United States Environmental Protection Agency

Endangerment and Cause or Contribute Findings for Greenhouse Gases Under Section 202(a) of the Clean Air Act:

EPA's Response to Public Comments

Volume 4: Validity of Future Projections

Validity of Future Projections

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FOREWORD

This document provides responses to public comments on the U.S. Environmental Protection Agency's (EPA's) Proposed Endangerment and Cause or Contribute Findings for Greenhouse Gases Under Section 202(a) of the Clean Air Act, published at 74 FR 18886 (April 24, 2009). EPA received comments on these Proposed Findings via mail, e-mail, and facsimile, and at two public hearings held in Arlington, Virginia, and Seattle, Washington, in May 2009. Copies of all comment letters submitted and transcripts of the public hearings are available at the EPA Docket Center Public Reading Room, or electronically through http://www.regulations.gov by searching Docket ID *EPA-HQ-OAR-2009-0171*.

This document accompanies the Administrator's final Endangerment and Cause or Contribute Findings for Greenhouse Gases Under Section 202(a) of the Clean Air Act (Findings) and the Technical Support Document (TSD), which contains the underlying science and greenhouse gas emissions data.

EPA prepared this document in multiple volumes, with each volume focusing on a different broad category of comments on the Proposed Findings. This volume of the document provides responses to public comments regarding the validity of future projections.

In light of the very large number of comments received and the significant overlap between many comments, this document does not respond to each comment individually. Rather, EPA summarized and provided a single response to each significant argument, assertion, and question contained within the totality of comments. Within each comment summary, EPA provides in parentheses one or more lists of Docket ID numbers for commenters who raised particular issues; however, these lists are not meant to be exhaustive and EPA does not individually identify each and every commenter who made a certain point in all instances, particularly in cases where multiple commenters expressed essentially identical arguments.

Several commenters provided additional scientific literature to support their arguments. EPA's general approach for taking such literature into consideration is described in Volume 1, Section 1.1, of this Response to Comments document. As with the comments, there was overlap in the literature received. EPA identified the relevant literature related to the significant comments, and responded to the significant issues raised in the literature. EPA does not individually identify each and every piece of literature (submitted or incorporated by reference) that made a certain point in all instances.

Throughout this document, we provide a list of references at the end of each volume for additional literature cited by EPA in our responses; however, we do not repeat the full citations of literature cited in the TSD.

EPA's responses to comments are generally provided immediately following each comment summary. In some cases, EPA has discussed responses to specific comments or groups of similar comments in the Findings. In such cases, EPA references the Findings rather than repeating those responses in this document.

Comments were assigned to specific volumes of this Response to Comments document based on an assessment of the principal subject of the comment; however, some comments inevitably overlap multiple subject areas. For this reason, EPA encourages the public to read the other volumes of this document relevant to their interests.

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Acronyms and Abbreviations

ACIA	Arctic Climate Impact Assessment
AGAGE	Advanced Global Atmospheric Gases Experiment
AMO	Atlantic Multidecadal Oscillation
AOGCM	Atmosphere-Ocean General Circulation Models
AR4	IPCC Fourth Assessment Report
CCSP	U.S. Climate Change Science Program
CERES	Clouds and the Earth's Radiant Energy System (CERES)
C	degrees Celsius
CH_4	methane
CO_2	carbon dioxide
ECS	equilibrium climate sensitivity
ENSO	El Niño-Southern Oscillation
EPA	U.S. Environmental Protection Agency
ERBS	Earth Radiation Budget Satellite
GHG	greenhouse gases
GCM	general circulation model
GDP	gross domestic product
Gt	gigatonnes
GtC	gigatonnes of carbon
$GtCO_2$	gigatonnes of carbon dioxide
hPa	hectopascal
IPCC	Intergovernmental Panel on Climate Change
IQA	Information Quality Act
ISCCP	International Satellite Cloud Climatology Project
K	degrees Kelvin
km	kilometers
LGM	Last Glacial Maximum
MER	market exchange rate
milliW/m^2	milliwatts per square meter
MIT	Massachusetts Institute of Technology
MJO	Madden-Julian Oscillation
mm	millimeters

Acronyms and Abbreviations (Continued)

MSU	microwave sounding unit
NAM	Northern Annular Model
NASA	National Aeronautics and Space Administration
NCDC	National Climatic Data Center
NOAA	National Oceanic and Atmospheric Administration
NRC	National Research Council
OH	water-oxygen
PDF	probability density function
PDO	Pacific Decadal Oscillation
ppm	parts per million
PPP	purchasing power parity
RF	radiative forcing
RSS	Remote Sensing System
SAGE	Stratospheric Aerosol and Gas Experiment
SRES	IPCC Special Report on Emissions Scenarios
SW	short wave
TAR	IPCC Third Assessment Report
TOA	top of the atmosphere
TSD	Technical Support Document
UAH	University of Alabama in Huntsville
UN	United Nations
USGCRP	U.S. Global Change Research Program
W	watts
WAIS	West Antarctic Ice Sheet
W/m^2	watts per square meter
WD	water vapor dimers
WV	water vapor
XBT	expendable bathythermograph

4.0 Validity of Future Projections

Comment:

A number of commenters (e.g., 2898.1, 3394.1, 3411.1, 3596.2, 3722) state that specific aspects of the evidence about future projections summarized in the TSD do not support the Administrator's endangerment finding.

Response:

The specific issues that underlie these comments are addressed in the responses throughout this volume, and other volumes of the Response to Comments document. With regard to the commenters' conclusion that the current science does not support an endangerment finding with respect to future projections, we disagree based on the scientific evidence before the Administrator. See the Findings, Section IV.B, "The Air Pollution is Reasonably Anticipated to Endanger Both Public Health and Welfare," for details on how the Administrator weighed the scientific evidence underlying her endangerment determination in general, and with regard to future projections in particular.

4.1 Climate Models

Comment (4-1):

A number of commenters (e.g., 0339, 2759, 2818, 3214.1, 3217, 3360, 3446.1) argue that the hypothesis of danger from human-induced climate change is founded on the use of computer models and that flaws in these models weaken or invalidate the conclusions of the Intergovernmental Panel on Climate Change (IPCC) and the Technical Support Document (TSD), many citing the poor representation of clouds in the models. One commenter states "Their unproven theory that CO_2 is the main cause of global warming is only supported by computer models." A commenter (0664) claims that the sheer complexity of the system makes modeling impossible at this time. Many of the following comments in this section of the Response to Comments document make similar arguments.

Response (4-1):

We respond here to a theme that runs through many of the comments in this section. Many commenters arguments appear to assume that the findings of the assessment literature rest solely or predominantly on the outputs of climate models and that these outputs are flawed and unreliable. Based on our review of the assessment literature, EPA finds that both of these assumptions are incorrect.

First, models are not the foundation of climate science, rather they are the tools used to better understand information and data from multiple sources and disciplines. Paleoclimate data, basic theory, observations of climate changes, and other branches of climate science together have provided (and continue to provide) the basis for key findings in the assessment literature. Indeed, research long before the advent of the computer found that the climate should respond to increased CO_2 concentrations. Recently, scientists have used paleoclimate data about historical analogues such as the last interglacial and glacial maximum to estimate climate sensitivity, sea level response to temperature change, and other important climatic variables (Jansen et al., 2007, Hegerl et al., 2007). Computer modeling is, of course, important because it improves refinement of predictions, attributions, and analysis of non-linear interactions of a complex system, and thus climate models will continue to play a major role in understanding and projecting the future of the climate system. However, the characterization of a number of commenters that the projection and attribution findings of the IPCC, the U.S. Global Change Research Program (USGCRP), and others are supported only by the output of models is not accurate.

With respect to the issues commenters raise concerning flaws in the models used for projections and attribution studies, it is well recognized that models are representations of complex systems and may not be able to perfectly represent all interactions in the system being modeled. For example, clouds are difficult to model computationally because the physics involved in cloud formation occurs at scales smaller than the resolution of most climate models. Although model-based results are subject to some degree of inherent uncertainty, as reflected in the assessment literature and in the TSD, these uncertainties are acknowledged; uncertainties do not mean that the models are fatally flawed or unreliable representations of the climate system. Climate models have been demonstrated to successfully simulate a number of climatic properties, as documented in the IPCC, U.S. Climate Change Science Program (CCSP), National Research Council (NRC), and USGCRP reports on which the TSD primarily relies.

Absolute certainty is not required, and in fact, the TSD summarizes both the important role and the limitations of models in Section 6(b), quoting Meehl et al. (2007):

> [C]onfidence in models comes from their physical basis, and their skill in representing observed climate and past climate changes. Models have proven to be extremely important tools for simulating and understanding climate, and there is considerable confidence that they are able to provide credible quantitative estimates of future climate change, particularly at larger scales. Models continue to have significant limitations, such as in their representation of clouds, which lead to uncertainties in the magnitude and timing, as well as regional details, of predicted climate change. Nevertheless, over several decades of model development, they have consistently provided a robust and unambiguous picture of significant climate warming in response to increasing greenhouse gases.

Karl et al. (2009) reaches a similar conclusion, stating:

> All of the models used in this work [Karl et al., 2009] have imperfections in their representation of the complexities of the "real world" climate system. These are due to both limits in our understanding of the climate system, and in our ability to represent its complex behavior with available computer resources. Despite this, models are extremely useful, for a number of reasons.
>
> First, despite remaining imperfections, the current generation of climate models accurately portrays many important aspects of today's weather patterns and climate. Models are constantly being improved, and are routinely tested against many observations of Earth's climate system. Second, the fingerprint work shows that models capture not only our present-day climate, but also key features of the observed climate changes over the past century. Third, many of the large-scale observed climate changes (such as the warming of the surface and troposphere, and the increase in the amount of moisture in the atmosphere) are driven by very basic physics, which is well-represented in models. Fourth, climate models can be used to predict changes in climate that can be verified in the real world. Examples include the short-term global cooling subsequent to the eruption of Mount Pinatubo and the stratospheric cooling with increasing carbon dioxide. Finally, models are the only tools that exist for trying to understand the climate changes likely to be experienced over the course of this century. No period in Earth's geological history provides an exact analogue for the climate conditions that will unfold in the coming decades.

A CCSP report (2008c) assessed model strengths and limitations in detail, and the introduction states:

> Scientists extensively use mathematical models of Earth's climate, executed on the most powerful computers available, to examine hypotheses about past and present-day climates. Development of climate models is fully consistent with approaches being taken

> in many other fields of science dealing with very complex systems. These climate simulations provide a framework within which enhanced understanding of climate-relevant processes, along with improved observations, are merged into coherent projections of future climate change…
>
> The science of climate modeling has matured through finer spatial resolution, the inclusion of a greater number of physical processes, and comparison to a rapidly expanding array of observations. These models have important strengths and limitations. They successfully simulate a growing set of processes and phenomena; this set intersects with, but does not fully cover, the set of processes and phenomena of central importance for attribution of past climate changes and the projection of future changes.

The consensus of the assessment literature is that models serve a useful purpose within the field of climate science, successfully modeling a number of processes. This is true despite the complexity of the system that was mentioned by one commenter. Although there are a number of limitations and uncertainties, they are accounted for, on a global scale, by providing a range of global mean temperature estimations for any given emission scenario (e.g., Figure 10.26, Meehl et al., 2007). This range, though important, still enables robust conclusions about the impact of greenhouse gases (GHGs) on temperature.

A number of specific comments on model flaws are addressed by responses within this volume. It is EPA's conclusion that the models have demonstrated the ability to accurately simulate many key aspects of climate, and in light of the key conclusions of the IPCC, CCSP, and USGCRP in regard to model skill and limitations, we have determined that it is fully appropriate to report model-based projections and attribution studies in the TSD.

