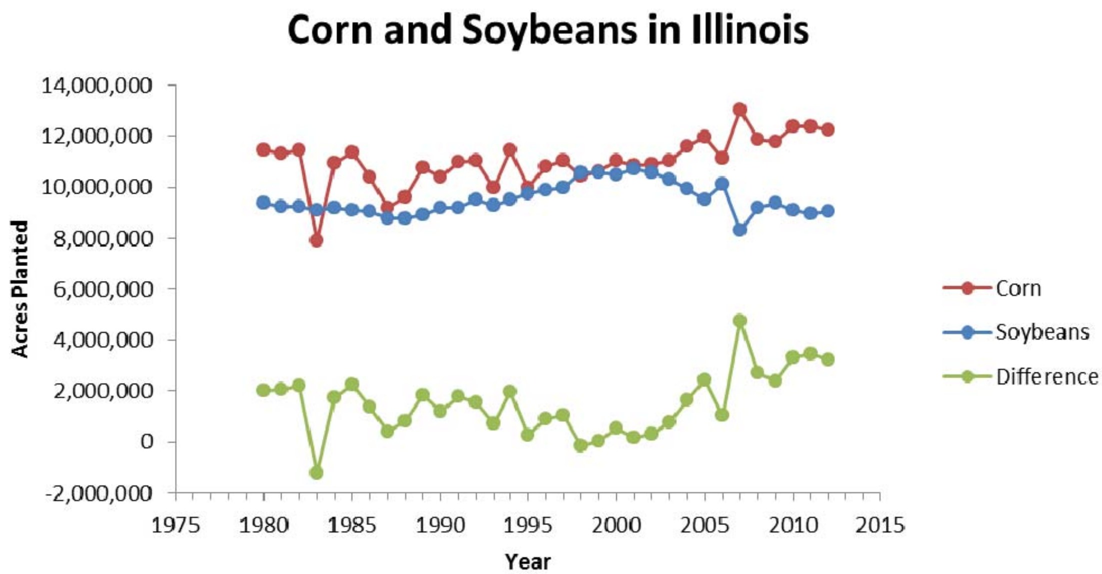
**Page 228, lines 31-49**

Internal inconsistency.

The first sentence is correct. Supply affects price. The second sentence is misleading. Price is the way markets allocate production, and the global production data shown above clearly demonstrate that global weather can't conspire to drastically lower production.

**Page 231, lines 10-18**

Nationwide, policy has become more important than climate in determining our food supply. This needs to be noted. Here's an example from Illinois:




*CAPTION: Annual acres planted of corn and soybeans in Illinois. (Data from the National Agricultural Statitics Service, USDA)*

Corn and soybean acreage was roughly equal around 2000. The upward trend in corn acreage is a result of George Bush's ethanol mandate, not climate. As the mandate will continue to increase, the effect will be even larger in the future, as long as the ag lobby has its way.

**Page 232, lines 1-14, Figure 6.4 and caption.**

This is highly misleading. No serious student believes that yields flat-lined in 2000. Instead, what these projections are, are detrended yields. The fact of the matter is that, even Lobell (2011) shows that the technological component of ag yields dwarfs the interannual weather (or climate) component. This caption should clearly indicate this. Why doesn't it?

**Page 235, lines 21**

If you really mean "perennial" crops, that's a very tiny portion of the world's supply. If you mean annual crops, this sentence is sooo 1970s. Genetic engineering speeds up

varietal adoption by 20 years, and gives a much more predictable result than “selective breeding for both plants and animals.”

**Page 238, line 15**

Why is kudzu a concern?

Yes it will take over open areas where it is not removed, but is there *any* demonstrable effect on southern crop yields as a result of it? That needs to be documented if this sentence is to stand.

**Page 238, lines 21-22**

Um…wasn’t the switch to no-till (which conserves soil and water) responsible for a massive jump in glyphosphate use—an order of magnitude, at least, compared to maybe a doubling from enhanced carbon dioxide levels that we aren’t likely to see for 100 years (at least—thanks to shale gas)? Which is more important to agriculture, climate and weather or technology? (The answer is obvious).

**Page 238, line 23**

Absurd.

Of course “a warmer world brings higher humidity in wet years.” Sounds like good weather for sugar cane to me. Seriously, I haven’t seen a demonstration that high dew points inhibit production—see Brazil, for example?

**Page 240, Figure 6.9**

You have to be kidding!

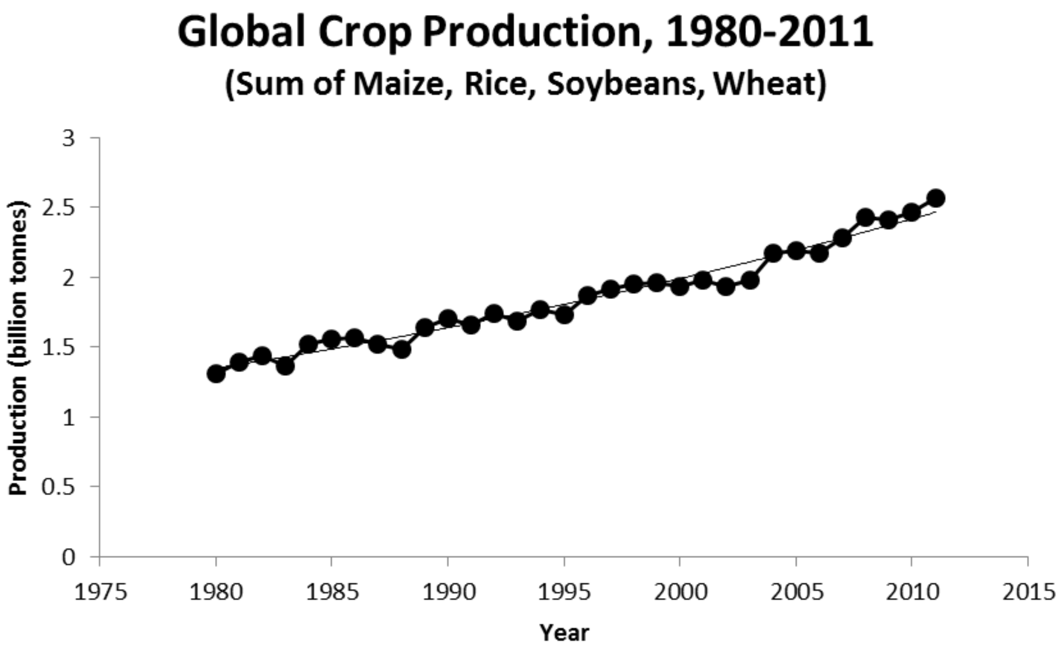
This is what gives your opponents big shotguns against a large barn. It is simply nonscience to take one point (Des Moines) and display a graph with an obvious variance of at least two days, with a trend of slightly over *one in 117 years*, as if that means something.

Remember that a “trend” that is not significant cannot be differentiated from a line with a slope of zero. In those cases, lines should never be drawn through data. This trend needs to be tested for significance. Normally I would do that but in this case *it’s your job*.

**Page 243, lines 12-42; Page 244, lines 1-7**

The section on Food Security (and Key Point #5 of the Chapter) is terribly misleading.

Food supply is a function of production minus consumption. This statement is supported in the text by Lobell (2011), a paper (mis)titled "Climate Trends and Global Crop Production since 1980." The paper is actually about yield, which is the amount per planted or harvested acre. (Production is the product of yield multiplied by acreage.) So, while it is obvious that changes in temperature and precipitation have detectable effects on yield, what really matters is how much is produced, which is reproduced below:




*CAPTION: Global annual total production from maize, rice, soybeans, and wheat. Data source: Food and Agriculture Organization, United Nations, available at http://faostat.fao.org/site/567/DesktopDefault.aspx?PageID=567*

An exponential fit ($Y = 2283.3 - 2.32 \text{ (year)} + .00059(\text{year})^2$) is significantly better than a linear one, which demonstrates how insignificant the climatic component of global food production is.

Where's the change in the "stability of food supplies"? As the climate has apparently become worse, negatively affecting yields (Lobell, 2011), productivity continues to increase exponentially.

What's missing here? There's simply little, if any, effect of year-to-year global climate variability. That's because the world food system is highly diverse, in terms of varieties grown and the climates in which they grow.

Further, there is a tremendous amount of reserve built into food supply because of the (stupid) diversion towards biofuels. In fact, the amount we divert to ethanol dwarfs the amount that is lost to climate. Lobell (2011) reported that, after allowing for the growth enhancement from atmospheric carbon dioxide, global average crop yields were reduced by a bit less than 1% (which is small compared to the amount that they increased because of technological advances) during the period 1980-2008.

But consider this. The U.S. produces about 36 percent of the world's corn. And about 40 percent of U.S. corn is used to produce ethanol for use as a gasoline substitute instead of being consumed by humans or animals. Globally, corn makes up 30 percent of total worldwide production of the four crops studied by Lobell's group.

And even this less than 1 percent impact was described by Lobell et al. as perhaps being "overly pessimistic" because it did not fully incorporate long-term adaptive farming responses to changing climate conditions (i.e., farmers are not as dumb as statistical models make them out to be).

What this means is that even under overly pessimistic scenarios, we still currently burn more than 4 times as much grain as climate change has taken away. Thinking about this in future terms, if we observe *twice* as much climate change from 2010 through 2038 as we did from 1980 to 2008 (Lobell's study period), all we would have to do is stop burning *half* as much ethanol as we do now to make up for the *entire* global climate-related crop reduction.

Therefore, climate is an irrelevant overlay on world food supply for the foreseeable future. If we really need the food, just stop the stupid conversion to ethanol.

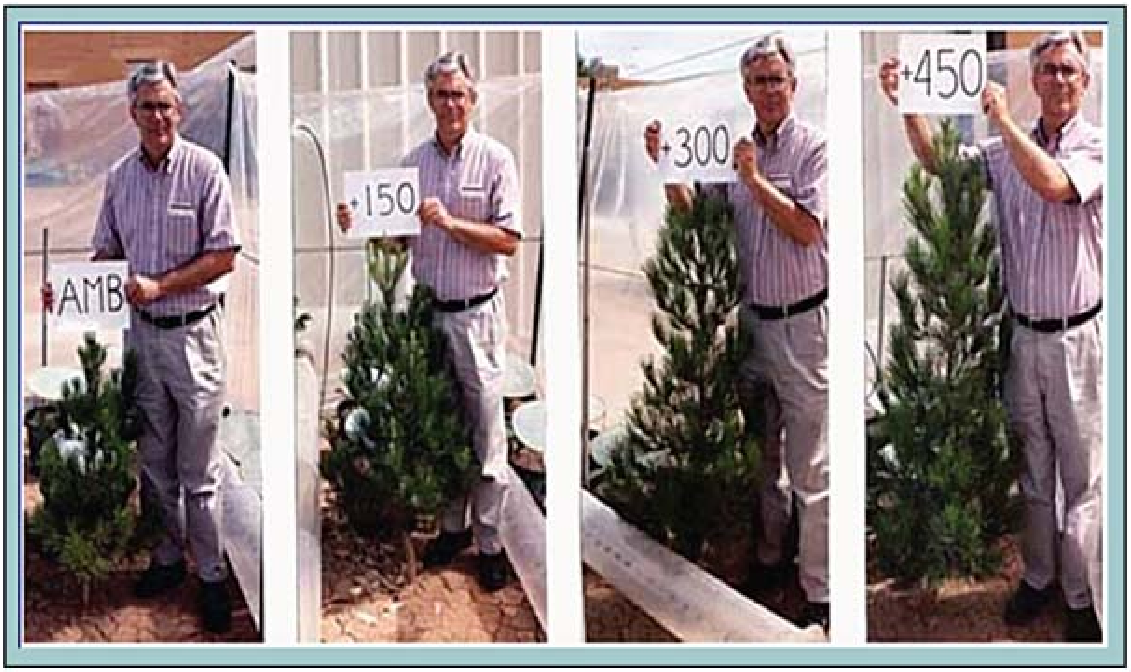
Obviously there is a lot of wastage in the world food supply from this stupid policy and any statement about the "stability of food supply" and "food security" challenges, is a result of the farm lobby and not climate. Why is this missing from the Assessment?

Reference:

Lobell, D.B., W. Schlenker, and J. Costa-Roberts, 2011. Climate trends and global crop production since 1980. *Science*, 333**,** 616-620.

**Page 244, lines 8-14, Figure 6.10 and caption**

Congratulations for rescuing this misleading chestnut from the 2009 report! It's only missing one thing: a crop. I suggest you substitute this one showing two perennial crops, Sherwood Idso and Eldarica Pine, under conditions of enhanced carbon dioxide.



*CAPTION: Eldarica pines grown in increasing concentration of carbon dioxide clearly show growth enhancement.*

**Traceable Account Page 245.**

**Description of evidence base**

Inaccurate citation.