Comment (4-2):

Some commenters (0664,1309.1) argue that inability to model more than a week of weather means we cannot model 100 years of climate. Two commenters (2818, 3446.1) claim that 77 percent of the data essential for model initialization do not exist. One commenter (0639.1) cited "a NASA scientist" who claims that United Nations (UN) calculations did not include the appropriate boundary conditions.

Response (4-2):

The claim that our inability to forecast the weather means that we cannot possibly understand the climate is a common one, and it is unfounded. It is based on the belief that "weather" and "climate" are essentially the same thing, with the same modeling limitations. This issue is discussed in the assessment literature, and the IPCC Fourth Assessment Report (Le Treut et al., 2007) provides a clear explanation of the difference between climate and weather:

> Climate is generally defined as average weather, and as such, climate change and weather are intertwined. Observations can show that there have been changes in weather, and it is the statistics of changes in weather over time that identify climate change. While weather and climate are closely related, there are important differences. A common confusion between weather and climate arises when scientists are asked how they can predict climate 50 years from now when they cannot predict the weather a few weeks from now. The chaotic nature of weather makes it unpredictable beyond a few days. Projecting changes in climate (i.e., long-term average weather) due to changes in atmospheric composition or other factors is a very different and much more manageable issue.

Thus, we disagree with the assertion by commenters that a limitation in the ability to predict weather implies a limitation in the ability to predict climate on the 100-year timescale.

The claims that insufficient model initialization data exists, and that there are no appropriate boundary conditions, are related to the issue of the differences between weather and climate. A climate model usually divides the globe up into a large number of grid cells. Each cell has at any given point in time a temperature, wind direction, precipitation, gas composition, and other characteristics. The model then uses physics routines to determine how the characteristics of these grid cells would change for the next time step in the model (often several minutes of "model time"). The behavior of these grid cells defines the weather in the model—heat waves, storms, etc. Therefore, grid-by-grid data would be very important for near-term weather predictions. Indeed, the limitations of the initialization data (initial boundary conditions) and the resolution of the model are key factors in determining how far into the future a weather model can make accurate forecasts. On the other hand, 100-year climate projections are relatively insensitive to small changes in initial conditions. For purposes of predicting that June will be warmer than December, one does not necessarily need to know the exact atmospheric conditions of every square inch on the planet. For purposes of predicting the climate response (the average weather) to CO_2 concentrations, it is also not necessary to perfectly initialize the model. Indeed, most climate models are "spun-up" from time periods long before the present day, rather than being initialized to match current conditions; this "spin-up" is done to make sure that the model is in a state that is stable and self-consistent, which is more important than whether the model weather on the 5th of June matches the real-world weather on the 5th of June. Therefore, we find that these objections do not weaken the conclusions of the assessment literature as summarized in the TSD.

Comment (4-3):
Many commenters argue that models do not capture a number of important processes. For example, commenters list processes such as clouds, dust, chemistry, and biological systems (0195,0546,3722), "Asian brown cloud" (3330.1), black carbon (3316.1,3596.1), historical albedo changes (0639.1), soil moisture (3596.1), various non-GHG anthropogenic forcings such as land use change and heat islands (3222.1), solar activity (2895), cosmic radiation (0650), and increased evaporation (1606.1) (which the commenter notes has not been seen because of global dimming). One commenter cites Wild (2005b) and Stanhill and Cohen (2009) when stating that "[c]urrent models do not consider the observed solar 'dimming' and post-1985 'brightening.'" Commenter (3722) states that EPA failed to consider soils and vegetation in the carbon cycle, citing Dyson (1999) as stating that it makes no sense to consider the atmosphere and ocean alone.

One commenter (3316.1) quotes the Scientific Alliance as stating that "it is sobering to note that the last IPCC Assessment Report, published just two years ago, makes no mention of the significant effect of soot."

Response (4-3):
We have reviewed the assessment literature in light of these comments and the referenced materials, and we conclude that commenters are incorrect. In fact, the models do include the most essential climate-related processes. Many or most models include the effects of black carbon, global dimming, and other aerosol issues (including the atmospheric brown cloud), atmospheric chemistry, biological ecosystem uptake, natural variability on various timescales, land use change and historical albedo changes, and soil moisture and evaporation. These are all addressed in detail by the IPCC (Randall et al., 2007) and CCSP (2008c), which detail many of the newer advances in modeling terrestrial systems, aerosol indirect effects, and other properties. Figure 1.2 of Le Treut et al. (2007) and Figure 1.1 of CCSP (2008c) show graphical depictions of the evolution of climate models, showing that modern models now include chemistry, sulfates, precipitation, volcanic activity, land surface albedo, non-sulfate aerosols, rivers, interactive vegetation, sea ice, the carbon cycle, and overturning ocean circulation. Randall et al. note that carbon cycle dynamics now include soil carbon cycle, and devote a section to soil moisture feedbacks.

Global dimming refers to the reduction of sunlight reaching the surface, in large part due to increased aerosol emissions during the mid to late 20th century. This dimming has consequences for vertical temperature distributions, evaporation, ecosystem growth, and other climatic variables. Dimming in particular was addressed in terms of observational studies by the IPCC in Trenberth et al. (2007). Both submitted references (Wild, 2005b; Stanhill and Cohen, 2009) are also observational studies and are consistent with the IPCC treatment of these observations in that both studies show that dimming has reduced after 1990 (the Wild study was included in Trenberth et al.), though there is disagreement between the two studies in the urban bias of the dimming, with Stanhill and Cohen showing that in Israel the evidence is consistent with a more broad-based dimming. Any model that includes aerosols (which is nearly all of them) also includes the basic physics that lead to "dimming" and "brightening." There are, however, continued uncertainties about historical and projected aerosol emissions, and those uncertainties are summarized in Section 6 of the TSD.

Clouds are difficult to model computationally because the physics involved in cloud formation occurs at scales smaller than the resolution of most climate models. Therefore, clouds are represented by parameterizations in global climate models rather than being calculated explicitly. Because cloud responses to climatic change are important for both the trapping and reflection of energy, we recognize that clouds contribute to uncertainties in model-based results. For this reason, the TSD summarizes the uncertainties involved in estimating the indirect albedo effect (see Section 4[a]), some regional changes in precipitation (see Section 6[b]), and issues involving cloud representation (see Section 6[b]).

With regards to the IPCC Assessment Report not mentioning soot, we note that soot and black carbon are both addressed at length in Forster et al. (2007). See also the response on black carbon in Volume 3 on commenters attributing recent temperature change to black carbon instead of GHGs.

Historical patterns of solar activity are also included in the models. For EPA's responses to issues involving the influence of cosmic radiation on climate, please refer to Volume 3 of this Response to Comments document.

Comment (4-4):
One commenter (3769.1) objects to the lack of inclusion in climate models of water vapor dimers, citing Paynter et al. (2007). The commenter states that "their atmospheric absorption of incoming solar radiation in the near infrared … may lead to a negative climate feedback as WV [water vapor] concentration increases in the lower troposphere."

Response (4-4):
In response to this comment, we reviewed the literature on water vapor dimers, and we conclude that the explicit inclusion in models of water vapor dimers is not likely to materially change any conclusions from the assessment literature regarding climate feedbacks. Water vapor dimers (WD) are phenomena that occur at high concentrations of water vapor when pairs of water molecules interact in ways that can change their light absorption properties. Therefore, the absorption properties of water in the atmosphere may be dependent on this effect. Some studies suggest that inclusion of this behavior can lead to an increase in atmospheric absorption of sunlight by a couple of percent (Chýlek and Geldart, 1997) compared to a modeled situation without water dimers.

We do not find in the literature (including the referenced paper by Paynter et al., which was a laboratory measurement of the dimer absorption spectra and included no statements about implications for climate feedbacks) any support for the commenter's assertion that WD are likely to lead to negative climate feedbacks, much less sufficient negative feedback to materially change projections of climate change.

Indeed, while the posited increased absorption by water vapor dimers at higher temperatures might reduce surface insolation at the surface by absorbing more sunlight before it reaches the ground, the increased absorption should also increase the warming of the troposphere. Thus, it appears that it is more likely that there would be a net positive feedback rather than a net negative feedback (though it would be a small contribution either way). This interpretation is supported by Pfeilsticker et al. (2003), which finds that atmospheric short wave absorption by water dimers should exert a positive climate feedback and additionally reduce atmospheric convection by heating the atmosphere and reducing surface evaporation. Pfeilsticker also states that "[a]ccordingly, WDs should be regarded as a greenhouse gas, of which the radiative effects are, however, already considered in most atmospheric radiative transfer models."

We conclude that this is an active area of research, and there are a number of questions that have yet to be resolved. However, we find no evidence to support the implication by the commenter that the lack of explicit inclusion of water vapor dimer physics in models weakens the conclusions drawn from models. As Pfeilsticker noted, while water dimers are not explicitly resolved, their radiative effects are already reflected in most radiative transfer models, and if anything their inclusion is more likely to lead to increased warming rather than cooling.

Comment (4-5):

Some commenters argue that climate models do not properly include some modes of natural variability. For example, some commenters (e.g., 3291.1) mention the Madden-Julian Oscillation (MJO), including a citation of the IPCC statement that deficiencies remain in the simulations of the MJO. One commenter (3316.1) cites a paper by Spencer that uses a simple model to show that if the Pacific Decadal Oscillation (PDO) modulates cloudiness, it can explain up to two-thirds of the recent warming. One commenter (0454) states that a 60-year cycle of warming and cooling can explain temperature changes rather than CO_2.

Response (4-5):

As reviewed in response number 4-1, models accurately portray many important aspects of the climate. The IPCC does discuss limitations of the models with respect to the portrayal of some modes of natural variability, but states that these limitations are already reflected in the range of global temperature change projected from a given change in greenhouse gas forcing (Randall et al., 2007). Randall et al. conclude that:

> Despite such uncertainties, however, models are unanimous in their prediction of substantial climate warming under greenhouse gas increases, and this warming is of a magnitude consistent with independent estimates derived from other sources, such as from observed climate changes and past climate reconstructions.

The Madden-Julian Oscillation is an observed variation in tropical winds and rainfall with a variation of 30 to 90 days (Randall et al., 2007). The IPCC notes that "[s]imulation of the Madden-Julian Oscillation (MJO) remains unsatisfactory." However, as stated above, these limitations are already included within the range of uncertainty represented by the IPCC (and summarized in the TSD). The commenter submitted no documentation supporting any claim that limitations in simulating the MJO undermines major conclusions in the assessment literature.

With respect to the PDO study by Spencer (and the 60-year cycle of warming and cooling, which is likely to be a reference to the PDO), as far as we could determine this study has not passed peer review and currently exists as a blog post. We reviewed the blog post, and determined that it uses a simple energy balance model with variable ocean depths and cloud sensitivity, and an assumption that cloud cover variations were directly proportional to the PDO signal. The study showed that with one combination of parameters, the PDO-cloud link could explain three quarters of the 20th century warming trend (another version of the blog post only attributes two thirds of the 20th century warming to the PDO). When Spencer included GHGs and other forcings compiled by James Hansen, then the fit was closer. The evidence provided by Spencer for a PDO-cloud link was an analysis that showed that there was a correlation between the PDO index and average annual cloudiness over the global oceans measured by the Clouds and the Earth's Radiant Energy System (CERES) instrument over a seven-year period.

There are a number of issues with this study. The first is that the parameters for the simple model are chosen to yield a match with the global temperature and do not appear to be constrained by physical limits. The use of global temperature as the only test of the model is also a problem; most formal attribution studies based on data from the past century often include more data such as hemispheric, oceanic, or atmospheric temperatures in addition to global surface temperatures (Hegerl et al., 2007). As noted in Hegerl et al., the natural modes of variability examined do not produce spatial patterns of change that match historical temperature patterns. Additionally, the specific test used to come up with "three quarters" of warming used only the PDO and initialization conditions to match global temperatures, rather than including all known forcings, which will artificially inflate any correlation between the PDO and the temperature record.