The Assessment says:

> Evidence that climate change will have impacts on crops and livestock is based on numerous studies and is incontrovertible (….Lobell et al., 2011)

Here's what Lobell et al., actually say:

> However, we do not directly estimate the full set of adaptation possibilities that might occur in the long term under climate change (8). For this reason, we prefer to view these not as predictions of actual impacts, but rather as a useful measure of the pace of climate change in the context of agriculture.

Note that the Assessment is in the future tense and Lobell et al., say that they do not view their findings as predictions, but "rather as a useful measure of the pace of climate change in the context of agriculture." Text should be changed to reflect what Lobell et al. actually said.

## Chapter 7 Forestry

### General Comment

This is a remarkably thin chapter, with little reference to a voluminous literature on growth enhancement that in general is in evidence from NDVI data. While the chapter is correct that this does not apply to many places in the western US (and southern Alaska), there is little doubt that there is a substantial greening of the eastern 2/3's of the lower 48 states. Why is so little attention paid to the possible causation of this salutary change?

Does that alter the narrative?

### Page 266, lines 6-12

Here is a typical paragraph that simply ignores a voluminous literature that can at least in part explain why at least the eastern 2/3 of the lower 48 states is greening so rapidly.

Everything else being equal, rising temperatures and declining moisture availability, i.e., heat and drought, will lead to decreased tree growth (Karl et al., 2009). However, the increase in the atmosphere's $CO_2$ concentration ameliorates and can compensate for these two phenomena by simultaneously increasing the optimal temperature for photosynthesis (Jurik et al., 1984; Long, 1991; McMurtrie and Wang, 1993) and the efficiency with which trees use water (Leal et al., 2008; Wyckoff et al., 2011; Brienen et al., 2011). The beneficial impacts of the rise in the air's $CO_2$ content is demonstrated by the results of several studies of temperate trees (Tognetti et al., 1998; Paoletti et al., 2007; Wyckoff and Bowers, 2010) and boreal trees (Peltola et al., 2002; Bergh et al., 2003; Kostiainen et al., 2004). And this $CO_2$-induced productivity stimulation is experienced by trees that are also experiencing water insufficiency (Knapp et al., 2001; Tognetti et al., 2002; Soule and Knapp, 2006) and very old age (Phillips et al., 2008; Laurance et al., 2009; Lewis et al., 2009).

Very little attention is paid to the importance of management with regard to insect outbreaks, particularly Western Pine Beetle.

Pine bark beetles are endemic over most of the continental US, and this endemicity results in sporadic (and sometimes severe and widespread) outbreaks. These create a patchy forest distribution that favors ecosystem diversity. The overlay of more favorable climate conditions (warmer winters) increases the likelihood of severe outbreaks, such as those currently occurring in the Northwest.

Severe outbreaks simply cannot be stopped in a heavily infested forest. However, management of non-infested areas greatly reduces the likelihood of a severe outbreak (Leatherman et al., 2011).

The severe dieback of extensive stands of Northwest forest has a counterintuitive effect on severe crown fires. While it is a "rural legend" that these large areas of dead trees provide more fuel in an already fire-prone environment (a myth that the 2009 iteration of this report also uncritically propagated), in fact, modern research shows that pine beetle-killed forests result in *less* fuel to burn and actually suppress severe fires (Simard et al., 2011).

References:

Bergh, J., Freeman, M., Sigurdsson, B., Kellomaki, S., Laitinen, K., Niinisto, S., Peltola, H. and Linder, S. 2003. Modelling the short-term effects of climate change on the productivity of selected tree species in Nordic countries. *Forest Ecology and Management,* 183, 327-340.

Brienen, R.J.W., Wanek, W. and Hietz, P. 2011. Stable carbon isotopes in tree rings indicate improved water use efficiency and drought responses of a tropical dry forest tree species. *Trees,* 25, 103-113

Jurik, T.W., Weber, J.A. and Gates, D.M. 1984. Short-term effects of $CO_2$ on gas exchange of leaves of bigtooth aspen (*Populus grandidentata*) in the field. *Plant Physiology,* **75**, 1022-1026.

Karl, T.R., Melillo, J.M. and Peterson, T.C. 2009. *Global Climate Change Impacts in the United States*. Cambridge University Press, Cambridge, United Kingdom.

Knapp, P.A., Soule, P.T. and Grissino-Mayer, H.D. 2001. Post-drought growth responses of western juniper (*Juniperus occidentalis* var. *occidentalis*) in central Oregon. *Geophysical Research Letters,* 28, 2657-2660.

Kostiainen, K., Kaakinen, S., Saranpaa, P., Sigurdsson, B.D., Linder, S. and Vapaavuori, E. 2004. Effect of elevated [$CO_2$] on stem wood properties of mature Norway spruce grown at different soil nutrient availability. *Global Change Biology,* 10, 1526-1538.

Laurance, S.G.W., Laurance, W.F., Nascimento, H.E.M., Andrade, A., Fearnside, P.M., Rebello, E.R.G. and Condit, R. 2009. Long-term variation in Amazon forest dynamics. *Journal of Vegetation Science,* 20, 323-333.

Leal, S., Eamus, D., Grabner, M., Wimmer, R. and Cherubini, P. 2008. Tree rings of *Pinus nigra* from the Vienna basin region (Austria) show evidence of change in climatic sensitivity in the late 20th century. *Canadian Journal of Forest Research,* 38, 744-759.

Leatherman, D.A., et al., 2011. Mountain Pine Beetle, Colorado State University Extension, http://www.ext.colostate.edu/pubs/insect/05528.html

Lewis, S.L., Lopez-Gonzalez, G., Sonke, B., Affum-Baffoe, K., Baker, T.R., Ojo, L.O., Phillips, O.L., Reitsma, J.M., White, L., Comiskey, J.A., Djuikouo K., M.-N., Ewango,

C.E.N., Feldpausch, T.R., Hamilton, A.C., Gloor, M., Hart, T., Hladik, A., Lloyd, J., Lovett, J.C., Makana, J.-R., Malhi, Y., Mbago, F.M., Ndangalasi, H.J., Peacock, J., Peh, K. S.-H., Sheil, D., Sunderland, T., Swaine, M.D., Taplin, J., Taylor, D., Thomas, S.C., Long, S.P. 1991. Modification of the response of photosynthetic productivity to rising temperature by atmospheric $CO_2$ concentrations: Has its importance been underestimated? *Plant, Cell and Environment,* 14, 729-739.

McMurtrie, R.E. and Wang, Y.-P. 1993. Mathematical models of the photosynthetic response of tree stands to rising $CO_2$ concentrations and temperatures. *Plant, Cell and Environment,* 16, 1-13.

Paoletti, E., Seufert, G., Della Rocca, G. and Thomsen, H. 2007. Photosynthetic responses to elevated $CO_2$ and $O_3$ in *Quercus ilex* leaves at a natural $CO_2$ spring. *Environmental Pollution,* 147, 516-524.

Peltola, H., Kilpelainen, A. and Kellomaki, S. 2002. Diameter growth of Scots pine (*Pinus sylvestris*) trees grown at elevated temperature and carbon dioxide concentration under boreal conditions. *Tree Physiology,* 22, 963-972.

Phillips, N.G., Buckley, T.N. and Tissue, D.T. 2008. Capacity of old trees to respond to environmental change. *Journal of Integrative Plant Biology,* 50, 1355-1364.

Simard, M., et al., 2011. Do mountain pine beetle outbreaks change the probability of active crown fire in lodgepole pine forests? *Ecological Monographs*, 81(1), 3-24

Soule, P.T. and Knapp, P.A. 2006. Radial growth rate increases in naturally occurring ponderosa pine trees: a late-20th century $CO_2$ fertilization effect? *New Phytologist,* 171, 379-390.

Tognetti,R., Johnson, J.D., Michelozzi, M. and Raschi, A. 1998. Response of foliar metabolism in mature trees of *Quercus pubescens* and *Quercus ilex* to long-term elevated $CO_2$. *Environmental and Experimental Botany,* 39, 233-245.

Tognetti, R., Raschi, A. and Jones M.B. 2002. Seasonal changes in tissue elasticity and water transport efficiency in three co-occurring Mediterranean shrubs under natural long-term $CO_2$ enrichment. *Functional Plant Biology,* 29, 1097-1106.

Votere, R. and Woll, H. 2009. Increasing carbon storage in intact African tropical forests. *Nature,* 457, 1003-1006.

Wyckoff, P.H. and Bowers, R. 2010. Response of the prairie-forest border to climate change: impacts of increasing drought may be mitigated by increasing $CO_2$. *Journal of Ecology,* 98, 197-208.

**Page 266, lines 14-28**

No mention of important changes in tree demographics that are in an opposite sense to what is being reported in this paragraph.

Johnson and Abrams (2009) explored growth rate (basal area increment, BAI) relationships across age classes (from young to old) for eight tree species commonly found throughout the eastern United States. They note,

> a remarkable finding of this study is that even the oldest trees of several species had slow but increasing BAI values, which continued throughout the life of most trees…[which] contradicts the sigmoidal growth model that predicts growth rate should plateau and then decline, as middle age trees approach old age,

and

> over the last 50-100 years, younger trees within a species grew faster than did the older trees when they were of the same respective age.

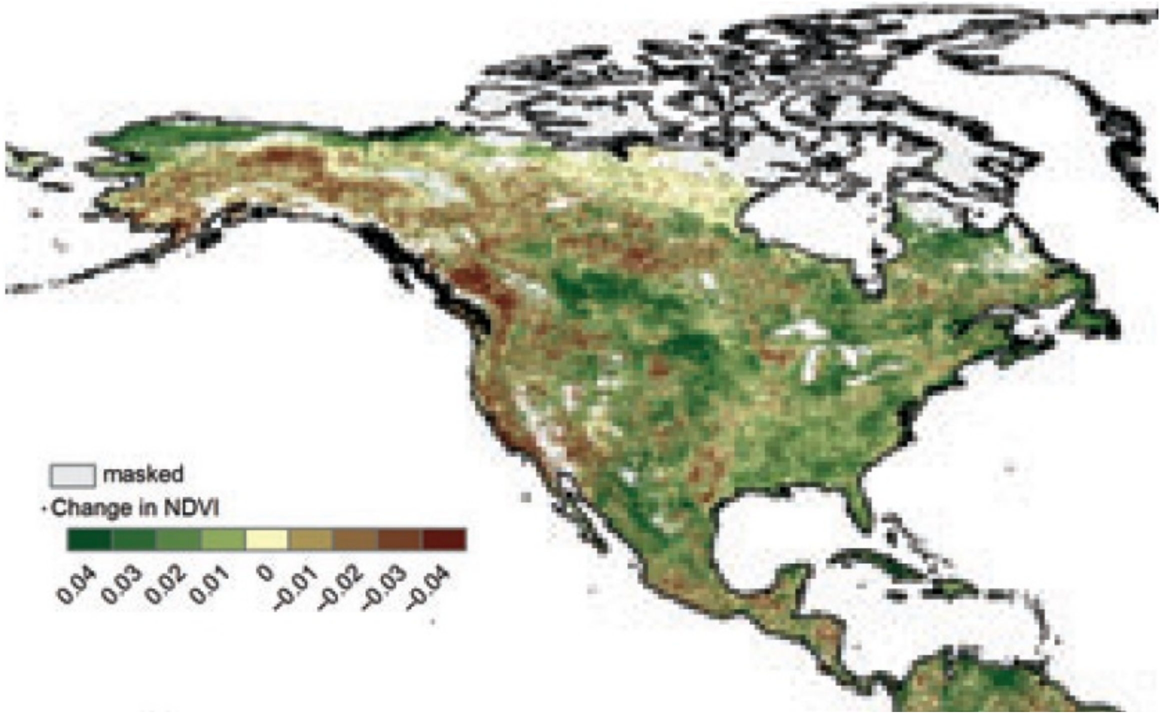
Knapp and Soule (2011) also found this to be the case with ponderosa pine trees in the USA's northern Rocky Mountains.

Johnson and Abrams (2009) wrote that their finding "may be due to a stimulatory effect of anthropogenic global change defined in the broadest sense," including "increased $CO_2$ levels, warming temperatures, increased precipitation, and changes in precipitation chemistry," while noting that "yearly average temperatures, atmospheric $CO_2$ and nitrogen levels have increased in the eastern US." Knapp and Soule went further, stating that "old-growth ponderosa pine forests of the northern Rockies have likely benefited from the effects of increased atmospheric $CO_2$ since the mid-20th century," additionally noting that "the benefits increase with tree age."

How could this paragraph ignore this?

There is an extensive literature to resolve the speculation here, as well as an integrative measure of forest health/growth, namely the NDVI data published in various sources mainly by Mynini and Nemani at BU.

Here's a blowup of the most recent (de Jong et al., 2012) version we could find. While this paper notes that there is a tendency for a decline in the greening and an increase in the "browning" over time on a global scale, the draft is about climate change impacts *in the United States*.




*CAPTION: Global greening and browning in terms of normalized difference vegetation index changes between 1982 and 2008 (adapted from deJong et al., 2012)*

This section indicates a general tendency towards declining forest health/growth in the western US from climate change, and similar effects in the eastern US from pollution. Obviously, whatever is being forced in the eastern 2/3's of the US is associated with greening, not browning. This paragraph needs at least to note this.