In particular, the method used in the paper to determine a PDO-cloud feedback is limited by only having seven years of data; Loeb et al. (2007) found that 15 years would be required to detect trends at the 90% confidence level using the CERES instrument. It is also unclear why the observational analysis uses only ocean data when the simple model uses global cloud shifts—the short-wave and long-wave ocean-only data in the Spencer blog post appear to have different trends than the global data reported in Peterson et al. (2009).

Therefore, while this is an area of continuing research, we find that the Spencer study is not a compelling alternative to the conclusions of the assessment literature.

Comment (4-6):
Commenters (e.g., 2883.1) argue that the lack of an ocean heat content upward trend since 2003 shows that climate models are incomplete.

Response (4-6):
See our responses to comments on ocean heat content measurements in Volume 2 of this Response to Comments document. This lack of upward trend is both disputed and of a time period that is short for the purpose of trend detection. With respect to modeling, CCSP (2008c) states that ocean heat uptake observations are beginning to be sufficient to test models, and that models provide reasonable simulations of this uptake but have underestimated observed sea level rise. Because sea level rise is in part a function of ocean heat uptake, this is additional data suggesting that the models are not significantly overestimating ocean heat content trends.

Comment (4-7):
Some commenters argue that the lack of inclusion by climate models of heat from volcanoes (especially subsea) and other geothermal sources implies that the models are wrong.

Response (4-7):

We have reviewed the literature in light of this comment, and we disagree with commenters that treatment of geothermal heat sources in models weakens the conclusions of the assessment literature.

Our review of the literature indicates that the total heat geothermal heat flux is small in comparison to the change in forcing from anthropogenic factors. Sclater et al. (1981) estimated total geothermal heat flowing out of the Earth to be about 4.2×10^{13} watts (W)—or roughly 80 milliwatts per square meter (milliW/m^2) averaged over the entire planet. According to Sclater et al., most of this heat loss results from creation of oceanic plate, and the paper found that variations in this flux were small on short (less than 60-million-year) time scales. Given this fairly steady flow, and the fact that 80 milliW/m^2 is roughly 20 times smaller than the current net anthropogenic forcing of about 1.6 W/m^2 at the top of the atmosphere, the contribution of changes in geothermal flux to current warming must be much less than 20 times the contribution of anthropogenic increases in greenhouse gas concentrations.

We also note that geothermal fluxes have in fact been included in some climate models, as demonstrated in the papers by Adcroft et al. (2001) and Scott et al. (2001). They found that steady state geothermal heat from the ocean bottom (which they estimated at 50 milliW/m^2) did have some influence on some aspects of ocean circulation, but did not show that there was any significant contribution to large-scale atmospheric or oceanic changes.

Therefore, we have determined that the conclusions about global-scale phenomena in the assessment literature are not affected by inclusion or lack of inclusion of the geothermal heat issue in the climate models.

Comment (4-8):
A commenter (1924) claims that future predictions use climate models that assume that increased GHGs are the only influence on global temperature change, citing a 1990 paper by Wigley and Barnett.

Response (4-8):
After reviewing the literature, we find no support for the claim that model-based climate projections from the assessment literature, as summarized in the TSD, assume that increased atmospheric GHG concentrations are the only influence on global temperature change. Wigley and Barnett (1990) included a simple energy balance model that assessed only the impact of GHGs, but the existence of one simple 20-year-old model does not contradict the existence of hundreds of more complex models in use today. As discussed in response to other comments in this and other volumes of the Response to Comments document, models include many other factors that influence global temperature change, such as natural variability, historical volcanic eruptions (and sometimes stochastic approximations of future volcanic eruptions), aerosol effects, and land use change influences. Therefore, GHGs are not the “only influence” on global temperature increase projections in models.

Comment (4-9):
A commenter (0553) claims that tipping points exist only in general circulation models (GCMs) and have never been observed in nature as a result of CO_2 concentration changes.

Response (4-9):
EPA has reviewed the assessment literature and concludes that the commenter’s claim that tipping points only exist in GCMs is false. For example, glacial-interglacial transitions—the times when the great ice sheets advance or retreat at the beginning or end of an ice age—are fairly dramatic examples of global tipping points initiated by modest changes in the distribution of solar forcing. Please refer to Volume 3,

Section 3.2.4: CO_2 and Past Global Warming Episodes for our responses to other comments on the relationship between CO_2 and temperature over geologic time.

Comment (4-10):
Many commenters (0736, 0798, 1606.1, 2883.1, 2890.1, 2898.1, 3372, 3432.1, 3446.1), mention the lack of an observed tropospheric hotspot as a climate model flaw.

Response (4-10):
Please refer to Volume 3 for our responses to comments on the tropospheric warming in the tropics and its treatment in models.

Comment (4-11):
Commenters object to climate models, arguing that they have not predicted Antarctic cooling in the past 50 years (1606.1) or a net gain of Antarctic and Greenland ice recently (1606.1), and that they overpredicted Antarctic sea ice loss (2883.1).

Response (4-11):
See our responses on temperature trends in Antarctica and whether Antarctica is cooling in Volume 2, and attribution issues including the use of models to determine the causes of Antarctic temperature changes in Volume 3.

See responses in Volume 2 of the Response to Comments document and Section 4(f) of the TSD, which summarize the findings of the assessment literature on recent loss of ice from the Antarctic and Greenland ice sheets, contradicting the assertion of the commenter that there has been a net gain of Antarctic and Greenland ice.

Slower Antarctic sea ice retreat compared to Arctic sea ice retreat is consistent with the models, as stated in Meehl et al. (2007): “In 20th- and 21st-century simulations, antarctic sea ice cover is projected to decrease more slowly than in the Arctic (Figures 10.13c, d and 10.14), particularly in the vicinity of the Ross Sea where most models predict a local minimum in surface warming.” Further, Lemke et al. (2007) found that “the antarctic data provide evidence of a decline in sea ice extent in some regions, but there are insufficient data to draw firm conclusions about hemispheric changes prior to the satellite era,” therefore, while satellite-era Antarctic sea ice has exhibited a statistically insignificant increasing trend, it is not clear that the longer term trend has been positive. There are uncertainties involved in sea ice projections: Randall et al. (2007) note that “[d]espite notable progress in improving sea ice formulations, AOGCMs [Atmosphere-Ocean Global Climate Models] have typically achieved only modest progress in simulations of observed sea ice since the TAR [IPCC Third Assessment Report]. The relatively slow progress can partially be explained by the fact that improving sea ice simulation requires improvements in both the atmosphere and ocean components in addition to the sea ice component itself.” However, despite these uncertainties, the projections of reduction of sea ice extent in both hemispheres by the end of the century under all IPCC scenarios are robust, and the uncertainties involved do not substantially change the conclusions of the assessment literature.

Comment (4-12):
Some commenters object that models greatly exaggerate warming because global warming since the 1970s has been associated with a decrease in upper tropospheric water vapor and increase in outgoing longwave radiation (4003), or negative absolute humidity trends (2883.1).

Response (4-12):
We disagree that the evidence shows that upper tropospheric water vapor has declined. Please refer to Volume 3 for EPA's response to comments on trends in water vapor and attribution. The trends in water vapor do not support the commenter's argument that the models greatly exaggerate warming.

Comment (4-13):
A commenter (2898.1) objects to statements on attribution from the TSD because, according to the commenter, models don't properly simulate important modes of natural variability (e.g., the El Niño-Southern Oscillation (ENSO), the Pacific Decadal Oscillation (PDO), and the Atlantic Multidecadal Oscillation (AMO). Another commenter (3291.1) argues that model flaws, surface record flaws, and other uncertainties make attribution impossible, that goodness of fit is an expert judgment and therefore biased, and that using model averages smoothes out year-to-year variations. A third commenter (3330.1) claims that models are "curve fitted" to match, and that explaining global temperatures may not explain hemispheric differences. Another (3722) objects that claiming that the fact that models match historical data only if human emissions are included is an exercise in curve-fitting. Several commenters (3323.1, 4003, 4041.1, 4932.1, and 5158) state that models and the IPCC ignore the possibility of indirect solar variability (Section 2.5), which if important would again be likely to have the effect of overstating the importance of GHGs/CO_2.

Response (4-13):
Please refer to Volume 3 for EPA's responses to comments broadly focused on the extent to which observed climate change can be attributed to the observed increase in atmospheric GHG concentrations. Here, we concentrate on the issues raised by the commenters on the use of computer models in the attribution process.

Chapter 9.4.1.4 of the IPCC Fourth Assessment Report (AR4) (Hegerl et al., 2007) discusses the influence of GHGs on surface temperature and describes how statistical approaches are used with various models in order to attribute changes to GHGs, other anthropogenic influences, and natural factors. The fit within this approach does not use any human judgment, nor is it an exercise in curve-fitting: the allocation of different weights to different sources results solely from statistically determining the best match to observations using different weights for various forcings. The four models shown in Figure 9.9 in Hegerl et al. (2007) all indicate that the net contribution of natural factors to total warming over the 20th century was actually a cooling influence. No scaling factor for solar or volcanic forcing would be able to explain the trends and patterns as well as the combinations of anthropogenic and natural forcings used in the IPCC and the formal attribution literature. Further, we note that the models do explain hemispheric and continental trends as well as global trends.

If the underlying data on temperature and forcing were in error, the precise allocations would change. However, the judgment of the science community, as expressed in the CCSP, USGCRP, NRC and IPCC reports, is that it is unlikely that the errors in these underlying data could be large enough to substantively change the attribution. This judgment is expressed, for example, in the IPCC (2007b) statement that "[m]ost of the observed increase in global average temperatures since the mid-20th century is very likely due to the observed increase in anthropogenic greenhouse gas concentrations." The commenters have not provided scientific literature that supports a claim that these errors are large enough to change the conclusions on attribution.

With respect to model simulation of modes of natural variability, Randall et al. (2007) report the following:

> Since the TAR [Third Assessment Report], developments in AOGCM formulation have improved the representation of large-scale variability over a wide range of time scales. The models capture the dominant extratropical patterns of variability including the Northern and Southern Annular Modes, the Pacific Decadal Oscillation, the Pacific-North American and Cold Ocean-Warm Land Patterns. AOGCMs simulate Atlantic multi-decadal variability, although the relative roles of high- and low-latitude processes appear to differ between models. In the tropics, there has been an overall improvement in the AOGCM simulation of the spatial pattern and frequency of ENSO, but problems remain in simulating its seasonal phase locking and the asymmetry between El Niño and La Niña episodes. Variability with some characteristics of the MJO is simulated by most AOGCMs, but the events are typically too infrequent and too weak.

This shows that the models used for attribution studies do capture many important aspects of natural variability. Additionally, Hegerl et al. (2007) find that natural modes of variability would not produce the appropriate spatial patterns of change, whereas anthropogenic forcings do explain those spatial patterns:

> When human factors are included, the models also simulate a geographic pattern of temperature change around the globe similar to that which has occurred in recent decades. This spatial pattern, which has features such as a greater warming at high northern latitudes, differs from the most important patterns of natural climate variability that are associated with internal climate processes, such as El Niño.

Therefore, the accuracy with which models capture natural variability is sufficient for confidence in attribution studies that use models, especially when buttressed by the other, similar lines of evidence assessed by Hegerl et al. (2007).