References:

De Jong, R., et al., 2012. Trend changes in global greening and browning: contribution of short-term trends to longer-term change. *Global Change Biology*, 18, 642-655.

Johnson, S.E. and Abrams, M.D. 2009. Age class, longevity and growth rate relationships: protracted growth increases in old trees in the eastern United States. *Tree Physiology,* 29, 1317-1328.

Knapp, P.A. and Soule, P.T. 2011. Increasing water-use efficiency and age-specific growth responses of old-growth ponderosa pine trees in the Northern Rockies. *Global Change Biology,* 17, 631-641.

**Page 267, lines 10-17**

Reiterate comment from Page 266, lines 6-12.

**Page 274, lines 1-41, Bioenergy section**

Wow. Deforestation doesn't contribute to an increase in atmospheric carbon dioxide? That seems to be the sense here. Whether wood is burned (oxidized) or decomposed (oxidized, slower) the result is largely the same. This section might as well be extolling the (nonexistent) virtues of ethanol; it's about that logical.

We guess there are similarities between the two: both are subsidized buy-offs of large numbers of nominally republican landowners, both contribute atmospheric carbon dioxide, both will only supply a small amount of energy, and both cost much more than the alternatives.

## Chapter 8 Ecosystems

### General Comments

This chapter is significantly better than most of those reviewed here, and stands in sharp contrast to the atrocious and politically-driven polemic that is the Human Health section (Chapter 9). Perhaps this might be due to the fact that CLA Peter Groffman received his BA at UVa which at the time provided appropriate intellectual diversity.

The intro section, before expansion of the first Key Message, is significantly better than most others in the overall assessment, being quite straightforward about the limitations of our understanding at this point in time (and the potential continued limitations).

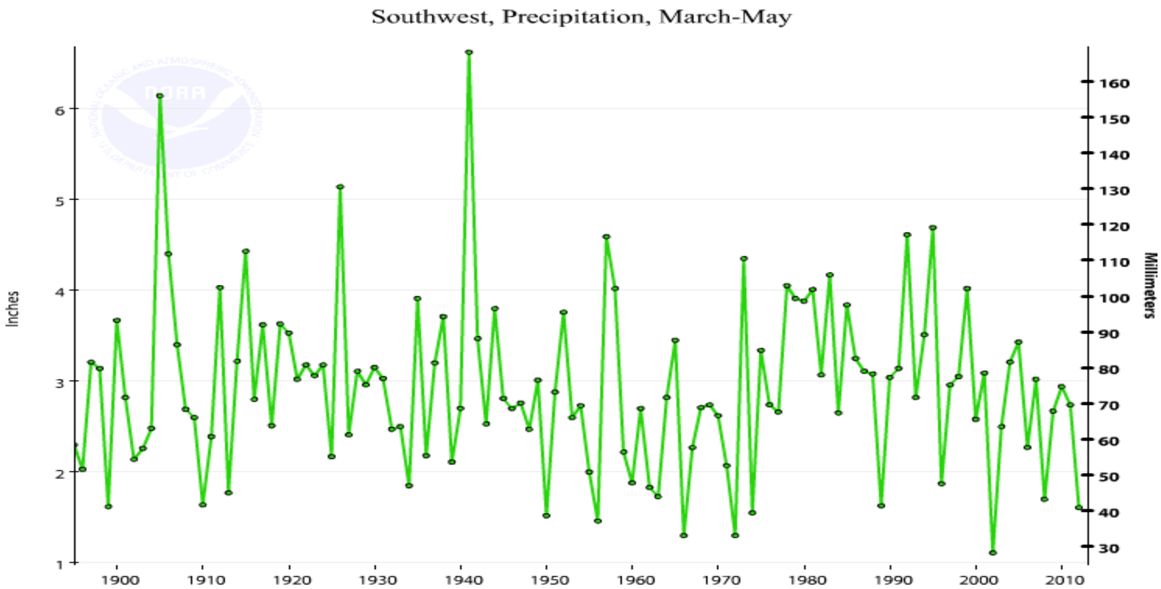
**Page 292, lines 21-25**

Usage of 40% of input in 25% of our watersheds, compared to 76% in the Colorado River basin, suggests that considerable additional capacity exists around the country. This is particularly true in the Southeast, where an average annual rainfall in excess of four feet per year means that any chronic and increasing supply limitations are driven by politics and NIMBY, and not the climate. Groffman's old haunts in Charlottesville are an archetypical example of this process.

**Page 292, lines 25-26**

"…lower spring precipitation".

Actually no change, according to NCDC. Also, the spring average of slightly under three inches is by far the lowest seasonal average in the region, and not a particularly important contributor to the annual flow. There is no significant overall trend in both spring precipitation and the PDSI histories (precipitation history according to NCDC shown below).




*CAPTION: March-May precipitation in the "Southwest" region (data source: NCDC Climate at a Glance).*

**Page 293, lines 23-25**

Implications not noted.

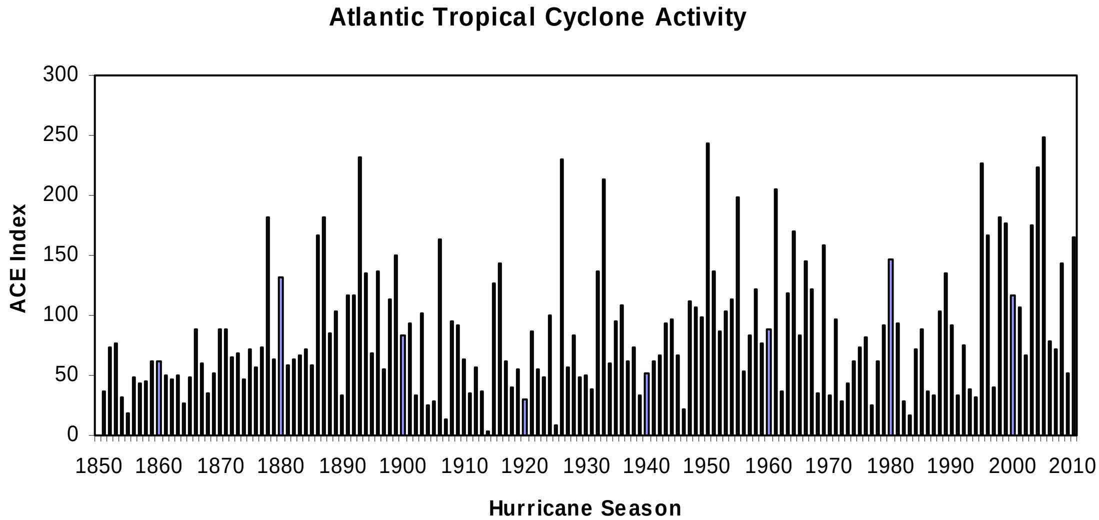
A severe 1994 outbreak of cryptosporidium is thought to have been responsible for at least 54 deaths in Milwaukee, but this was due to abnormally high concentrations that remained in the water after treatment.  As cryptosporidium is present in 17% of sampled U.S. drinking water supplies (Rose et al., 1991), and the lack of any evidence for large scale endemicity indicates outbreaks are more a result of treatment error rather than climatic change. Text should be changed to reflect this.

Reference:

Rose, J.B., C.P. Gerba, and W. Jakubowski, 1991. Survey of Potable Water-Supplies for Cryptosporidium and Giardia. *Environment Science & Technology*, 25, 1393-1400.

**Page 295, lines 6-16, Figure 8.2 and caption**

What is the purpose of this?  If it is to say that estuaries like Pamlico sound are more subject to algae blooms because of increasing hurricane activity, that's a false conflation, as hurricane activity is not increasing, and shows no relationship whatsoever to temperature.




*CAPTION: Accumulated Cyclone Energy (ACE) index for the Atlantic Basin from 1851 through 2010. There is obviously no relationship to global temperature. Data available at http://www.aoml.noaa.gov/hrd/tcfaq/E11.html.*

If it is meant to say that hurricane activity will increase in the future in this region, the literature is very conflicted on that. The consensus is still evolving as to how anthropogenic climate change will alter the characteristics of Atlantic basin tropical cyclones. There is growing evidence that the frequency of Atlantic basin tropical cyclones will be little changed, but that some storms may become more intense, although the preferred tracks of storms may be altered in such a way as to reduce the threat of a U.S. landfall (e.g., Wang et al., 2011). However, such changes are not anticipated to emerge above the level of natural noise until very late in this century (Michaels et al., 2005).

References:

Michaels, P. J., P. C. Knappenberger, and C. W. Landsea, 2005. Comments on "Impacts of CO2-Induced Warming on Simulated Hurricane Intensity and Precipitation: Sensitivity to the Choice of Climate Model and Convective Scheme". *Journal of Climate*, 18, 5179-5182.

Wang, C., L. Hailong, S-K. Lee, and R. Atlas, 2011. Impact of the Atlantic warm pool on United States landfalling hurricanes. *Geophysical Research Letters*, 38, L19702, doi:10.1029/2011GL049265.

**Page 297, lines 9-11**

Nothing unprecedented in Alaska.

Kaufmann et al. (2004) noted that for 10-12kyr ybp that Alaskan temperatures were 1.6 +/- 0.8°C higher than the 20th century average, which makes the current era no more than similar at best. Worth noting here.

Reference:

Kaufmann, D.S., et al., 2004. Holocene thermal maximum in the western Arctic (0–180°W). *Quaternary Science Reviews*, 23(5–6), 529-560.

**Page 297, lines 40-43, page 298, lines 1-3**

This makes it sound like climate is *the* driver of the massive Canadian bark beetle epidemic according to Raffa et al., 2008. This neglects the importance of irrational fire suppression. As Raffa et al., 2008 note:

> Management practices in some regions have also increased the abundance of susceptible hosts. Lodgepole pine–dominated forests cover much of the interior regions of western Canada, and most originated from stand-replacing wildfires. Because of aggressive fire suppression, the annual burned area declined from about 100,000 ha to less than 1000 ha over the last five decades (Taylor and Carroll 2004). This reduced rate of disturbance yielded forests in which nearly 70% of lodgepole pine was more than 80 years old, significantly greater than would be expected under a natural wildfire regime, and an overall threefold increase in the amount of susceptible pine, from 1910 to 1990…

The text should be revised to note this.

**Page 299, line 5-26, page 300, lines 1-18**

The section on Ecosystem-based management (pp 299-300) is very good, especially compared to much of the content of many of the other chapters in this report.

**Page 302-305, Box 2**

There should be some indicators of whether changes are salutary, negative, or neutral.

For example, with regard to biological response 2 (page 302, lines 10-11), Wiebe and Gerstmar (2010) write, “This suggests that there is no ecological mismatch linked to prey availability for Northern Flickers and that individuals could benefit by laying earlier if spring temperatures allow”, i.e., that this warming may be salutary.

Biological response 12 has *nothing* to do with observed or projected climate change and merely states if rain is above normal in the late summer, bison gain weight, and above normal midsummer precipitation decreased weight. That's like saying if there's little food that herbivores will lose weight, or that tree seedling survival is lower in dry years (biological response 21) Shocking! Remove them.

A problem is that no one (except fools like me) are going to look up your citations. So people (like The President) will hold forth about all these changes, not knowing they are really nugatory. For example, biological response 19 hardly merits a mention. From Kascher et al., 2011:

> "Although the absolute loss in optimal habitat for native species might regionally affect as many as 11 species, this is predicted only in relatively small areas (Fig. 5A)."

Suggestion: Remove.

## Chapter 9 Human Health

### General Comment

When I reviewed the 2009 draft Assessment, I stated that in my long career, of the many such documents I had reviewed, it was "by far the worst."

That was then, this is now. The Human Health chapter is the worst single chapter I have ever read on climate change. It is littered with statements of "fact" that are easily challenged by the simplest observations. Here is an example from page 349:

> …some patients with mental illness are especially susceptible to heat (ref). Suicide varies seasonally (ref), suggesting potential climate impacts on depression.

OK. *Are people more depressed in the South?* Silly me, I was under the impression that Seasonal Affective Disorder is related to cold temperatures and short days. I stand corrected.

It goes on:

> Dementia is a risk factor for hospitalization and death during heat waves (ref). Patients with severe mental illness such as schizophrenia are at risk during hot weather related both to their illness (ref) and their medications (ref).

*More schizophrenia is expressed in the south and in Arizona?*

Further,

> Additional potential mental health impacts, less well understood [!!!-eds], include distress associated with environmental degradation (refs), and the anxiety and despair that knowledge of climate change might elicit in some people (ref).

You might add, *"caused by scientists pushing insane hypotheses as facts."* How depressing!

**Page 334, lines 1-3**

This sentence is completely meaningless. Delete.

**Page 334, lines 3-6**

You continue to miss the positive externalities associated with climate change, like the fact that we have doubled our life spans in societies that were largely powered by fossil fuels that have slightly raised mean global temperature.

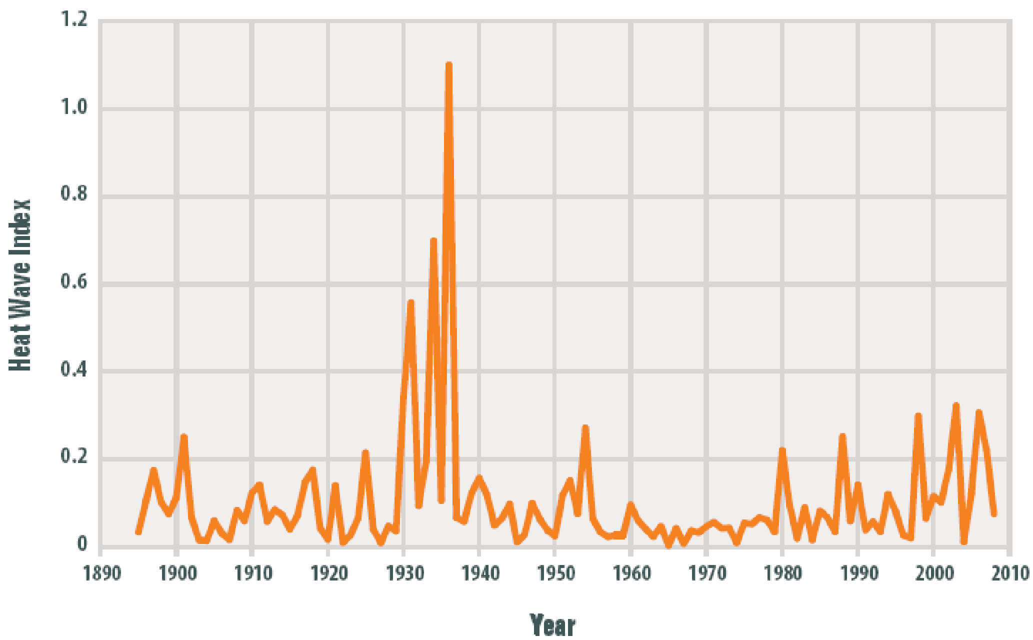
Doubling the lifespan of, say, two billion people, is equivalent to saving one billion lives. This dwarfs any negative effects of climate change. Me, I'll take 85 quality years versus 43 with a price of one degree Celsius, which I can counter simply by moving from the city into the burbs.

**Page 334, lines 9-15**

Multiple system failure here with regard to extreme events! Please correct in light of the following:

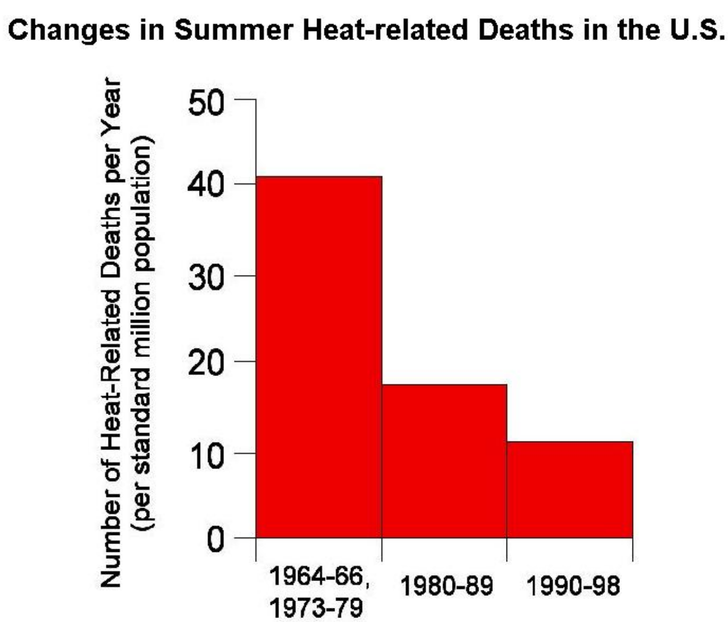
With regard to the effects of "increasingly frequent and intense extreme heat," mortality from heat waves declines as heat wave frequency increases. The NCA claim to the contrary is not borne out in the empirical data. From the 1970s to the 1990s, population-standardized heat-related deaths declined across the U.S. (despite the great Chicago heat wave of 1995) (e.g., Davis et al., 2003). Between 1979–1992 to 1993–2006, the average annual death rates for excessive cold and excessive heat declined by 31% and 17%, respectively (Goklany, 2009).

Based on data from 1895 onward, heat waves in the U.S. peaked in the 1930s, according to the U.S. Climate Change Science Program (USCCSP, 2008). However, the latter notes that, "In contrast to the 1930s, the recent period of increasing heat wave index is distinguished by the dominant contribution of a rise in extremely high nighttime temperatures."




*CAPTION: Annual value of a U.S. national average "heat wave" index. Heat waves are defined as warm spells of 4 days in duration with mean temperature exceeding the threshold for a 1 in 10 year event (US EPA, 2010).*

Several studies find that heat waves for the most part have become less deadly in urban areas. Davis et al. (2003) found that from 1964 to 1998, heat-related deaths declined significantly for 19 of 28 U.S. metropolitan areas, as well as for the 28-city average. Kalkstein et al. found a reduction in mortality attributable to excessive heat events from 1996 to 2004 for 40 major U.S. metropolitan areas.

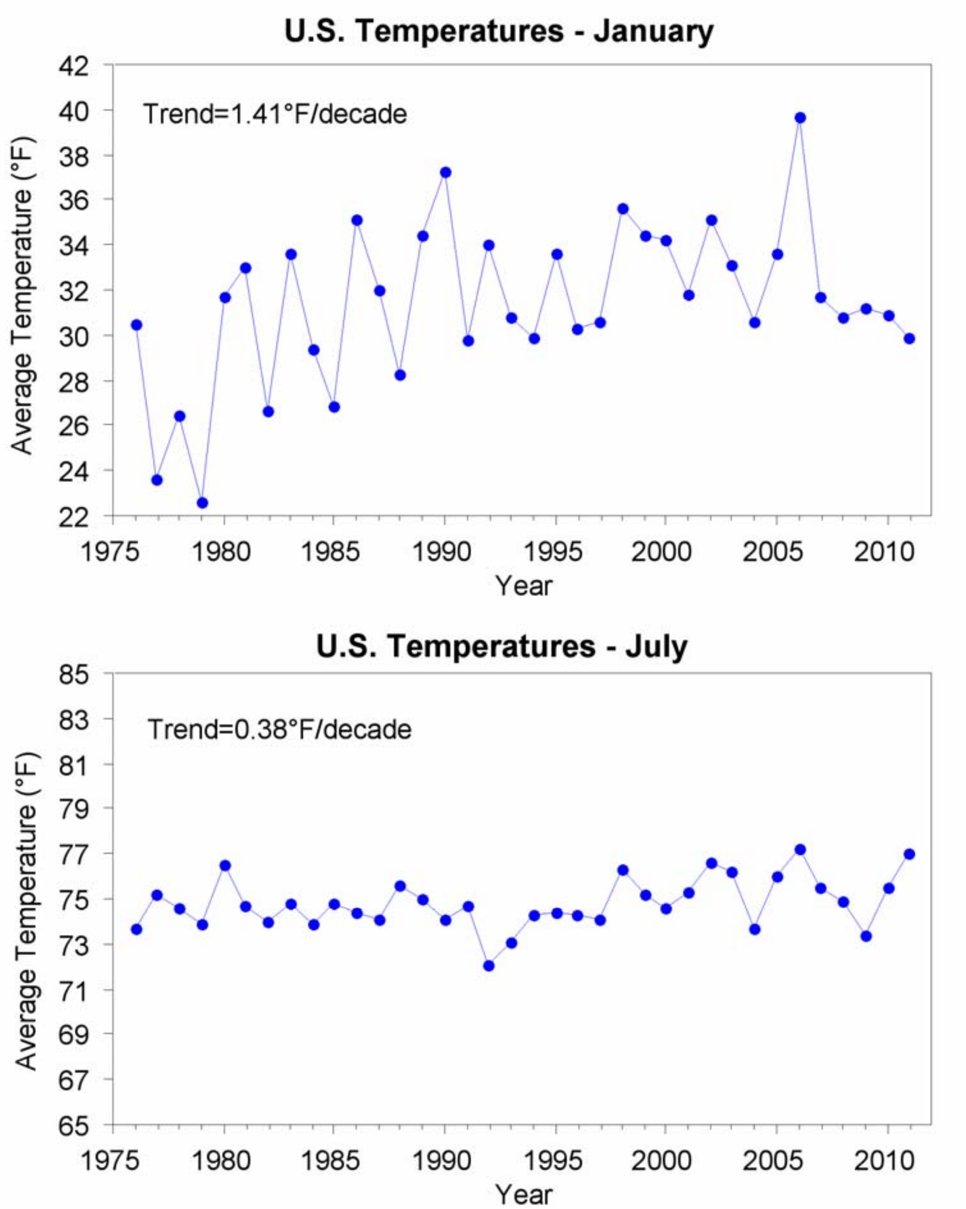


*CAPTION: Average heat-related mortality in U.S. urban areas has declined nationwide (Davis et al., 2003); subsequent research shows this trend continues into the 21st century (Kalkstein et al., 2011).*

The Davis et al. (2003) study also shows that base heat wave mortality is much lower in urban areas where they are more frequent. Notably, the two cities with the lowest mortality, Tampa and Phoenix, have some of the oldest age-distributions in the world. Thus the subsequent (Page 350, lines 4-5) statement that "the elderly are more vulnerable to heat stress," while physiologically correct, is profoundly misleading. Not only it is quite clear that in affluent societies that adaptation to heat more than compensates for the relative inability of the elderly to tolerate very high temperatures, but also increasing vulnerability as the elderly population increases has little to do with climate *change*.

Further, greenhouse warming will warm the extreme cold of winter more than it will raise the high temperatures of summer. Greenhouse physics demonstrates that the cold, dry air of the winter must warm more than the hot, moist air of the U.S. summer. This is because the atmosphere's two main greenhouse gases, water and carbon dioxide, absorb some of the same infrared wavelengths emitted by the earth's surface. When both gases are in short supply (as they were in the necessarily dry winter air prior to the major emissions of carbon dioxide) an increment of either of them creates much more warming than a similar change in the moist warm air of summer. This logarithmic response of temperature to greenhouse gases at similar wavelengths has been known for over a hundred years.

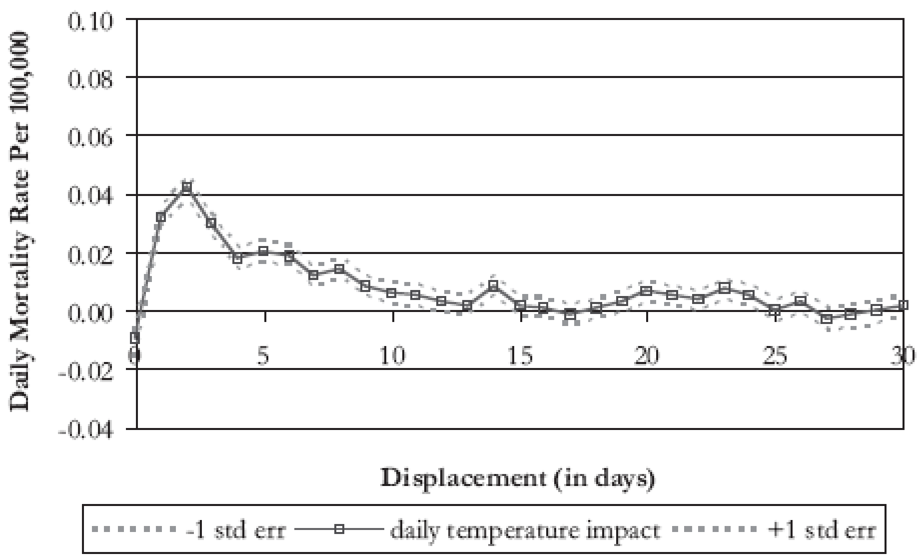
The reality of this can be demonstrated by comparing January and July temperatures over the US that are concurrent with the global warming that began in the mid-1970s. These are the two months that see the most extreme cold and warm excursions of the calendar year.