Comment (4-14):

Several commenters (3323.1, 4003, 4041.1, 4932.1, and 5158) submitted a document authored by Alan Carlin that states that models and the IPCC ignore the possibility that there may be other significant natural effects on global temperatures that we do not yet understand. According to the commenters, this possibility invalidates IPCC statements implying that one must assume anthropogenic sources in order to duplicate the temperature record. The commenters argue that the 1998 spike in global temperatures is very difficult to explain in any other way. The argument includes the statement that “[p]erhaps the closest simple ‘explanation’ for the observed changes in global temperatures is provided by the PDO and/or AMO together with ENSO,” and also that the satellite temperature trend should be divided into periods, a 1978 to 1997 period with no trend, a 1998 spike, a high period from 1999 to 2006, which was possibly “an after-effect of the sudden surge in 1998,” and finally, a strong downward trend from 2007 to 2009. Carlin cites a Web site—Arrak (2008) (http://icecap.us/images/uploads/ThereWasNoGlobalWarmingBefore1997.pdf)—in support of this claim and presents a figure from Arrak (2008).

Response (4-14):

EPA has reviewed Alan Carlin’s document, as well as the body of scientific literature represented in the assessment reports. We find that the assessment literature clearly demonstrates that the historical pattern of global temperature change is due to a combination of natural and anthropogenic factors. The peak temperatures in 1998 are understood to be a result of an extremely potent El Niño event added on top of a long-term anthropogenically driven warming trend (Easterling, 2009). Historical elements of natural variability such as volcanoes, solar insolation changes, and ENSO events do not provide compelling explanations for the long-term trend of warming over the past half century as seen in both the satellite and temperature records, whereas anthropogenic changes in greenhouse gases and aerosols in combination with natural variability do provide such an explanation, both in terms of spatial and temporal patterns of change (Hegerl et al., 2007).

The method of dividing a 30-year temperature period into smaller periods and deciding that the temperature pattern is indicative of periods of no-trend with a “step” caused by the 1998 ENSO event is not a rigorous scientific approach. This division was apparently made by estimating trends visually, rather than using any rigorous statistical methodology—indeed, Arrak (2008) (a non-peer-reviewed source) justifies the analysis by stating “we humans are good at pattern recognition.” Human pattern recognition is no substitute for statistics. This document even claims that the eruptions of Pinatubo and Chichon had no cooling effect because the coincident cooling was part of a visual oscillatory pattern; we find this claim to be at odds with the robust scientific literature on the impact of volcanic sulfate aerosols. Additionally, the allocation of a “strong downward trend” from 2007 to 2009 is merely looking at very short-term variability. As shown in Figure 2.8 of Peterson et al. (2009), due to internal model variability, negative short-term trends of as long as a decade are consistent with long-term warming—therefore, we find that this “strong downward trend” is consistent with an excursion from a longer term warming trend. In the half year since the publication of this report, all monthly temperatures have been above the decadal average, demonstrating how reliance on short-term variability is not a robust methodology for trend determination. Nor is the use of only one temperature dataset, the microwave sounding unit (MSU) satellite data, appropriate when other datasets including surface temperature, ocean heat content, and a number of physical indicators such as glacial melt and rising sea level all show warming during portions of the short-term periods when there are plateaus in the MSU dataset.

Therefore, we find that the evidence that the Carlin document relies on for this analysis of recent temperature trends is very weak, and the arguments from this document do not provide reason to conclude that the major CCSP and IPCC assessments of the literature, relying as they do on multiple studies and lines of evidence, are in error.

Comment (4-15):

Some commenters (e.g., 2898.1, 3136.1) object to regional-scale model projections. For example, one commenter (3136.1) states that natural variability has large impacts on local and regional trends, attributing Alaskan warming in specific and northwest North American trends in general from 1950 to 1976 to shifts in the PDO (citing Hartmann and Wendler, 2005) and states that these trends occasionally randomly coincide with model projections. Other commenters (3316.1, 3222.1,) argue that models cannot project regional climate changes. Other problematic historical regional projections that have been raised by commenters are Antarctic and Arctic changes (3722), especially in reference to a comparison by Dr. Akasofu (2007), citing work by Bill Chapman, between models and observations from the Arctic Climate Impact Assessment (ACIA). Another commenter (3701.1) criticizes the capability of climate models to predict drought, precipitation, regional temperature, and temperature variability based on a number of studies. Commenters also criticize reliance on anomalies rather than absolute temperatures, claiming that models cannot predict absolute temperatures as well as they predict anomalies. Similarly, for historical data, a commenter (3701.1) objects that models have had a poor record of predicting climate fluctuations, regional-scale trends, and temperatures at specific weather stations.

Response (4-15):

We have reviewed the TSD in light of these comments. We have determined that the TSD accurately represents the current state of knowledge on regional-scale model projections as expressed in the assessment literature and that the assessment literature has accurately represented the state of the science. As the TSD summarizes in Section 6(b), confidence decreases in changes projected by global climate models at smaller spatial scales. Many important small-scale processes cannot be represented explicitly in models and so must be included in approximate form as they interact with larger scale features (Randall et al., 2007). Some of the most challenging aspects of understanding and projecting regional climate changes relate to possible changes in the circulation of the atmosphere and oceans, and their patterns of variability (Christensen et al., 2007). Nonetheless, the IPCC (2007d) concluded that recent advances in regional-scale modeling lead to higher confidence in projected patterns of warming and other regional-scale features, including changes in wind patterns, precipitation, and some aspects of extremes and of ice.

With regards to Hartmann and Wendler (2005), the paper shows that there is a correlation between the PDO index and the temperature data used in the paper for most regions of Alaska except the near-Arctic regions. The paper does not, however, present a rigorous attribution analysis demonstrating a lack of influence of GHGs. The analysis by the authors finds that a (not statistically significant) cooling trend in many regions of Alaska between 1977 and 2001 "is in contrast to some theories regarding the atmospheric warming in an increasing greenhouse gas environment," but local trends are a superimposition of natural variability and longer term trends. Given that the PDO statistic that the authors use to explain the increase in temperature in 1977 has a statistically significant negative trend between 1977 and 2001, a rigorous attribution analysis should at least include that natural trend. Natural variability could potentially make spurious agreements between models and a single individual region such as Alaska; however, the conclusions of the IPCC are based on a comparison of many regions to model trends, giving statistical significance to those conclusions. Similarly, as stated previously, the IPCC notes that confidence decreases for projections at smaller spatial scales and shorter time periods, although the existence of uncertainty does not eliminate the value of these projections.

With respect to the ACIA comparison, we were able to locate a peer-reviewed paper by Bill Chapman (Chapman, 2007), which was a comparison between models and climate for the Arctic region. While the analysis by Dr. Akasofu (2007) had statements such as "we found that there was no resemblance at all, even qualitatively," the paper itself stated that "[p]atterns of variability of observed and simulated surface air temperatures agree very well across all seasons," though it did identify certain regions of biases such as a strong cold bias in the models in the Barents Sea corresponding "to a region of oversimulated sea ice." As summarized in the TSD, Karl et al. (2009) found that the observed decline in Arctic sea ice has been more rapid than projected by climate models, so this finding of oversimulated ice is consistent with the TSD.

Importantly, changes in temperature on the global scale are not as sensitive to large scale forcings as are localized temperature patterns that depend on atmospheric wind patterns and shifting ocean currents.

With regards to projections of temperature changes at specific weather stations, weather stations are addressed in Volume 2. Modeling a single weather station is the extreme case of modeling small spatial scales, and therefore models are not expected to replicate weather behavior at single stations.

Finally, Section 5 of the TSD now addresses the impact of the PDO and other modes of internal variability on the likelihood of extreme temperatures, drought, precipitation, and small-scale projections:

> As with temperature, attributing changes in precipitation to anthropogenic forcing at continental or smaller scales is more challenging. One reason is that as spatial scales considered become smaller, the uncertainty becomes larger because internal climate variability is typically larger than the expected responses to forcing on these scales (Gutowski et al., 2008). For example, there is considerable evidence that modes of internal variability (such as ENSO, the Pacific Decadal Oscillation,[1] and NAM)

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[1] The Pacific Decadal Oscillation (PDO) is a pattern of Pacific climate variability that shifts phases on at least inter-decadal time scale, usually about 20 to 30 years. The PDO is detected as warm or cool surface waters in the Pacific

> substantially affect the likelihood of extreme temperature, droughts, and short-term precipitation extremes over North America (Gutowski et al., 2008).

Please refer to Volume 3 for EPA’s responses to comments on temperature attribution for Alaska in relation to the PDO as well as responses regarding attribution for Antarctic temperature trends, and Volume 2 for EPA’s responses to comments on the existing temperature trends in Alaska, the Arctic, and the Antarctic.

Comment (4-16):

A number of commenters (e.g., 0650, 2057, 2890.1, 3291.1, 3679.1, 3722) note that cloud modeling is important for accurate representation of the climate system and precipitation calculations, and yet that it is an area with a lot of uncertainty. Several commenters quote the IPCC—for example, one commenter (3291.1) provides the statement by the IPCC that “[s]ignificant uncertainties, in particular, are associated with the representation of clouds, and in the resulting cloud response to climate change. Consequently, models continue to display a substantial range of global temperature change in response to specified greenhouse gas forcing.”

Response (4-16):

We agree that cloud modeling is important for accurately representing climate system and is subject to significant uncertainties. The TSD summarizes the findings of the assessment literature on this issue and acknowledges the uncertainties, as discussed in the assessment literature, involved in modeling clouds and precipitation. For example, in Section 6(b), the TSD states, following Meehl et al., 2007):

> Models have proven to be extremely important tools for simulating and understanding climate, and there is considerable confidence that they are able to provide credible quantitative estimates of future climate change, particularly at larger scales. Models continue to have significant limitations, such as in their representation of clouds, which lead to uncertainties in the magnitude and timing, as well as regional details, of predicted climate change. Nevertheless, over several decades of model development, they have consistently provided a robust and unambiguous picture of significant climate warming in response to increasing greenhouse gases.

The TSD also addresses the confidence and limitations involved in predicting precipitation referencing both IPCC (2007d) and CCSP (2008c). We note that the conclusions in the scientific literature are that the findings are robust despite these uncertainties. A detailed analysis of uncertainties associated with cloud modeling and their relationship to overall model results is found in Randall et al. (2007), among other underlying references for the TSD. Indeed, the statement in Randall et al. (2007) directly after the quote provided by commenter (3291.1) is: “Despite such uncertainties, however, models are unanimous in their prediction of substantial climate warming under greenhouse gas increases, and this warming is of a magnitude consistent with independent estimates derived from other sources, such as from observed climate changes and past climate reconstructions.”

Comment (4-17):

A number of commenters (e.g., 0400, 0499, 3215.1, 3340, 3596.1,) object to positive feedbacks in climate models. One commenter (0400) states that there has been no runaway climate change in the past despite large changes in CO_2 and claims therefore that there must be strong negative feedbacks in the system. A

Ocean, north of 20°N. During a “warm,” or “positive,” phase, the west Pacific becomes cool and part of the eastern ocean warms; during a “cool,” or “negative,” phase, the opposite pattern occurs.

number of commenters (e.g., 0400, 3215.1, 3291.1, 3340, 3596.1) argue that some work shows that there is a bias towards over predicting feedbacks based on Spencer and Braswell (2008). One commenter (3291.1) argues that the estimate of climate sensitivity uncertainty has hardly changed since 1979 because modelers cannot characterize feedbacks, and another commenter (3330.1) claims that climate sensitivity ranges are determined by changing model parameters.

Response (4-17):

Please refer to Section 4.3 of this volume for EPA's responses to comments specific to climate sensitivity and temperature projections. In this response, we focus on the role of positive feedbacks in the model-based studies that provide one source of climate sensitivity estimates.