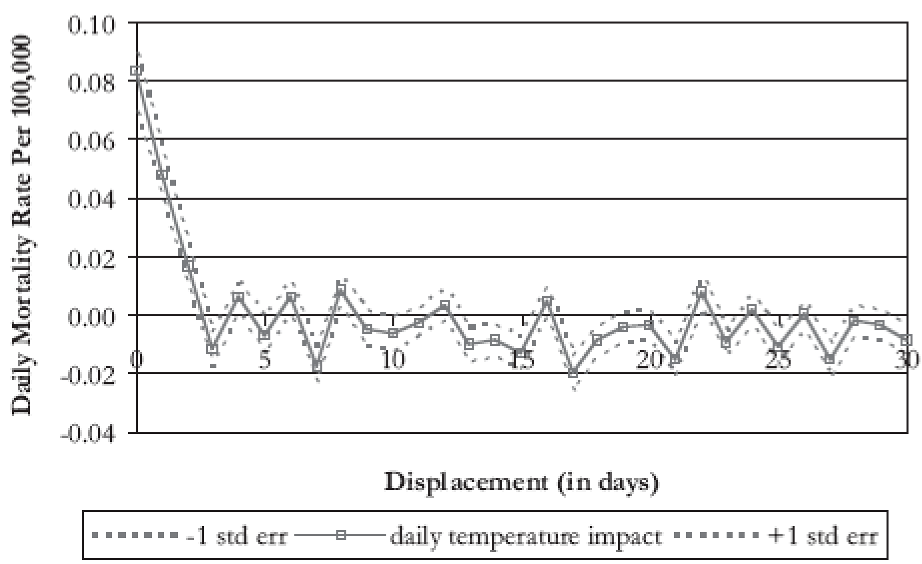

*CAPTION: January and July climatologically have the year's most extreme temperatures. These plots are coterminous US average temperature beginning in 1976, which is the beginning year for the second ("global") warming of the 20$^{th}$ century. It is very clear from this data that the extreme cold of winter has warmed approximately three times more than the extreme heat of summer (data source: NCDC).*

As the relative warming of extreme cold must be greater than the increase in extreme heat, and weather-related deaths from cold far exceed those from heat, greenhouse warming should therefore result in an overall decrease in temperature-related mortality.

**(A) Daily Mean Temperature Less than 30°F**




**(B) Daily Mean Temperature More than 80°F**




*CAPTION: Estimated effect of cold and hot temperature exposure on daily female all-cause mortality rates for 30 days following exposure (Deschenes and Moretti, 2009). Note that the number of abnormal deaths from extreme winter cold persists to day 15, while the death rate actually drops below normal three days after extreme heat, and the average anomaly remains negative through 30 days.*

References:

Davis RE, Knappenberger PC, Michaels PJ, Novicoff WM, 2003. Changing Heat-Related Mortality in the United States. *Environmental Health Perspectives*, 111, 1712–18.

Deschenes, O., and E. Moretti, 2009. Extreme weather events, mortality and migration. *Review of Economics and Statistics,* 91, 659-681.

Goklany, I., 2009. Deaths and death rates from extreme weather events: 1900-2008. Journal of the American Physicians and Surgeons, 14(4), 102-109.

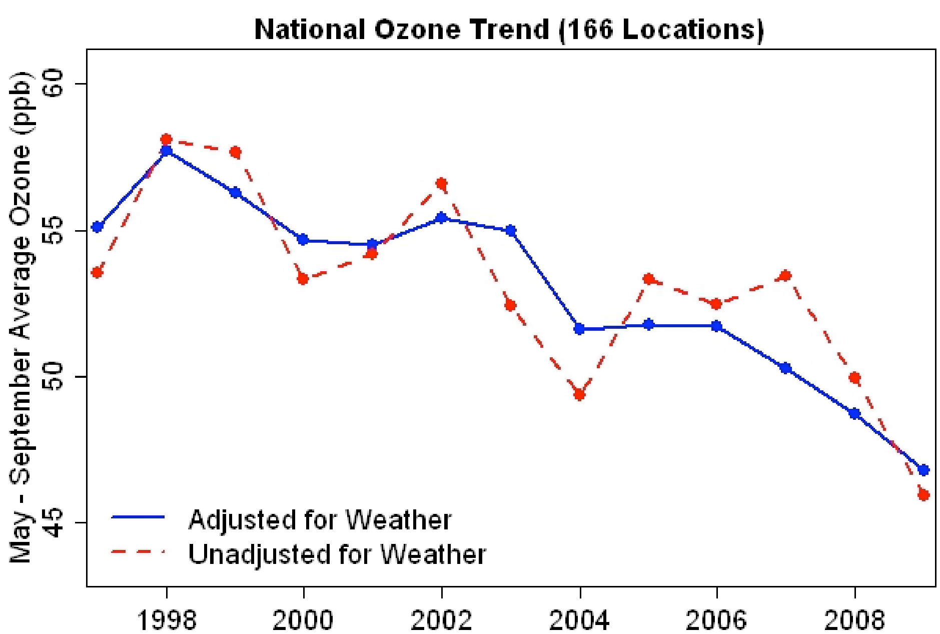
Kalkstein, L.S., Greene, S., Mills, D.M., and Samenow, J., 2011. An evaluation of the progress in reducing heat-related human mortality in major U.S. cities. *Natural Hazards*, 56 (1), 113-129.

U.S. Climate Change Science Program, 2008. Weather and Climate Extremes in a Changing Climate Final Report, Synthesis and Assessment Product 3.3.

U.S. Environmental Protection Agency, 2010. Climate Change Indicators in the United States, www.epa.gov/climatechange/indicators.html, updated from U.S. Climate Change Science Program's 2008 report: Synthesis and Assessment Product 3.3: Weather and climate extremes in a changing climate.

**Page 334, lines 22-23**

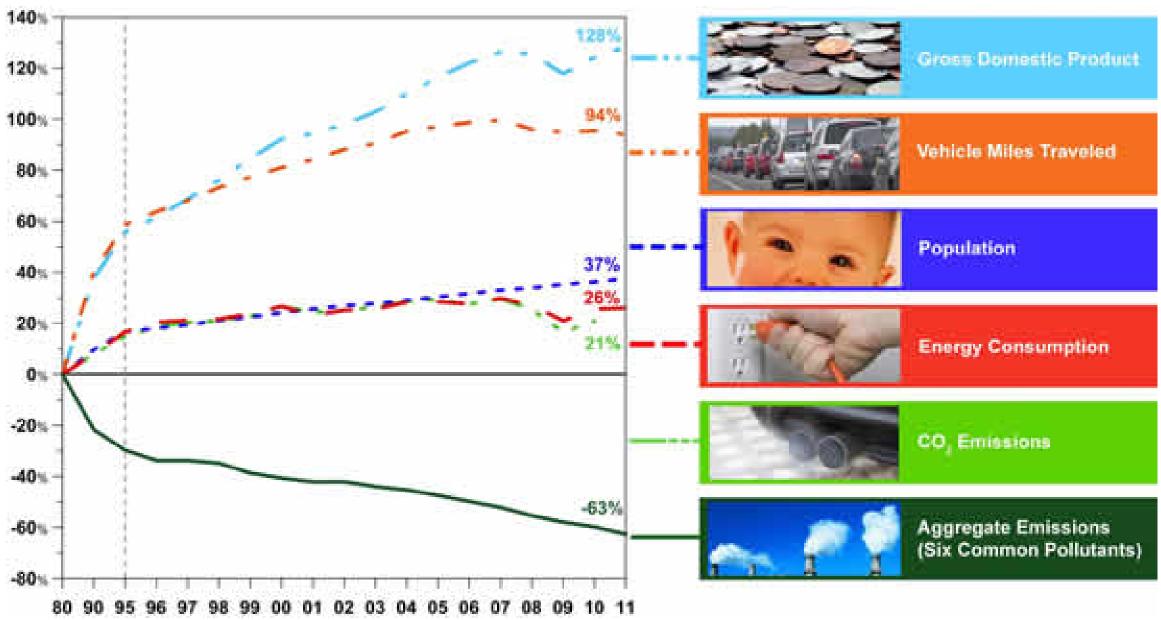
Forecast is not consistent with observations. Sentence could stay as long as it acknowledges the actual behavior of low-level ozone is opposite to what it asserts.




*CAPTION:*

*Both the observed trend in May–September ozone (red line) and the trend corrected for varying weather conditions (blue line) show a significant decline from 1997–2009 (EPA).*

In general, warming temperatures should increase the rates of the chemical reactions that create urban smog, ozone, and other noxious compounds, including $NO_x$. Nonetheless, as cities have warmed, air quality has improved. This has occurred despite major increases in economic activity and vehicular traffic in most cities. According to EPA estimates, total emissions of six major pollutants declined by more than 60% over that same time period.




*CAPTION: Despite an increasing population, energy consumption, and economic productivity, U.S. pollution emissions declined by 63% since 1980 (EPA).*

There are several reasons for this. Regulations regarding pollutants are certainly a major factor, and, despite increasing the rates of associated chemical reactions, climate change also plays a role. Pollutants tend to concentrate in stable air masses in which temperature increases with height above the surface. However, because the surface has been warming relative to the overlying atmosphere, the long-term tendency has been to destabilize the atmosphere, resulting in more vertical mixing and less concentrated pollutants. This trend is particularly true in cities (the areas of greatest pollution concern) because of the urban heat island effect in which cities are generally warmer than the surrounding rural areas. Furthermore, precipitation serves to wash pollutants out of the atmosphere. While nationally-averaged rainfall has increased, there is another increment that is strictly related to urban warming itself (e.g., Dixon et al., 2003) and so played a role in air quality improvements.

Given the historic trends in air quality, technology, and climate, it is highly likely that U.S. air quality in future decades will be even better than it is today and that the populace will be healthier and have an even longer life expectancy.

References:

Dixon, P. Grady, Thomas L. Mote, 2003. Patterns and Causes of Atlanta's Urban Heat Island–Initiated Precipitation. *Journal of Applied Meteorology*, 42, 1273–1284.

U.S. Environmental Protection Agency, Air Trends, http://epa.gov/airtrends/index.html

**Page 336, lines 1-12, Figure 9.2 and caption**

Why rag on ragweed? Yes it produces pollen that is allergenic, but also, from anecdotal reports (e.g. Illinois Wildflowers),

> Common Ragweed is very valuable to many kinds of wildlife. Honeybees have been observed collecting pollen from the male flowers, otherwise flower-visiting insects are not attracted to this plant. The caterpillars of several moths eat the foliage, flowers, or seeds, including *Schinia rivulosa* (Ragweed Flower Moth), *Synchlora aerata* (Wavy-Lined Emerald), *Tarachidia erastrioides* (Small Bird-Dropping Moth), *Tarachidia candefacta* (Olive-Shaded Bird-Dropping Moth), and others (see Moth Table). In my experience, some species of grasshoppers are quite abundant around colonies of Common Ragweed, probably because they eat the foliage and prefer the disturbed, open habitats where this plant occurs. Many upland gamebirds and granivorous songbirds are attracted to the oil-rich seeds (see Bird Table). Because the spikes of seeds often remain above snow cover, they are especially valuable to some of these birds during winter. The seeds are also eaten to some extent by the Thirteen-Lined Ground Squirrel, Meadow Vole, and Prairie Vole. The seeds are probably semi-digestible, thus some of them are likely distributed far and wide by these animals. On the other hand, the foliage is quite bitter, therefore mammalian herbivores do not often consume it…

Common Ragweed is a major cause of hay fever during the late summer and fall. Aside from this unfortunate characteristic, it has considerable ecological value to various birds and moths, and therefore it isn't necessarily desirable to destroy this plant on sight.

Reference:

Illinois Wildflowers, http://www.illinoiswildflowers.info/weeds/plants/cm_ragweed.htm

**Page 337, Figure 9.3 and caption**

Yes, pollen counts rise with increasing carbon dioxide—because it makes for a greener world, which is being observed (see many Myneni and Nemani and NASA NDVI references).

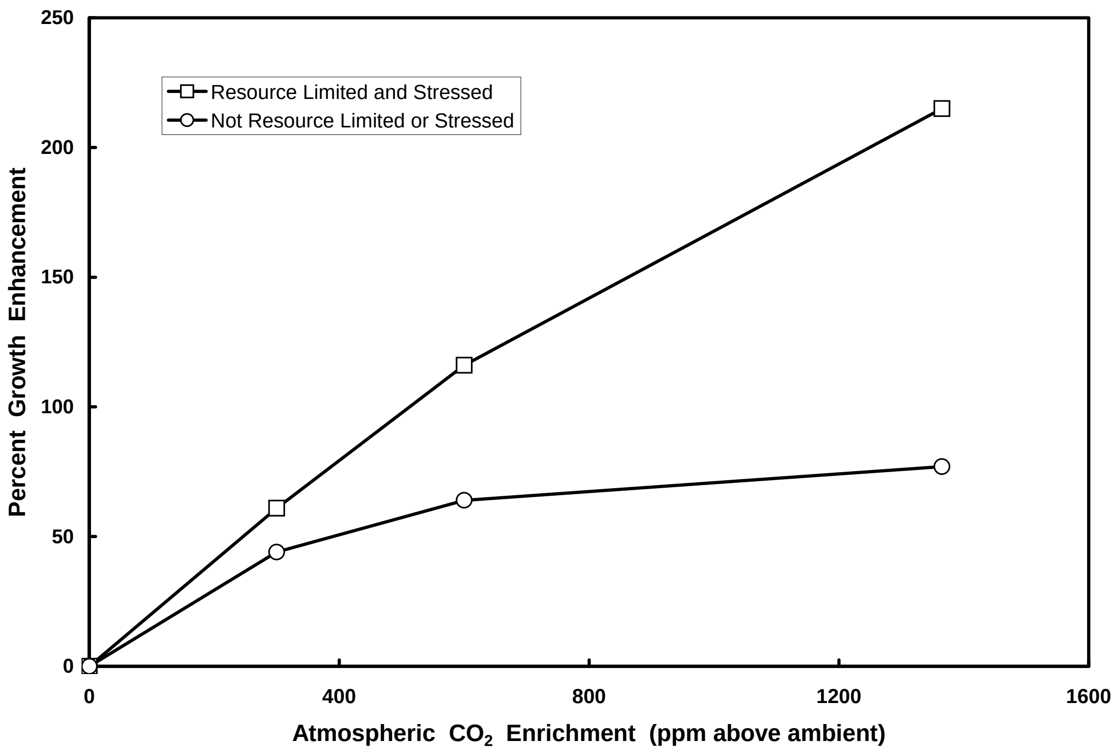
Rising temperatures and carbon dioxide concentration increase and/or decrease pollen production and the pollen season in some plants and/or others.

Because increased levels of CO2 enhance plant growth and higher temperatures lengthen the growing season, many plants and crops are blooming earlier and growing over a longer period of time. This is advantageous for certain commercial crops (e.g. grapes) that require a very long growing season to fully mature.

This enhanced plant growth also affects pollen. With an earlier onset of spring, this will shift the timing of the pollination of most plants. But the impact of higher temperatures and CO2 on pollen is uncertain, as some species of plant seem to produce more pollen given the longer growing season (e.g., Rasmussen, 2002), while others are negatively impacted (e.g., Matsui et al., 1997).

Higher levels of atmospheric $CO_2$ ameliorate, and sometimes fully compensate for, the negative influences of various environmental stresses on plant growth, including the stress of high temperature.

Atmospheric $CO_2$ enrichment has also been shown to help ameliorate the detrimental effects of several environmental stresses on plant growth and development, including high soil salinity (e.g., Azam et al., 2005), high air temperature (e.g., Aranjuelo et al., 2005) , low light intensity (e.g., Sefcik et al., 2006), high light intensity (e.g., Rasineni et al., 2011), UV-B radiation (e.g., Zhao et al., 2004), water stress (e.g., Robredo et al., 2007), and low levels of soil fertility (e.g., Barrett et al., 1998). Elevated levels of $CO_2$ have additionally been demonstrated to reduce the severity of low temperature stress **(**Boese et al., 1997), oxidative stress (e.g., Donnelly et al., 2005), and the stress of herbivory (e.g., Newman et al., 1999). In fact, the percentage growth enhancement produced by an increase in the air's $CO_2$ concentration is generally even *greater* under stressful and resource-limited conditions than it is when growing conditions are ideal.




*CAPTION. Percent growth enhancement as a function of atmospheric $CO_2$ enrichment in parts per million (ppm) above the normal or ambient atmospheric $CO_2$ concentration for plants growing under stressful and resource-limited conditions and for similar plants growing under ideal conditions. Each line is the mean result obtained from 298 separate experiments (Idso and Idso, 1994).*

Among the list of environmental stresses with the potential to negatively impact agriculture, the one that elicits the most frequent concern is high air temperature. In this regard, there is a commonly-held belief that temperatures may rise so high as to significantly reduce crop yields, thereby diminishing our capacity to produce food, feed, and fuel products. It has also been suggested that warmer temperatures may cause a northward shift in the types of crops grown by latitude that could have additional adverse impacts on agricultural production. However, frequently left out of the debate on this topic is the fact that the growth-enhancing effects of elevated $CO_2$ typically increase with rising temperature. For example, a 300-ppm increase in the air's $CO_2$ content in 42 experiments has been shown to raise the mean $CO_2$-induced growth enhancement from a value of zero at 10°C to a value of 100% at 38°C (Idso and Idso, 1994).

This increase in $CO_2$-induced plant growth response with increasing air temperature arises from the negative influence of high $CO_2$ levels on the growth-retarding process of photorespiration, which can "cannibalize" 40 to 50% of the recently-produced photosynthetic products of $C_3$ plants. Since this phenomenon is more pronounced at high temperatures, and as it is ever-more-inhibited by increasingly-higher atmospheric $CO_2$ concentrations, there is an increasingly-greater potential for atmospheric $CO_2$ enrichment to benefit plants as air temperatures rise.

A major consequence of this phenomenon is that the optimum temperature for plant growth generally rises when the air is enriched with $CO_2$. For a 300-ppm increase in the air's $CO_2$ content, in fact, several experimental studies have shown that the optimum temperature for growth in $C_3$ plants typically rises by 5°C or more (e.g., Cowling and Sykes, 1999). These observations are very important; for an increase of this magnitude in optimum plant growth temperature is greater than the largest air temperature rise predicted to result from a 300-ppm increase in atmospheric $CO_2$ concentration. Therefore, even the most extreme global warming envisioned by the Intergovernmental Panel on Climate Change will probably not adversely affect the vast majority of Earth's plants; for fully 95% of all plant species are of the $C_3$ variety. In addition, the $C_4$ and CAM plants that make up the rest of the planet's vegetation are already adapted to Earth's warmer environments, which are expected to warm much less than the other portions of the globe; yet even some of these plants experience elevated optimum growth temperatures in the face of atmospheric $CO_2$ enrichment (Chen et al., 1994).

Consequently, a $CO_2$-induced temperature increase will likely *not* result in crop yield reductions, nor produce a poleward migration of plants seeking cooler weather; for the temperatures at which nearly all plants perform at their optimum is likely to rise at the same rate (or faster than) and to the same degree as (or higher than) the temperatures of their respective environments. And other research indicates that even in the absence of a concurrent increase in atmospheric $CO_2$, plants may still be able to boost their optimum temperature for photosynthesis as the temperature warms (Gunderson et al., 2010).

References:

Aranjuelo, I., Irigoyen, J.J., Perez, P., Martinez-Carrasco, R. and Sanchez-Diaz, M. 2005. The use of temperature gradient tunnels for studying the combined effect of $CO_2$, temperature and water availability in $N_2$ fixing alfalfa plants. *Annals of Applied Biology,* 146, 51-60.

Azam, F., Aziz, F., Sial, M.H., Ashraf, M. and Farooq, S. 2005. Mitigation of salinity effects on *Sesbania aculeat*a L., through enhanced availability of carbon dioxide. *Pakistan Journal of Botany,* 37, 959-967.

Barrett, D.J., Richardson, A.E. and Gifford, R.M. 1998. Elevated atmospheric $CO_2$ concentrations increase wheat root phosphatase activity when growth is limited by phosphorus. *Australian Journal of Plant Physiology,* 25, 87-93.

Boese, S.R., Wolfe, D.W. and Melkonian, J.J. 1997. Elevated $CO_2$ mitigates chilling-induced water stress and photosynthetic reduction during chilling. *Plant, Cell and Environment,* 20, 625-632.

Chen, D.X., Coughenour, M.B., Knapp, A.K. and Owensby, C.E. 1994. Mathematical simulation of $C_4$ grass photosynthesis in ambient and elevated $CO_2$. *Ecological Modeling,* 73, 63-80.

Cowling, S.A. and Sykes, M.T. 1999. Physiological significance of low atmospheric $CO_2$ for plant-climate interactions. *Quaternary Research,* 52, 237-242.

Donnelly, A., Finnan, J., Jones, M.B. and Burke, J.I. 2005. A note on the effect of elevated concentrations of greenhouse gases on spring wheat yield in Ireland. *Irish Journal of Agricultural and Food Research,* 44, 141-145.

Gunderson, C.A., O'Hara, K.H., Campion, C.M., Walker, A.V. and Edwards, N.T. 2010. Thermal plasticity of photosynthesis: the role of acclimation in forest responses to a warming climate. *Global Change Biology,* 16, 2272-2286.

Idso, K.E., and Idso, S.B. 1994. Plant responses to atmospheric $CO_2$ enrichment in the face of environmental constraints: A review of the past 10 years' research. *Agricultural and Forest Meteorology,* 69, 153-203.

Matsui, T., Namuco, O.S., Ziska, L.H., Horie, T., 1997. Effects of high temperature and CO2 concentration on spikelet sterility in indica rice. *Field Crops Research*, 51, 213–219.

Newman, J.A., Gibson, D.J., Hickam, E., Lorenz, M., Adams, E., Bybee, L. and Thompson, R. 1999. Elevated carbon dioxide results in smaller populations of the bird cherry-oat aphid *Rhopalosiphum padi*. *Ecological Entomology,* 24, 486-489.

Rasmussen, A., 2002. The effects of climate change on the birch pollen season in Denmark. *Aerobiologia*, 18, 253–265.

Robredo, A., Perez-Lopez, U., Sainz de le Maza, H., Gonzalez-Moro, B., Lacuesta, M., Mena-Petite, A. and Munoz-Rueda, A. 2007. Elevated $CO_2$ alleviates the impact of drought on barley improving water status by lowering stomatal conductance and delaying its effects on photosynthesis. *Environmental and Experimental Botany,* 59, 252-263.

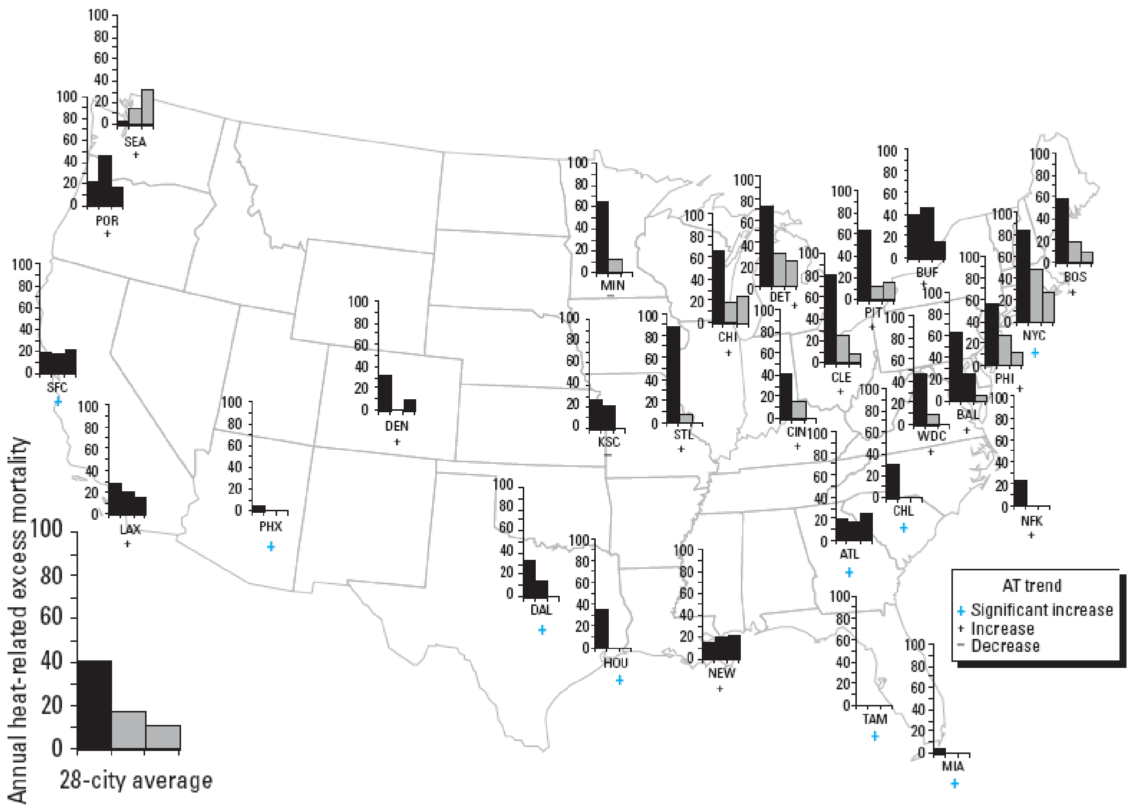
Sefcik, L.T., Zak, D.R. and Ellsworth, D.S. 2006. Photosynthetic responses to understory shade and elevated carbon dioxide concentration in four northern hardwood tree species. *Tree Physiology,* 26, 1589-1599.

Zhao, D., Reddy, K.R., Kakani, V.G., Mohammed, A.R., Read, J.J. and Gao, W. 2004. Leaf and canopy photosynthetic characteristics of cotton (*Gossypiuym hirsutum*) under elevated $CO_2$ concentration and UV-B radiation. *Journal of Plant Physiology,* 161, 581-590.

**Page 342, lines 1-13**

Assertions run counter to large data analysis. It is incumbent on NCADAC to counter the references and analysis in the following commentary.

America's cities predict the adaptation of society to prospective global warming. The urban heat island effect has raised averaged urban air temperatures by 2 to 5°F over the surrounding countryside and as much as 20° at night (Grimmond, 2007). This warming takes place gradually and is similar in magnitude to non-urban warming rates predicted for the 21st century from increasing atmospheric greenhouse gases. Cities and their residents are indeed testing whether or not global warming increases heat-related mortality.