In response to comments, we have reviewed the work by Spencer and Braswell (2008), which argues that the observed changes in ocean temperature are due to changes in cloud cover rather than the other way around. Specifically, they show that a specific type of climate feedback estimation study based on using short-wave flux data from satellites (Forster and Gregory, 2006) can be biased if it does not account for the possibility that the temperature relationship between cloud cover and ocean temperatures works both ways. While this preliminary work is interesting, the conclusions of bias are based on a very simple model with a heat capacity equivalent to a 50 meter thick ocean, and it is not clear that the same results would be obtained from using this technique with more complex, realistic models. Certainly cloud feedbacks are one of the main areas of uncertainty in the climate field, but other recent papers such as Clement et al. (2009) continue to find that clouds contribute to positive feedbacks in the climate system. Additionally, the conclusion of low sensitivity from the Spencer et al. study is not consistent with the large body of work, as synthesized in the assessment literature, which uses multiple methods and datasets, including paleoclimate data, 20th century data, and responses to volcanic forcings, in order to estimate climate sensitivity (Hegerl et al., 2007).

While the numerical range presented in the latest IPCC assessment for climate sensitivity bears a resemblance to the range presented in the Charney report in 1979, the earlier estimate was a rough estimate with no associated probabilities. The range presented in Hegerl et al. now includes a “likely” range, a “very likely lower limit,” and a “most likely” climate sensitivity, and this range is based on a number of different climate sensitivity studies. The remaining uncertainty range is due to the difficulty in characterizing feedbacks, but these ranges are estimated based on a number of observational studies as well as computational approaches and are therefore not merely based on changing model parameters.

In regard to the comment that the absence of runaway climate change in the past (where runaway climate change refers to a small change in forcing leading to ever increasing temperatures, similar to what happened to the atmosphere of Venus) indicates that there must be strong negative feedbacks in the climate system that will prevent runaway climate change in the future, it appears that the commenter is confusing “runaway climate change” with “positive feedbacks.” In the assessment literature and the underlying science, positive feedbacks in the context of climate models do not necessarily imply runaway climate change. As temperature increases, the outgoing radiation increases as the fourth power of the temperature change (following the Stefan-Boltzman Law). Thus, a positive feedback can just mean that the total temperature change from a given forcing will be larger than the change would have been without those feedbacks; only past a certain threshold can positive feedbacks lead to runaway climate change. The likely range of climate sensitivities reported in the assessment literature are all below that critical threshold. Therefore, we find that it does not follow from the commenter's assertion regarding the lack of historical runaway warming that there are no positive feedbacks. In fact, several well-understood examples of positive feedbacks are discussed in the assessment literature, including a water vapor feedback where increased temperatures lead to increased water vapor, which lead to increased temperature (but summing to a finite total temperature change), or sea ice-albedo feedbacks where sea ice retreat in response to higher temperatures will lead to increased solar absorption and therefore enhanced temperature response of high latitudes (Hegerl et al., 2007).

Comment (4-18):
A commenter (3722) argues that the IPCC ignores the fact that models can produce sensitivities outside of their “favored range” and states that Stainforth et al. (2005) discarded as “obviously wrong” modeling experiments that produce cooling.

Response (4-18):
We have reviewed Stainforth et al. (2005) and other literature on climate sensitivities and determined that the objection to the IPCC approach of determining climate sensitivities does not give sufficient consideration to the process by which the IPCC range is determined. First, theoretical model analysis is not the only source of sensitivity estimates; as described in Section 4.3 of this volume, responses to volcanoes, paleoclimate data, and past-century data are all used in determining the most likely climate sensitivities. In addition, many different modeling studies inform the IPCC conclusions on this topic, each of which has undergone peer-review. Stainforth et al. (2005) itself is not cited in Hegerl et al. (2007), and therefore does not form part of the IPCC conclusions on climate sensitivity.

Stainforth et al. developed a system to allow users to run climate models at home and provided each user with a model with randomly selected parameter sets. Certain parameter sets were excluded based on the model never reaching equilibrium during spin-up experiments (model drift). In six out of 414 remaining cases, Stainforth et al. discarded model simulations where an interaction between a simplified slab ocean and clouds in the Pacific lead to unphysical behavior. In these cases, one symptom of the unphysical behavior happened to be cooling; Figure 1b of the Stainforth paper also shows that those six “obviously wrong” modeling experiments were definite outliers. They were not discarding estimates just because the estimates produced cooling, but rather due to this unphysical behavior; it also so happens that there were simply no comparable outliers with equivalent unphysical behavior on the warm end of the study.

While the Stainforth et al. paper is an interesting exploration of possible sensitivities that are consistent with model structure, with sensitivities ranging from less than 2°Kelvin (K) to more than 11°K, the paper does not constrain sensitivities nearly as much as the other approaches listed in Hegerl et al. (2007). It was not designed to test which parameter combinations were consistent with observations, but rather the paper was an exploration of what climate sensitivities could be produced in a model setting without leading to unphysical behaviors.

We also note that most studies do not use a slab ocean like the Stainforth experiment, because slab oceans do not represent ocean behavior as accurately as more complex ocean models. The Stainforth effort used one because it was part of the climateprediction.net experiment using thousands of home computers to run many more GCM simulations than would have been possible for a single institution. The tradeoff of this approach was the use of a simplified ocean to reduce computation time, with the result that unphysical behaviors occasionally resulted.

For these reasons, we conclude that the commenter has not presented compelling evidence in support of the claim that the IPCC ignores models that produce sensitivities outside of any favored range.

Comment (4-19):
One commenter (3291.1) cited a Massachusetts Institute of Technology (MIT) study, Forest et al. (2006), which shows that climate models often overestimate heat uptake by the ocean. The commenter suggests

that because, as the commenter claims, models are calibrated to match the climate of the past century, there must be compensating errors in these models.

Response (4-19):
We have reviewed Forest et al. (2006) and determined that it does not necessarily indicate that there are compensating model errors in addition to the potential errors in estimating ocean heat uptake. The study was referenced in the IPCC (Hegerl et al., 2007):

> Although the heat uptake in the ocean cannot be explained without invoking anthropogenic forcing, there is some evidence that the models have overestimated how rapidly heat has penetrated below the ocean's mixed layer (Forest et al., 2006; see also Figure 9.15). In simulations that include natural forcings in addition to anthropogenic forcings, eight coupled climate models simulate heat uptake of 0.26 ± 0.06 W m^{-2} (±1 standard deviation) for 1961 to 2003, whereas observations of ocean temperature changes indicate a heat uptake of 0.21 ± 0.04 W m^{-2} (Section 5.2.2.1). These could be consistent within their uncertainties but might indicate a tendency of climate models to overestimate ocean heat uptake.

Climate models, ocean heat content data, and other data will continue to improve and be updated. Inconsistencies between models and data can indicate imperfections in either the models or the data. In the 2006 study by Forest et al., nine out of 11 TAR models were out of the 90% bounds of paired ocean heat uptake and climate sensitivity as determined by the study and six of 11 were outside the 99% bounds, but using updated methodology and comparing to more recent models, a 2008 paper by Forest et al. found that only five out of 11 new Fourth Assessment Report models fell outside those 90% bounds and none outside the 99% bounds of paired ocean heat uptake and climate sensitivity. The heat uptake results from the Forest et al. studies are dependent on the ocean heat content datasets, and newer results that were not included within either Forest et al. study have been released. These newer results (from Domingues et al. 2008) corrected for expendable bathythermograph (XBT) dataset errors and show a larger ocean heat uptake than the older Levitus dataset upon which the Forest et al. studies relied. The models would therefore be more consistent with the new, updated observations than they were with the old observations.

Even if models overestimate ocean heat uptake, it is not clear that there must necessarily be "compensating errors." This is because most models are not tuned to match the historical record as the commenter claims; rather, the matching of the historical record is a result of attempting to accurately model various climatic processes in the present (Randall et al., 2007). Therefore, improvements in the accuracy of modeling these processes can actually lead to better matching of the historical record and improvements in projections. In the case of correcting an overestimation of ocean heat uptake, the most likely result is that as less heat goes into the model oceans, more heat will stay in the model atmosphere and projections of temperature change will increase.

Please refer to Section 4.3 of this volume for EPA's responses to comments specific to climate sensitivity, and Volume 3 for our response on ocean heat content and attribution.

Comment (4-20):
One commenter (3427.1) claims that observations indicate the GHGs are not as well mixed in the atmosphere as assumed in models.

Response (4-20):
The commenter did not provide any literature, and it is not clear to what observations they are referring. Clearly, near sources and sinks, GHG concentrations will not be at the "global average concentration,"

but it is well accepted that within a couple of years of being emitted, a given pulse of gas will be well-mixed throughout the troposphere. Further, we note that few climate models include an *a priori* assumption that the gases are well-mixed; most models include atmospheric dynamics that calculate the movement of gases from one place to another. The result in these models is indeed a well-mixed atmosphere, and this matches the results of observations of background atmospheric concentrations at a number of remote monitoring sites in various networks (such as the Advanced Global Atmospheric Gases Experiment (AGAGE) network).

Comment (4-21):

Some commenters assert that models include arbitrary coefficients that are necessary for the final result. One (0650) argues that a trace gas would not be sufficient to cause warming without such a coefficient, another (0798) that the assumption of constant relative humidity is flawed. A commenter (2888.1) stated that models are hardwired to assume a two-thirds of a degree Celsius (C) increase due to 1 W/m^2 of radiative forcing. Commenter (3722) provides an analysis by Dr. Akasofu (2007) that claims that global climate models cannot test attribution because they are adjusted or "tuned" to reproduce the 0.6 to 0.7 degree Celsius rise.

Response (4-21):

We reviewed the assessment literature in light of these comments and conclude that the commenters' assertions indicate a lack of familiarity with state-of-the-art climate modeling. The commenters provided no literature to support their assertions. Climate models are designed to match the physical processes of the system to the best of the modeler's ability. In some cases, this requires "parameterization" of certain processes, particularly those that take place on scales too small to capture with the resolution of the model such as cloud behavior or the physics of plume behavior. Parameterization of large scale processes is usually only the case with EMIC (Earth Models of Intermediate Complexity) models, some of which include tunable parameters such as climate sensitivity and ocean heat uptake. Most of the larger AOGCM models do not include tunable parameters for these large-scale behaviors because these behaviors are an emergent behavior of the system, rather than something that is controlled by the user. One of the tests for EMIC models is to show that they can replicate the behavior of these larger AOGCM models.

Randall et al. (2007) does address the tuning of some parameters in AOGCMs, noting that this tuning must always meet at least two conditions: that the parameters remain within observationally based constraints and that the number of degrees of freedom from tunable parameters is less than the number of observational constraints used in model evaluations. Therefore, the selection of these parameters is not arbitrary. Randall et al. also state that where models have been tuned to give a good representation of an observed quantity, then agreement with that observation cannot be used to "build confidence" in that model. If models had been tuned to match global average surface temperature trends, which is not the case for most AOGCMs, then agreement with those trends would not produce confidence in the output of the model.

With respect to concerns about assuming a constant relative humidity, we find that water vapor content is calculated by most models in response to temperature changes. Early work, such as Arrhenius' calculations in 1896, assumed a constant relative humidity as a good first guess, but now it is explicitly calculated. Further, a study by Soden and Held (2006) examined the significance of this assumption, analyzing the difference between model calculations of humidity and the assumption of constant relative humidity. They found that the calculated relative humidity of most models was within 5% of being constant, and also stated: "Interestingly, the true feedback is consistently weaker than the constant relative humidity value, implying a small but robust reduction in relative humidity in all models on average…"

For these reasons, we disagree that the models include an assumption of constant relative humidity, or that coefficients are chosen "arbitrarily" in order to set the climate sensitivity. We note that the commenters did not provide peer-reviewed literature to support their claims.

Comment (4-22):
One commenter (3160) objects that models predict meteorological surface air temperature 2 meters above the surface, while CO_2 should only make small changes in actual ground surface temperatures, which would not be detectable at 2 meters.

Response (4-22):
The commenter appears to misunderstand both how the models calculate temperatures and the nature of the warming caused by CO_2. First, models calculate temperatures throughout the vertical structure of the atmosphere. The temperature 2 meters above the surface is often reported as a standardized comparison number, and this is considered accepted practice in the assessment literature and scientific community. Second, CO_2 is expected to change temperatures throughout the atmosphere, not just at the surface. We are aware of no evidence to support the commenter's claim that CO_2 will only cause small change in ground surface temperatures, and the commenter provided no literature to support this view.