*CAPTION: Population standardized heat wave related mortality is declining in almost all US cities and is lowest where in cities with elderly populations in which heat waves are most frequent (Davis et al., 2003).*

Heat waves for the most part have become less deadly in urban areas. From 1964 to 1998, heat-related deaths declined significantly for 19 of 28 U.S. metropolitan areas, as well as for the 28-city average (Davis et al., 2003). Since then, another study found a reduction in mortality attributable to excessive heat events from 1996 to 2004 for 40 major U.S. metropolitan areas (Kalkstein et al., 2011).

Baseline heat wave mortality is much lower in urban areas where they are more frequent (Davis et al., 2003). Notably, the two cities with the lowest such mortality, Tampa and Phoenix, have some of the oldest age-distributions in the world. The Assessment

statement "Many cities…have sustained dramatic increases in death rates following heat waves" is profoundly misleading in light of the totality of the data that is shown in the map and references above; it is quite clear that in affluent societies that adaptation to heat more than compensates for the relative inability of the elderly to tolerate very high temperatures.

References:

Davis, R. E., Knappenberger, P. C., Michaels, P. J., and Novicoff, W. M., 2003. Changing Heat-Related Mortality in the United States. *Environmental Health Perspectives*, 111, 1712–18.

Grimmond, S., 2007. Urbanization and global environmental change: local effects of urban warming. *Geographical Journal*, 171**,** 83-88.

Kalkstein, L.S., Greene, S., Mills, D.M., and Samenow, J., 2011. An evaluation of the progress in reducing heat-related human mortality in major U.S. cities. *Natural Hazards*, 56 (1), 113-129.

**Page 342, lines 14-22, Figure 9.7 and caption**

Unrealistic scenario; inconsequential result.

We realize this is repetitive, but the Assessment would do well to eliminate most references to A2, which was generated before the realization that shale gas will become ubiquitous as an electrical generation feedstock and may make strong inroads into vehicular transportation. The glib counter that "current emissions are above A2" is an infrared herring as emissions in A1B are actually supposed to be *higher* than A2 at this point in time.

More importantly, Figure 9.7 ignores a lot of real adaptation to hot weather. The figure above, in the last comment, shows that heat-related mortality is virtually nonexistent in Tampa and Phoenix, the two cities with the oldest population distributions and some of the highest heat indices in the nation.

Note Figure 9.7 shows an average (1971-2000) of 100-plus days over the Southwest. Splitting the difference between the unrealistic A2 and B1 yields an average of about 13 nationwide for 2041-70 (about as far as one can realistically project). Given the adaptation in the currently hot cities with relatively elderly population distributions, it is obvious that we will adapt to what is being forecast to occur—which, of course, is why heat-related mortality shows no rise in CDC data (but actually declines; see previous comment for references).

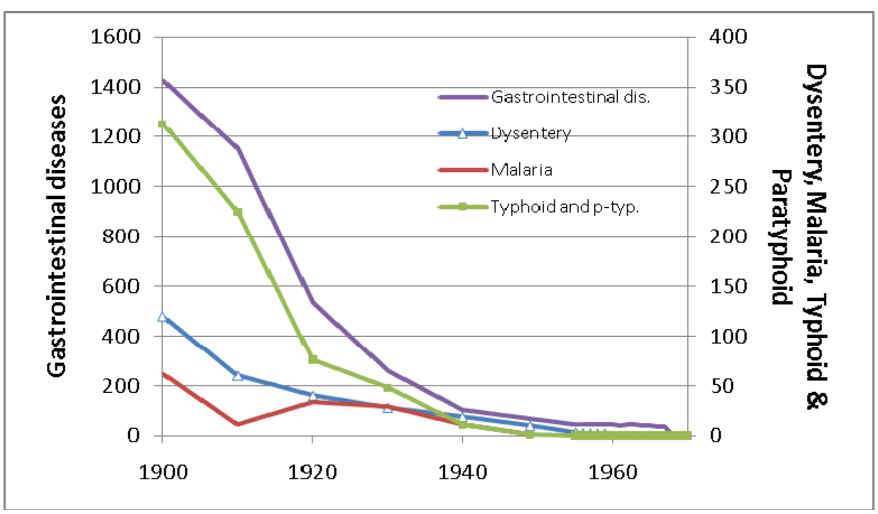
**Page 345, lines 9-24**

Food and Waterborne diseases have declined dramatically in frequency, despite warming temperatures. Text is very misleading with regard to the United States and in complete disregard for observed epidemiological data.

Climatic warming the U.S. is not likely to significantly affect food, water, and insect-born disease, as these were major killers in the early 20th century, which is the coldest period in the U.S. instrumental record. They were eradicated not by changing climate but by improved sanitation and prevention. Given the huge natural range of U.S. climate, the hypothesis that a few degrees of warming would suddenly bring back massive disease is risible (as is the notion that West Nile virus spread across the US because of climate change).

The cumulative death rate in 1900 from typhoid and paratyphoid, various GI diseases (gastritis, duodenitis, enteritis and colitis) and all forms of dysentery was 1,922 per million (Goklany, 2009). For a population the size of the U.S.'s today, that translates into over 600,000 deaths annually. Currently, however, deaths from all food, water and insect borne diseases are approximately 3,000 annually (Gillis 2011; Hall-Baker 2011). To put these numbers in context, the U.S. has 2,400,000 deaths annually. U.S. death rates from various water-related diseases—dysentery, typhoid, paratyphoid, other gastrointestinal diseases, and malaria—declined by 99.6–100.0% from 1900 to 1970.

A severe 1994 outbreak of cryptosporidium is thought to have been responsible for at least 54 deaths in Milwaukee, but this was due to abnormally high concentrations that remained in the water after treatment. As cryptosporidium is present in 17% of sampled U.S. drinking water supplies (Rose et al., 1991), and the lack of any evidence for large scale endemicity indicates outbreaks are more a result of treatment error rather than climatic change.

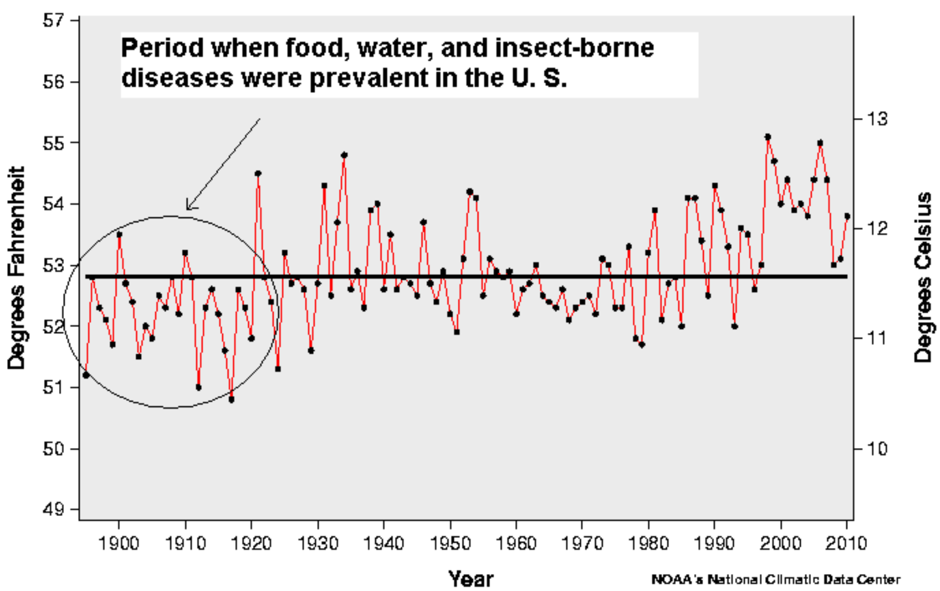



*CAPTION: Death rates (deaths per million) for various water-related diseases, U.S., 1900–70. By 1950, these had become (and remain) inconsequential from a public-health standpoint.*

**Malaria in the United States, 1882**

*CAPTION: Malaria was a national scourge in the United States in the late 19th century, when the average surface temperature of the nation was about 1.5° lower than present (Reiter, 2001).*

In the late 19th century, when the coterminous United States was about 1.5° cooler than the present, malaria was endemic to the Canadian border. Sanitation, not climate change, is the major determinant of the disease.




*CAPTION: U.S. annual average temperature (data source: NCDC).*

Likewise, food, water, and insect-borne diseases were at their peak during the coldest part of the 20th century. Does anyone seriously think that the massive decline in incidence that occurred as surface average temperature warmed 1.5°F will suddenly reverse as it continues to warm?

References:

Gillis, D., 2011. Vital Signs: Incidence and Trends of Infection with Pathogens Transmitted Commonly Through Food — Foodborne Diseases Active Surveillance Network, 10 U.S. Sites, 1996–2010, *MMWR* 60 (22), 749-55.

Goklany IM. 2009. Have increases in population, affluence and technology worsened human and environmental well-being? *Electronic Journal of Sustainable Development,* 1(3).

Hall-Baker, P.A., 2011. Summary of Notifiable Diseases — United States, 2009. *MMWR* 58 (53), 1-100.

Reiter, P., 2001. Climate change and mosquito-borne disease. *Environmental Health Perspectives*, 109, 141-161.

Rose, J.B., C.P. Gerba, and W. Jakubowski, 1991. Survey of Potable Water-Supplies for 21 Cryptosporidium and Giardia. *Environment Science & Technology*, 25, 1393-1400.

**Page 346, Figure 9.9 and caption.**

Comment above applies. Frequency of waterborne disease is a function of sanitation and public health, and historically has varied *inversely* with temperature; i.e. temperature is irrelevant to this in our technologically advanced society. Why would that change?

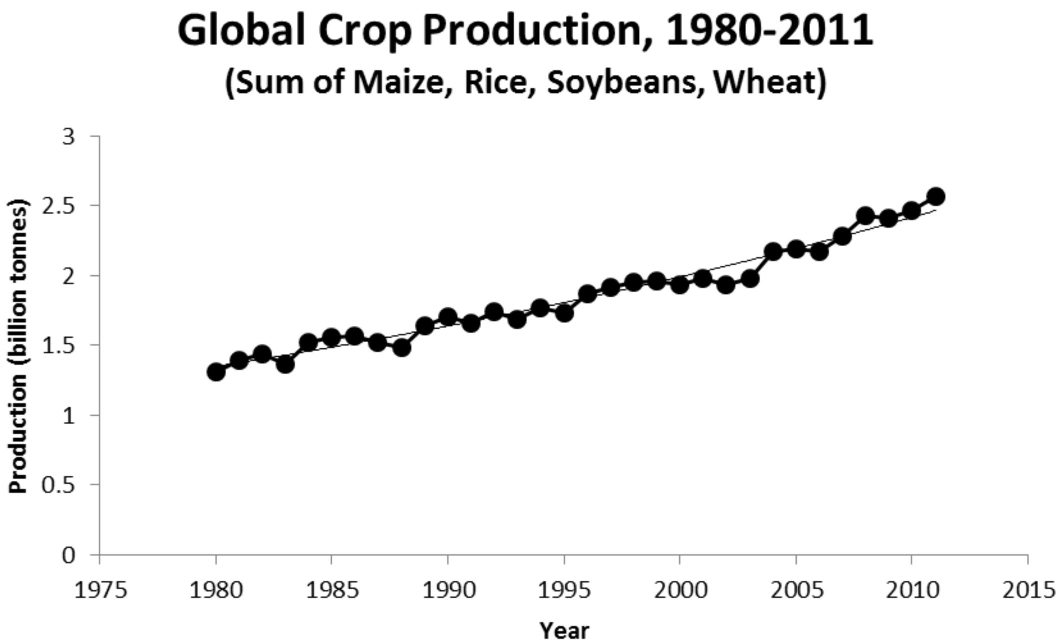
**Page 347, Figure 9.10 and caption**

Ditto above. This is just nonsense given the actual data on waterborne diseases.

**Page 348, lines 11-21**

Food-security is a non-issue.

Food supply is a function of production minus consumption. Production is the product of yield multiplied by acreage. So, while it is obvious that changes in temperature and precipitation have detectable effects on yield, what really matters is how much is produced, which is reproduced below:




*CAPTION: Global annual total production from maize, rice, soybeans, and wheat. Data source***:** *Food and Agriculture Organization, United Nations, available at http://faostat.fao.org/site/567/DesktopDefault.aspx?PageID=567*

An exponential fit (Y = 2283.3 - 2.32 (year) + .00059(year)$^2$) is significantly better than a linear one, which demonstrates how insignificant the climatic component of global food production is.

What's missing here? There's simply little, if any, effect of year-to-year global climate variability. That's because the world food system is highly diverse, in terms of varieties grown and the climates in which they grow.

Further, there is a tremendous amount of reserve built into food supply because of the (stupid) diversion towards biofuels. In fact, the amount we divert to ethanol dwarfs the amount that is lost to climate. Lobell (2011) reported that, after allowing for the growth enhancement from atmospheric carbon dioxide, global average crop yields were reduced by a bit less than 1% (which is small compared to the amount that they increased because of technological advances) during the period 1980-2008.

But consider this. The U.S. produces about 36 percent of the world's corn. And about 40 percent of U.S. corn is used to produce ethanol for use as a gasoline substitute instead of being consumed by humans or animals. Globally, corn makes up 30 percent of total worldwide production of the four crops studied by Lobell's group.

And even this less than 1 percent impact was described by Lobell et al. as perhaps being "overly pessimistic" because it did not fully incorporate long-term adaptive farming responses to changing climate conditions (i.e., farmers are not as dumb as statistical models make them out to be).

What this means is that even under overly pessimistic scenarios, we still currently burn more than 4 times as much grain as climate change has taken away. Thinking about this in future terms, if we observe *twice* as much climate change from 2010 through 2038 as we did from 1980 to 2008 (Lobell's study period), all we would have to do is stop burning *half* as much ethanol as we do now to make up for the *entire* global climate-related crop reduction.

Therefore, climate is an irrelevant overlay on world food supply for the foreseeable future. If we really need the food, just stop the stupid conversion to ethanol.

Reference:

Lobell, D.B., W. Schlenker, and J. Costa-Roberts, 2011. Climate trends and global crop production since 1980. *Science*, 333**,** 616-620.

**Page 350, Figure 9.12 and caption**

With regard to age-related mortality, Davis et al. (2003) found that from 1964 to 1998, heat-related deaths declined significantly for 19 of 28 U.S. metropolitan areas, as well as for the 28-city average. Kalkstein et al. (2011) found a reduction in mortality attributable to excessive heat events from 1996 to 2004 for 40 major U.S. metropolitan areas.

The Davis et al. study also shows that base heat wave mortality is much lower in urban areas where they are more frequent. The two cities with the lowest mortality, Tampa and Phoenix, have some of the oldest age-distributions in the world. Thus the statement that "the elderly are more vulnerable to heat stress," while physiologically correct, is profoundly misleading; it is quite clear that in affluent societies that adaptation to heat more than compensates for the relative inability of the elderly to tolerate very high temperatures.

Chronic respiratory disease deaths: by far the largest cause of this is smoking, and the decline in smoking rates is the main reason that respiratory disease death rates are now in decline. This volitional behavior is far more important than any climate change. Ditto for U.S. obesity and related diabetes rates. The dramatic increases have nothing to do with climate change, and dwarf climate's effects by orders of magnitude. If tens of millions of people are stupid enough to put themselves in harm's way like this, maybe we're kind of wasting our time harping on climate change, given the fact that we can't do much about it anyway?

References:

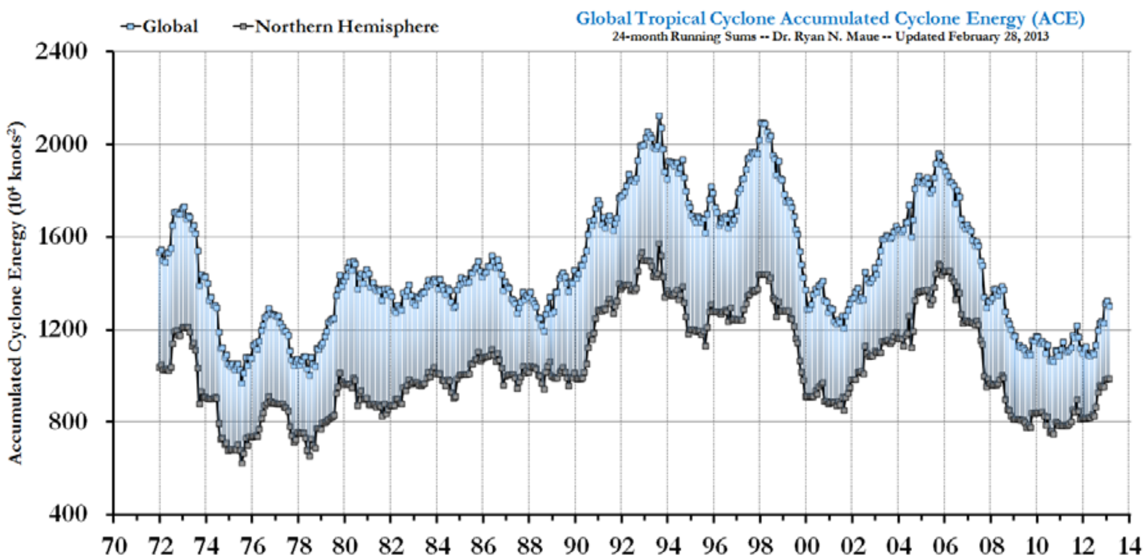
Davis, R.E., Knappenberger, P.C., Michaels, P.J., and Novicoff, W.M., 2003. Changing Heat-Related Mortality in the United States. *Environmental Health Perspectives*, 111, 1712–18.

Kalkstein, L.S., Greene, S., Mills, D.M., and Samenow, J., 2011. An evaluation of the progress in reducing heat-related human mortality in major U.S. cities. Natural Hazards 56 (1): 113-129.

**Page 352, Figure 9.13 and caption**

Nice job conflating Katrina, hurricanes and increasing "extreme weather events." Nice and misleading.

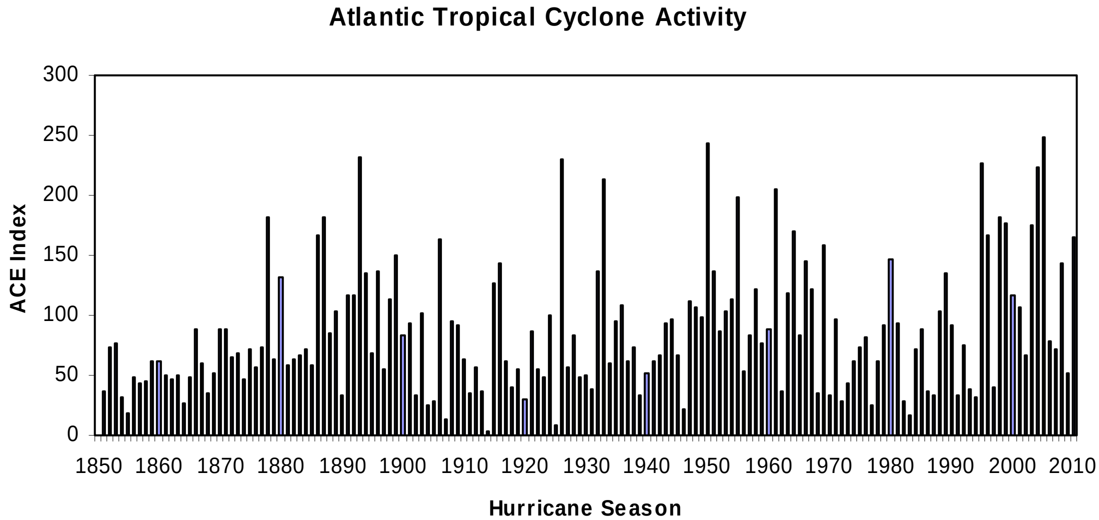
Here's what's really happening:




*CAPTION: Last 4-decades of Global and Northern Hemisphere Accumulated Cyclone Energy: 24 month running sums. Note that the year indicated represents the value of ACE through the previous 24-months for the Northern Hemisphere (bottom line/gray boxes) and the entire global (top line/blue boxes). The area in between represents the Southern Hemisphere total ACE. (data source: updates from Maue et al., 2011)*

The relationship between the global mean temperature anomaly and tropical cyclone energy in the Northern Hemisphere? Not very clear, to say the least! Further, tropical cyclone activity in the Atlantic Basin shows very little secular change.

The history of the ACE index shows the relatively high levels of activity in the 1880s-1890s, 1950s-1960s, and the period since 1995. Periods of low levels of Atlantic tropical cyclone activity include the 1850s, 1910s-1920s, and 1970s-1980s. There is simply no relationship between ACE and global mean temperature.




*CAPTION: Accumulated Cyclone Energy (ACE) index for the Atlantic Basin from 1851 through 2010. There is obviously no relationship to the temperature rise shown in the last chapter. (data source: http://www.aoml.noaa.gov/hrd/tcfaq/E11.html).*

A variety of factors act together to influence tropical cyclone development, growth, track, and whether or not a storm makes landfall along the U.S. coast. These include large-scale steering winds, atmospheric stability, wind shear, sea surface temperature and ocean heat content. Tropical storms and hurricanes develop and gain strength over warm ocean waters. However, it does not strictly follow that warmer waters lead to stronger hurricanes. New evidence has emerged that shows that the contrast in sea surface temperature between the main hurricane development region in the Atlantic and the broader tropical ocean plays an important role in hurricane development (Vecchi et al., 2008; Swanson 2008; Knutson et al., 2008). Additionally, other factors such as atmospheric stability and circulation can also influence hurricane frequency and intensity (Bell et al., 2011). For these and other reasons, a confident assessment of the causes of tropical cyclone variability in the Atlantic basin requires further study.

Projections are that sea surface temperatures in the main Atlantic hurricane development region will increase during this century under higher emissions scenarios. Other environmental factors are projected to change as well, complicating assessment of future tropical cyclone behavior. This highlights the need to better understand the relationship between hurricane frequency, intensity, climate, and climate change (Knutson et al., 2010).

The consensus is still evolving as to how anthropogenic climate change will alter the characteristics of Atlantic basin tropical cyclones. There is growing evidence that the frequency of Atlantic basin tropical cyclones will be little changed, but that some storms may become more intense, although the preferred tracks of storms may be altered in such

a way as to reduce the threat of a U.S. landfall (e.g., Wang et al., 2011). However, such changes are not anticipated to emerge above the level of natural noise until very late in this century (Michaels et al., 2005).

One important fact about the impact of tropical cyclones that is virtually certain is that further development of our coastlines, including growing population, increasing wealth, and expanding infrastructure, will increase the vulnerability to direct hurricane strikes regardless of any influence that a changing climate may impart (e.g., Pielke Jr., 2007).

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**Page 353, lines 20-22**

Chikengunya has “Devastated populations in other countries”? Here’s a quote from Dwibedi et al, cited on line 22.

“Morbidity was high though no deaths were recorded.”

Is that “devastation”?

Another reference, Rezza et al., 2007 is supposed to be evidence of climate-related spread of this disease. Read for yourself from Rezza—the Italian outbreak was caused by the jet plane, not climate change (also was not very “devastating”):

> Analysis of samples from human beings and from mosquitoes showed that the outbreak was caused by CHIKV. We identified 205 cases of infection with CHIKV between July 4 and Sept 27, 2007. The presumed index case was a man from India who developed symptoms while visiting relatives in one of the villages. Phylogenetic analysis showed a high similarity between the strains found in Italy and those identified during an earlier outbreak on islands in the Indian Ocean. The disease was fairly mild in nearly all cases, with only one reported death.

This little incident is an example of very bad work. At the very least, it appears to be an attempt to deceive the readers, few of whom would actually check the primary references.

The question is *why was this done*?

**Page 354, lines 36-41; Page 355, lines 1-35**

An amazing section pushing an integrated, intrusive agenda. Did Mayor Bloomberg write this?

Specifically, it advocates for “eliminating short vehicle trips” (Page 354, line 38), improvement of “fitness and health through increased physical activity” ( Page 355, line 2), *Volleyball will begin at 7am*, “Innovative urban design” with “increased access to active transport” (Line 4), *All must ride the bus, bike, or walk from their crowded Stalinesque dense housing,* “improved building construction, provision of services, and infrastructure creation (lines 6-7)”, *more expensive housing and more government spending from an economy 17 trillion dollars in hock*, “promoting social interaction”, *required Kumbaya begins around the campfire at 9pm*, a reduction in red meat consumption (lines 12-13), *darn that consumer choice! We’ll do something about that!!*, “a reduction [in methane] achieved through an overall decrease in the consumption, and therefore the production, of red meat (lines 15-16), *I’m sure the required flatulence tax will go over well with the farm lobby*, that a reduction in red meat consumption will reduce cardiovascular disease and cancer incidence, *not statistically significant in the largest study ever performed* (Rohrmann et al., 2013)*, but who cares, we’ve got an*

*agenda here!,* climate change mitigation and adaptation policy could also reduce health-related disparities between wealthy and poor communities, *by making energy more expensive and forcing people onto public transportation? Well, yes, everyone suffers*. “Hurricane Katrina demonstrated that communities of color, poor communities…are more vulnerable to the adverse effects of extreme weather events”. *Yes it is a fact that houses build on or near floodplains are much cheaper; perhaps the authors would prefer none at all?* “urban planning policies that ensure new building, including homes, are constructed to resist extreme weather events. *Of course the expense is passed on to those who will not live in them*, “improve the food security of low-income residence by preventing decreased crop production due to climate change*, when three times as much effective production loss is a result of ethanol diversion, when compared to climate change (see earlier comment).*

This last section is the quintessence of why so many people think that climate “science” is merely a stalking horse for a larger interventionist agenda.

Reference:

Rohrmann, S., et al. 2013. Meat consumption and mortality. Results from the European Prospective Investigation into Cancer and Nutrition. *BMC Medicine*, 11, doi:10.1186/1741-7015-11-63.