Comment (4-23):
A commenter (0700.1) contends that the UN exaggerates the Planck parameter by at least one third, because it incorrectly takes temperature and radiant-energy values from planetary emitting surfaces 6 miles apart, effectively repealing the fundamental equation of radiative transfer. Also, the commenter states that the UN fails to make any allowance for diurnal and latitudinal variations, which, according to a private communication to the commenter from Dr. David Evans, require a further 10% reduction in the value of the Planck parameter.

Response (4-23):
This comment reflects a lack of familiarity with how the climate models treat radiative transfer, and a misunderstanding of how the Earth is represented in these models. To be clear, the models used by the scientific community explicitly include radiative transfer equations. It appears the source of the confusion is that the forcings are often reported as top of the atmosphere (TOA) forcing, and the temperatures are often reported as surface temperature. Surface temperatures are reported because that is the altitude at which most humans and ecosystems exist; however, TOA forcing is reported because it is the simplest way to represent different climate forcings in a consistent manner. The fact that the models report the output in this way does not mean that the models are repealing the fundamental equations of radiative transfer. To the contrary, they explicitly calculate temperatures and radiative balances for every layer of the atmosphere in between the two altitudes, using standard thermodynamic and radiative constraint equations.

Similarly, the models explicitly calculate both diurnal and latitudinal variations. High resolution global climate models have grid cells that are less than 2 degrees in latitude and longitude and have daily cycles and latitudinal temperature gradients that are consistent with observations. No evidence was submitted to support the concern of Dr. David Evans relayed by the commenter

For these reasons, we conclude that the models treat radiative forcing appropriately and do not suffer from the weaknesses claimed by the commenter.

Comment (4-24):
Several commenters discussed modeling paleoclimatic conditions. One commenter (0736) objects to models because they do not show the medieval warming period and the following mini ice age. Another (1309.1) claimed that the models have never been backtested against ice core data. A third (3291.1) claims that historical variability in the paleoclimate record is larger than models can produce.

Response (4-24):
We have reviewed the relevant literature on this topic and find that the comments do not accurately reflect models abilities with regard to paleoclimatic conditions. The properties of the little ice age and the medieval warm period are addressed in responses within Volume 2; here, we address the question of how models represent these periods.

Models have been extensively tested against historical data on many time scales. This process isn't simple because there are more unknowns for paleoclimate data than modern data. Referencing simulations for the past millennia, Jansen et al. (2007) note that the forcing influence of orbital variations can be calculated quite accurately but are not important for the last 2000 years. However, for the past 2000 years, the ice core record provides accurate records of greenhouse gases at almost decadal resolution. There are uncertainties, however, in the forcing influence of land use changes, aerosols, ozone, solar irradiance, and volcanic effects during the preindustrial period. Despite these uncertainties, Bertrand et al. (2002), which was cited in the IPCC, found that a "combination of solar and volcanic forcings can explain the Little Ice Age and the Medieval Warm Period." The simulations by Bertrand et al. showed a medieval warm period in the interval 1000 to 1300 AD and a cold period from 1450 until an 18th century recovery using a particular solar irradiance history and a volcanic reconstruction based on Greenland ice sheet data. The simulation did not show that the medieval warm period was warmer than present, and the simulation was unable to reproduce 20th century temperatures without including anthropogenic forcings.

For the last glacial maximum, Jansen et al. found that models "adequately represent many of the major processes that determine this past climate state." The success of modeling the last glacial maximum demonstrates both that models have been tested against ice core data and that they can capture at least some modes of variability. However, Jansen et al. did find that models had not been used to try and replicate the "abrupt decadal to centennial-scale changes in the regional frequency of tropical cyclones, floods, decadal droughts and the intensity of the African-Asian summer monsoon," which are very likely to have occurred in the last 10,000 years but whose mechanisms are not well understood. The inability to simulate certain poorly understood processes in the distant past does not, however, imply that models are not well suited to modeling near term climate change, especially given that much more data exists for the current climate state than for states 10,000 years ago.

Therefore, given that models have been used to explain the little ice age, the medieval warm period, glacial maxima, and many other paleoclimate phenomena, we find that the assertions of the commenters are unsubstantiated.

Comment (4-25):
Some commenters (e.g., 1924, 3291.1, 3340, 7031) claim that the models have never been sufficiently validated, and several (3567.1, 3679.1, 3701.1) include a reference to Green and Armstrong (2007). One states that mathematical models cannot be extrapolated for more than twice the validation period (1924). Another differentiates "calibration" from "validation" (3291.1), stating that the calibration models undergo to match historical observations cannot be assumed to improve future projections without using a validation data set. A commenter (3316.1) states that the lack of validation means model results do not meet Information Quality Act (IQA) requirements. One commenter (3729.1) requests that models be tested on weather predictions for validation, another (4003) that they make predictions a season or year

ahead. And one commenter (3702.1) argues that legal case history requires that EPA explain assumptions and methodologies of all models used in rulemaking and that therefore the EPA must identify and comprehensively discuss all models used in the underlying synthesis reports. Several commenters (3222.1, 3316.1) address issues with the surface temperature record: one notes that this record is used to validate models and therefore the usefulness of this validation is in doubt.

Some commenters (0639.1, 3224, 3701.1) also request that model codes be made public if they are to be used for policy decisions. Others (3603.1, 3702.1) request that all the assumptions, uncertainties, and methodologies involved in models used to support the TSD be made public.

Response (4-25):

Please refer to previous comments in this section for EPA's responses to the difference between weather and climate in regards to requests that models be tested on weather, seasonal, or annual predictions. See Volume 2 for EPA's responses to the validity of the surface temperature record. In this response, we focus on model testing and the validity of model-based data.

We have reviewed the Green & Armstrong critiques referenced by a number of commenters, and we have concluded that they are flawed. Green & Armstrong evaluated Randall et al. (2007), "Climate Models and Their Evaluation," by using their own set of "forecasting principles" and determined that the chapter did not meet their criteria for a good forecast. However, Randall et al. was an evaluation of climate models in the literature; it was not a forecast and it certainly was not a policy analysis or recommendation. Because the purpose of Randall et al. was to assess climate models and modeling, it should not be a surprise that many of the principles that Green & Armstrong evaluated are not addressed within the IPCC chapter. For example, the first two "violations" (a term used by the commenter to describe how Randall et al. did not meet their specifications) listed, "Describe decisions that might be affected by the forecast" and "Prior to forecasting, agree on actions to take assuming different possible forecasts" are antithetical to a document that is designed to review existing science literature. Other similar "violations" included not performing cost-benefit analysis, testing client understanding of methods, and tailoring the analysis to the decision. Nothing in the work of Randall et al. was intended to recommend or assess decisions or actions that might be taken in response to particular forecasts. Many of the recommendations made by the Green & Armstrong paper seem not to be applicable to scientific research models of complex physical systems.

With regard to the apparent assumption by Green & Armstrong and various commenters that models are "calibrated" against historical global average temperature records, this is not true. See EPA's response to comments about "tuning" in response 4-21 in this section. As stated in Randall et al. (2007), "[i]f the model has been tuned to give a good representation of a particular observed quantity, then agreement with that observation cannot be used to build confidence in that model." However, because models have not been tuned to match global average surface temperature records, or a number of other historical climate variables, these variables are all valid comparisons to make for testing the models.

We note that the code for several major climate models is indeed public, such as GISS ModelE and the Community Climate System Model, and therefore it is possible for outside agencies to perform their own, independent verification of these models. Cases cited by commenters do not indicate that the agency's reliance on synthesis reports is inappropriate in any way, or that EPA is required to identify and comprehensively discuss all models used in the underlying synthesis reports. In fact, the cases confirm that agencies are granted an "extreme degree of deference" when they are "evaluating scientific data" *Appalachian Power Co. v. EPA*, 249 F.3d 1032, 1052 (D.C. Cir. 2001). In that case, the court specifically noted the value of models in regulations under the Clean Air Act, holding that the agency's use of models would only be arbitrary and capricious "when the model bears no rational relationship to the characteristics of the data to which it is applied." *Id.* (citations omitted). The court also noted that they can overturn the model only when it is "so oversimplified that the agency's conclusions from it are unreasonable." *Id.* (citations omitted). *Appalachian Power* does not mandate a line-by-line annotated defense of agency choice of science—it merely calls for an "actual reason articulated by the agency at some point in the rulemaking process." *Id.* at 1053-1054. EPA has fully explained and justified its use of the assessment literature and therefore the models used in that literature; its use of these reports is very reasonable. See Volume 1 for explanation of the use of the assessment literature in the TSD and the Finding.

Please see response number 4-36 regarding the use of models and compliance with Data Quality Act guidelines.

Comment (4-26):

Some commenters (e.g., 0169, 0400, 0454, 0545, 0582, 0736, 0798, 2818, 2882, 2898.1, 2933, 3446.1, 3477) argue that computer models have failed to predict recent temperature changes, especially temperature trends over the past decade, and are therefore flawed. Several commenters (e.g., 2883.1, 2898.1, 3215.1, 3596.1, 3596.2) state that 30-year temperature trends were at or below the low end of IPCC projections, and a few commenters (2898.1, 3215.1, 3596.2) include extensive further analysis of these trends compared to model trends. Two comments on recent warming trends (3136.1, 3316.1) cite Keenlyside et al. (2008) and note that none of the models for B1, A1B, or A2 have a 15 to 20 year period without warming, which, according to the commenters, means that models are overpredicting warming and also that the delay in warming leads to a delay in any feedbacks involving ocean temperatures, and that therefore climate models are flawed. Another commenter (3316.1) cites Keenlyside as well as Tsonis et al. (2007) to point out that some modelers are trying to take the warming pause into account, whereas, according to the commenters, the TSD does not include any such efforts. Some commenters (4528, 4666, 4670, and 4766) submitted a CATO-sponsored advertisement signed by a number of scientists which stated that: "The computer models forecasting rapid temperature change abjectly fail to explain recent climate behavior."

Response (4-26):

Please refer to Section 4.3 of this volume for EPA's detailed comments on future projections of temperature. Here we confine our response to the components of the comments relevant to the relationship between recent temperature trends and the validity of model-based projections.

Two recent studies have addressed whether recent temperature trends are consistent with model runs, and both Easterling and Wehner (2009) and Knight et al. (2009) find that the recent trends are well within model variability. Recent natural events not captured in the models such as the decrease in solar insolation and the La Niña event of the past year would lead to an expectation of less warming than the model average would suggest.

Knight et al. (2009) report the following:

> ENSO-adjusted warming in the three surface temperature datasets over the last 2–25 yr continually lies within the 90% range of all similar-length ENSO-adjusted temperature changes in these simulations (Fig. 2.8b). Near-zero and even negative trends are common for intervals of a decade or less in the simulations, due to the model's internal climate variability. The simulations rule out (at the 95% level) zero trends for intervals of 15 yr or more, suggesting that an observed absence of warming of this duration is needed to create a discrepancy with the expected present-day warming rate.

Easterling and Wehner (2009) reach a similar conclusion:

> What does this say about the variability of the climate system? Climate models are often criticized for producing a more or less monotonic-type response to anthropogenic forcing in 21st century simulations. Part of this may be due to the lack of volcanic and solar forcing in the SRES [IPCC Special Report on Emissions Scenarios] scenarios of anthropogenic forcing increase for the 21st century and part could be due to the fact that largescale oscillatory climate features, such as the El Niño-Southern Oscillation are not well simulated. However, even considering these criticisms, it is clear that the models can and do produce sustained multi-year periods of ''cooling'' embedded within the longer-term warming produced in the 21st century simulations. Therefore, it is reasonable to expect that the natural variability of the real climate system can and likely will produce multi-year periods of sustained 'cooling' or at least periods with no real trend even in the presence of long-term anthropogenic forced warming. Claims that global warming is not occurring that are derived from a cooling observed over such short time periods ignore this natural variability and are misleading.

Additionally, it is important to note that even the coldest year of this decade would have been one of the warmest of the previous century according to the surface temperature record datasets.

We have also reviewed the studies by Keenlyside et al. (2008) and Tsonis et al. (2007), which several commenters cited as evidence that some scientists believe that there may be multidecadal pauses in warming. The Keenlyside et al. paper uses a new methodology that involves calibrating the model oceans against observations, unlike most models that initialize the ocean in the preindustrial period and let the model respond to historical forcing. They find that this calibration improved the ability to predict certain types of northern Atlantic and eastern Pacific behavior for up to 10 years after the ocean calibration step, compared to models not using this methodology (e.g., 20th century radiative forcing simulations). The results from the paper "suggest that global surface temperature may not increase over the next decade, as natural climate variations in the North Atlantic and tropical Pacific temporarily offset the projected anthropogenic warming." However, the paper also states that "[h]indcast skill for global mean temperature (Fig. 4) is also high, but slightly less than the twentieth century RF [radiative forcing] simulations." Therefore, we do not find that the future global mean temperature trend projections from this paper are superior to the models from the assessment literature. Also, Keenlyside et al. make no claims that models are overpredicting warming in the long run; the models they use are based on the standard models, and the methodology applied only changes short term (decadal) projections.

Tsonis et al. (2007) examined historical temperatures and several climate indices (the Pacific Decadal Oscillation, the North Atlantic Oscillation, the El Niño/Southern Oscillation, and the North Pacific Oscillation). The paper found that when at several junctures where these climate indices had synchronized behavior, followed by an increase in coupling strength during the destruction of this synchronized behavior, there were concurrent shifts in global mean temperatures (1910, 1940, and the late 1970s are identified). Tsonis et al. find that this same behavior occurs in climate model runs that they examined. This paper makes no claim regarding the recent temperature trends and notes that these climate synchronization events can be "superimposed on an anthropogenic warming trend." Given that Tsonis et al. find that models have similar behavior to the natural system in this regard, these results do not show any flaws in the assessment models.

Therefore we find neither Tsonis et al. (2007) nor Keenlyside et al. (2008) contradict the hypothesis of a significant anthropogenic influence on warming. Additionally, because Easterling and Wehner (2009) and Knight et al. (2009) show that observations are still consistent with the results of the models used in the IPCC and CCSP reports, there is no need to rely on the mechanisms proposed by Tsonis et al. or Keenlyside et al. in order to explain recent temperature trends.

Comment (4-27):

One commenter (2890.1) quotes the TSD as stating "First, despite remaining imperfections, the current generation of climate models accurately portrays many important aspects of today's weather patterns and climate" and then goes on to state that "But being almost right is being wrong. Ether [sic] the model is accurate or it's wrong." Another commenter (3679.1) states that CCSP 3.1 is less confident about model results than EPA.

Response (4-27):

We have reviewed the relevant literature in light of these comments and disagree that models cannot contribute useful information to climate science and assessments without being 100% accurate or that the TSD is inconsistent with CCSP 3.1 (2008c). We find that the TSD provides a reasonable and accurate summary of the assessment literature, particularly CCSP 3.1, in terms of both key findings and major remaining uncertainties.

CCSP 3.1 states:

> The set of most recent climate simulations, referred to as CMIP3 models and utilized heavily in Working Group 1 and 2 reports of the Fourth IPCC Assessment, have received unprecedented scrutiny by hundreds of investigators in various areas of expertise. Although a number of systematic biases are present across the set of models, more generally the simulation strengths and weaknesses, when compared against the current climate, vary substantially from model to model. From many perspectives, an average over the set of models clearly provides climate simulation superior to any individual model, thus justifying the multimodel approach in many recent attribution and climate projection studies.

and

> The use of computers to simulate complex systems has grown in the past few decades to play a central role in many areas of science. Climate modeling is one of the best examples of this trend and one of the great success stories of scientific simulation. Building a laboratory analog of the Earth's climate system with all its complexity is impossible. Instead, the successes of climate modeling allow us to address many questions about climate by experimenting with simulations—that is, with mathematical models of the climate system. Despite the success of the climate modeling enterprise, the complexity of our Earth imposes important limitations on existing climate models.

We reflect this in the Section 6.2(b) of the TSD, where we quote CCSP3.1, Meehl et al. (2007), Randall et al. (2007), and Christensen et al. (2007) on limitations of modeling, such as this description of the results of the CCSP (2008c) report:

> The CCSP (2008c) report *Climate Models: An Assessment of Strengths and Limitations* finds that models "have been steadily improving over the past several decades," "show many consistent features in their simulations and projections for the future," and "are able to simulate the recorded 20th century global mean temperature in a plausible way." However, it cautions that projections of precipitation in some cases remain "problematic" (especially at the regional scale) and that "uncertainties in the climatic effects of manmade aerosols (liquid and solid particles suspended in the atmosphere) constitute a major stumbling block" in certain modeling experiments. It adds that "uncertainties related to clouds increase the difficulty in simulating the climatic effects of aerosols,

> since these aerosols are known to interact with clouds and potentially can change cloud radiative properties and cloud cover."

In addition, the argument of the commenter that if you are not 100% right you are wrong imposes a standard that is unreasonable. We confront uncertainty in many disciplines—medicine, energy, economics, construction, aerospace engineering—and in these areas we apply the information and models we have to reach reasoned conclusions. As is discussed in Section II of the Findings, neither the law nor common sense requires 100-percent certainty before we can reach a conclusion and act. Thus, the issue at hand is not "Are the models perfect?" but "Are they reasonable and useful representations of our understanding of the climate system?" This issue has been thoroughly reviewed by the CCSP, which subjected the climate models to unprecedented scrutiny, described their uncertainties and limitations, and concluded that climate modeling is "one of the great success stories of scientific simulation."

Thus, we conclude that it is reasonable and appropriate to rely on the assessment literature, and that climate models that do not (and cannot) accurately model every aspect of the global climate are nonetheless useful for attribution, projections, and understanding of climate phenomena. This is particularly the case when multiple models are applied, as in done in the climate assessment literature, and the results are examined across them all. Further, we find that no changes need be made to the discussion in Section 6 of the TSD.

Comment (4-28):

One commenter (3283.1) points to a statement in the TSD that model projections don't begin to diverge until 2030 and asks how models can be validated against observations in that case. Commenter also objects to 100% probability that the vast majority of the last hundred years' temperature change is due to anthropogenic GHGs without attributing changes to aerosols, land use change, measurement bias, heat island, and ocean cycles.

Response (4-28):

With respect to the first part of this comment, the statement in the draft TSD, released in April 2009 along with the Proposed Findings, was: "Through about 2030, the warming rate is mostly insensitive to choices between the A2, A1B, or B1 scenarios." This insensitivity to choice of emissions scenario is between a limited set of SRES greenhouse gas emission scenarios, not necessarily to climate model parameters, choice of climate model, or even sensitivity to policy scenarios that might involve changes in aerosol or other short-lived emissions species. The reason for this insensitivity is the long lifetime of most of the GHGs, the inertia of the climate system, and the magnitude of natural, short-term variability. Therefore, while it would be difficult to use temperature to distinguish between SRES scenarios on the 20-year time scale, it may be possible to use the temperature data to better constrain a number of other characteristics of models.

This issue is distinct from the issue of model validation, and there are a number of ways in which models are validated that do not require more information about near-term temperature change. For example, models are validated against events such as the eruption of Mount Pinatubo, or based on their ability to simulate small-scale phenomena such as ENSO variability, or against longer timescale historical records. Individual components of the models can also be tested, such as the ability of carbon cycle models to reproduce carbon-14 oceanic uptake trends. A number of such validation tests are discussed both in Randall et al. (2007) and CCSP (2008c), where they discuss a number of successes as well as some remaining difficulties.

We find the commenter's objection to the 100% probability that the vast majority of the last 100 years of warming was due to anthropogenic GHGs confusing, because the TSD and the underlying assessment literature that it summarizes do not attribute anything with 100% probability and do not attribute past temperature changes to only one source. IPCC (see, for example, Hegerl et al., 2007) and other underlying references for the TSD include an extensive analysis of how the last century's temperature change is attributed to a combination of greenhouse gases, aerosols, land use changes, and other sources of natural variability. The conclusion of the IPCC is that most of the increase since the mid-20th century is very likely due to the observed increase in greenhouse gas concentration. In contrast, the IPCC does find that the warming of the system is unequivocal. Please refer to Volume 3 for EPA's responses to comments on the attribution of observed climate change to the observed increase in GHG concentrations.

Comment (4-29):
One commenter (3281) states that "[t]he worst case valid models forecast change in temperature due to anthropogenic forcings is a non-endangerment 0.27° C in 2100."

Response (4-29):
The commenter provides no documentation on what constitutes a valid model and no reference to the modeling simulation that projects a temperature change of only 0.27°C in 2100. In the large body of literature assessed by the IPCC, CCSP, NRC, and USGCRP reports, there are a large number of valid projections which show worst case temperature changes exceeding 5°C or more. See, for example, Meehl et al. (2007) with model mean warming over the 21st century ranging from 1.1° to 6.4° C. In light of the stark inconsistency between the comment and the assessment literature, and the lack of any supporting evidence, we disagree.

Comment (4-30):
In regard to the tropospheric warming over the tropics and the level of agreement between modeled and observed data, one commenter (3136.1) requested that the TSD cite Douglass (2007) and Santer (2008) in order to show that the discrepancy is "far from settled."

Response (4-30):
Please refer to Volume 3 for EPA's responses to comments specific to the anthropogenic fingerprint in the tropical troposphere.

Comment (4-31):
One commenter (3136.1) objects to a statement in the TSD, asking "Why will soils be drier given increasing precipitation and cloud cover (the latter of which mitigates against higher evaporation)? Because a model says so? When, in reality, over the last 100 years, precipitation increases have been far greater than evapotranspiration across the U.S.?"

Response (4-31):
The commenter objects to the statement in Section 8 of the TSD that "The IPCC (Field et al., 2007) reported with very high confidence that in North America disturbances like wildfire are increasing and are likely to intensify in a warmer future with drier soils and longer growing seasons."

Changes in soil moisture, and therefore dryness, are a function of the difference between water gain and water loss. Increased temperatures increase evaporation, and therefore, all other things being equal, should lead to increased dryness. However, increased precipitation (which increases water input, minus runoff losses) and increased cloudiness (which should decrease evaporative losses) can both reduce dryness. Therefore, future dryness will be a function of future temperature, precipitation, and cloudiness, among other variables.

The commenters provided a reference, McCabe and Wolock (2002), to support the claim that "precipitation increases have been far greater than evapotranspiration." We reviewed this paper, which found that "[t]rends in annual surplus and annual deficit suggest that the eastern US has become slightly wetter and the western US has become slightly drier during the period 1895–1999." In contrast to the assertion by the commenter that precipitation increases were "far greater" than evapotranspiration, the paper uses the term "slight" to describe the increase in annual surplus across the country, as well as finding drying in the western United States. As shown by the slight drying of the western United States, it does not follow that "increasing precipitation" implies an even distribution of precipitation geographically or temporally, or that increasing total precipitation will counteract the increased dryness projected for certain regions and seasons. As stated in Karl et al. (2009) "While it sounds counterintuitive, a warmer world produces both wetter and drier conditions. Even though total global precipitation increases, the regional and seasonal distribution of precipitation changes, and more precipitation comes in heavier rains (which can cause flooding) rather than light events."

Other observational studies support the contention that a warmer future can have drier soils in some regions or some seasons despite higher levels of national precipitation. The IPCC states that historically, "[d]espite the overall national trend towards wetter conditions, a severe drought has affected the southwest United States from 1999 through 2008 (see Section 4(l)), which is indicative of significant variability in regional precipitation patterns over time and space." Karl et al. (2009) found that increased extremes of summer dryness and winter wetness consistent with future projections have already been observed, not just modeled. Moreover, Jansen et al. (2007) found that some evidence suggests that during the past 2000 years, warmer than average summer temperatures were associated with particularly extensive, severe, and frequent droughts.

Model projections find that decreases in precipitation are actually likely in subtropical regions and the southwestern United States, even though precipitation is expected to increase globally. Karl et al. (2009) report model projections of future precipitation in the United States generally indicate northern areas will become wetter, and southern areas, particularly in the West, will become drier. Karl et al. also find that "[p]rojected increases in precipitation are unlikely to be sufficient to offset decreasing soil moisture and water availability in the Great Plains due to rising temperatures and aquifer depletion."

Therefore, we do not find that either observational data or model projections contradict the IPCC conclusions on the contributions of drier soils in a warmer future.

Comment (4-32):
One commenter (0634) discusses a large number of uncertainties in model simulations such as emissions projections, temperature and sea level rise projections, and even larger uncertainties in quantifying other impacts such as hurricanes, flooding, drought, etc. This commenter notes that in several cases impacts have already been greater than projected, such as accelerated ice melting and CO_2 emissions. This commenter therefore sees uncertainty as a cause for worry, not inaction.

Response (4-32):
Please refer to Volume 1 for EPA's responses regarding uncertainty. Please refer to Volume 9 for EPA's responses regarding the legal justification for acting under some continued uncertainty and the precautionary principle.

Comment (4-33):
One commenter (2895) states that more recent information on the rate of climate change indicates that the models are actually too conservative in their projections. The commenter states that the actual effects are occurring more rapidly and are likely to be much worse than the mid report scenarios that the scientists relied upon in their IPCC reports. In summary, the commenter indicates the proposed findings are correct and urges EPA to finalize them as proposed.

Response (4-33):
Although the uncertainty involved in projecting future climate change and impacts, as discussed in the TSD, may mean that climate change outcomes will be worse than the median projections from models, we have concluded that the TSD reflects the best science as currently incorporated in the assessment literature.

Comment (4-34):
Two commenters (3462.1, 3603.1) assert that the models referred to by EPA in the Proposal and TSD do not accurately capture effects due to climate change in the U.S. as a consequence of their global coverage. Commenter 3462.1 stated: "We believe EPA has an obligation to define how those models can be applied accurately to determine U.S.-specific health impacts and not simply extrapolate conclusions from broader information." Commenter 3603.1 stated: "Thus, to the extent that the TSD heavily relies on assessment or synthesis report that are based on modeling that is global in nature, with little focus on the U.S., it raises doubts about the efficacy of the TSD and accordingly the Proposed Findings to the extent that they may be dependent on the Revised TSD."

Response (4-34):
With respect to climate modeling, we agree that the TSD section on projected changes in U.S. temperature, precipitation patterns, and sea level relies in part on models that are necessarily global in scope, given that climate changes in one region can, in turn, affect climate in other regions. However, we disagree that our treatment of model results was inappropriately focused on global impacts or failed to adequately distinguish between U.S. and Canadian effects. And we also disagree that EPA's use of IPCC's regional analysis of North America is inappropriate or scientifically flawed.

First, as made clear in the TSD, many important regional differences are discernible from the model projections assessed by IPCC and CCSP. For temperature, the TSD notes that warming will be regionally variable. It summarizes information from the assessment literature that the largest warming through 2100 is projected to occur in winter over northern parts of Alaska, while warming near the coasts is not as large. For sea level rise, the TSD notes that the projected rate of sea level rise off the low-lying U.S. South Atlantic and Gulf coasts and for western Alaska is higher than the global average. For water resources, the TSD describes how precipitation intensity will increase particularly at mid and high latitudes where mean precipitation also increases. Other sections of the TSD also include considerable regional detail. To further strengthen the TSD's focus on region-specific impacts, we have added a chapter to the TSD that focuses specifically on regional impacts.

Second, for straightforward and entirely defensible scientific reasons many projections for North America are just as applicable to the U.S. as they are to the continent as a whole. It stands to reason, for instance, that if temperatures are projected to rise by a certain amount when averaged across the whole continent, they should be expected to rise by at least that much in areas of the U.S., such as Alaska, that both climate models and basic physical principles indicate are most susceptible to GHG forcing. Similarly, when the IPCC reports that 94% of the 355 significant observed changes to physical systems and 92% of the 455 significant observed changes to biological systems in North America were consistent with warming (Rosenzweig et al., 2007), it stands to reason that a majority of observed changes in the United States were also consistent with warming. There are important unresolved issues at finer geographic scales (e.g., although precipitation is expected to decrease in the southwest U.S. but increase over the rest of the continent, it is not clear precisely where the dividing line will fall) but the assessment literature appropriately denotes which outputs of continental-scale projections are robust and which are uncertain, and these conclusions and uncertainties are accurately and appropriately summarized in the TSD.

Comment (4-35):
At least one commenter (3316.1) argues that EPA relied on General Circulation Model (GCM) outputs it could not introduce in a federal court under the *Daubert* standard and thus is an "impermissible basis" for an endangerment finding.

Response (4-35):
Daubert does not foreclose EPA's use of GCM outputs. *Daubert* applies to evidence introduced in a "federal trial," not an agency rulemaking under the applicable statute (e.g., the Clean Air Act) (Daubert v. Merrell Dow Pharmaceuticals, 1993, page 582). *Daubert* assigns trial judges "the task of ensuring that an expert's testimony both rests on a reliable foundation and is relevant to the task at hand" (*Id.* at 597-598). The *Daubert* principles apply only at a trial where scientific evidence is presented to a "trier of fact," not at the rulemaking stage. To the extent commenter suggests EPA has failed to adequately explain its use of GCM outputs, EPA disagrees. Please see other comments within this section for further discussion of the validity of EPA's use of GCM outputs.

Comment (4-36):
A number of commenters (3136.1, 3372.1, 3415.1, 3548.1, 3596.1, 3679.1, 3729.1, 3747.1, and 10345) argue that EPA did not use models that comply with the IQA. Specifically, several of these commenters (3136.1, 3372.1, 3394.1, and 3747.1) argue that the models used by IPCC Fourth Assessment Report/Working Group 1 are incapable of reproducing behavior of the last decade. Other commenters (e.g., 3316.1) state that lack of validation means model results do not meet IQA requirements.

Response (4-36):
Please see Volume 1 for EPA's general response to the information quality concerns submitted during the public comment process. The Data Quality Act does not impose its own standards or establish any requirements regarding the quality of agency information; instead, it requires only that an agency "issue guidelines" ensuring data quality.

With respect to the IQA issues raised regarding the use of climate models, the *Guidelines for Ensuring and Maximizing the Quality, Objectivity, Utility and Integrity of Information Disseminated by the Environmental Protection Agency* state that the EPA should use the "best available science and supporting studies conducted in accordance with sound and objective scientific practices, including, when available, peer reviewed science and supporting studies." The climate models and other models used in the IPCC and CCSP report meet the standards of the best available science available: they have been validated and compared against theory, observation, and other models. The models are routinely used in scientific practice throughout the country and the world. The TSD appropriately represents both the strengths and limitations of the models, as determined by the major assessment reports. Therefore, the model results presented in the TSD are consistent with EPA's *Guidelines*.

With respect to the comment on EPA models and the IQA, the only EPA models referenced in the TSD and endangerment finding are those used in development of the Interim Report (U.S. EPA, 2009a) that is summarized in Volume 5, Section 2: Air Quality. In addition, please see previous responses to comments in this section on specific modeling issues.

See Volume 1 for our response to general comments on the procedures and approaches taken by the IPCC and USGCRP/CCSP in their assessment reports. The TSD's summary of model-based findings from these assessments is consistent with EPA's *Guidelines for Ensuring and Maximizing the Quality, Objectivity, Utility and Integrity of Information Disseminated by the Environmental Protection Agency* (U.S. EPA, 2002).

4.2 Future Projections of Greenhouse Gas Emissions and Concentrations

Comment (4-37):

A number of commenters argue that the projections of increasing CO_2 emissions and concentrations over the next century included in the TSD are flawed, though the commenters are divided in whether they think that the projections are overestimates or underestimates. Many commenters claim that projections of both concentrations and emissions are too high. One commenter (3281.1) who claims that projections are too large shows results from a non-peer reviewed model that had much lower emissions projections than the IPCC, and also claims that the IPCC forecast CO_2 concentrations for 2004 were 398 parts per million (ppm), showing that the IPCC overprojects CO_2 concentrations. Another commenter (2895) claims that carbon emissions might increase at most to 7 billion or 8 billion tons per year in the next century, providing no supporting evidence for this assertion. Another (3440.1) states that CO_2 is accumulating in the atmosphere at half the rate projected by the IPCC, and that projections of CO_2 concentrations therefore need to be halved. One commenter (3394.1) requests a "better" analysis of the year-to-year variation in CO_2 concentration growth rate asking if this variation would have any implications for attribution or projections. Another commenter (3756.1) claims that models are overpredicting climate change because they do not take into account peak oil or the response of biomass to increased CO_2; this commenter further claims that the models already overpredict atmospheric CO_2 concentrations. This commenter presents a biokinetic model based on fitting an equation to CO_2 concentrations since 1965, which—coupled with lower emissions estimates—predicts that CO_2 concentrations will peak at 412 ppm in 2034 and then decline.

Finally, several commenters (3323.1, 3702.1, 4003, 4041.1, 4932.1, and 5158) argue that the recent recession is not accounted for in the old SRES projections and requests that EPA revise emissions scenarios based on newer data. To the extent that ambient GHG levels are relevant for future global temperatures, these commenters state that these emissions reductions should greatly influence the adverse effects of these emissions on public health and welfare. According to one commenter, the April 2009 TSD did not reflect the emission changes that have already occurred nor those that are likely to occur in the future as a result of the recession. The commenter objects to the fact that the topic is not even discussed.

Commenters who claim that the projections are too low also discuss emissions projections and uptake. Commenter (3414.3) notes that the carbon cycle can be a dangerous positive feedback for global warming, citing an article that quots Chris Field, co-chair of IPCC Working Group II, as stating: "There is about 1,000 billion tons of carbon in these [permafrost] soils," with staggering implications for global climate. Chris Field was further quoted as stating: "The IPCC fourth assessment didn't consider either the tundra-thawing or tropical forest feedbacks in detail because they weren't yet well understood." One commenter (3475) submitted Solomon et al. (2009) on irreversible climate change and Raupach et al. (2007) as evidence that emissions growth of CO_2 exceeds worst case IPCC emissions scenarios from 2000 to 2004.

Response (4-37):
We have reviewed the TSD and the relevant assessment reports in light of these comments, and we disagree with the general assertion that the emissions and concentrations projections summarized in the TSD are unrealistic. The TSD summarizes the findings of the IPCC (2007) and CCSP (2007b) assessments. As stated in the Section 6 of the TSD regarding the IPCC scenarios:

> The main drivers of emissions are population, economic growth, technological change, and land use activities, including deforestation. The detailed underlying assumptions (including final and primary energy by major fuel types) across all scenarios, and across all modeling teams that produced the scenarios, can be found in IPCC (2000). The range of GHG emissions in the scenarios widen over time to reflect uncertainties in the underlying drivers.

The IPCC scenarios thus span a large range of possible futures. As noted in the TSD, total *cumulative* (1990 to 2100) CO_2 emissions across the SRES scenarios range from 2,826 gigatonnes of CO_2 ($GtCO_2$) (or 770 Gt of carbon [GtC]) to approximately 9,322 $GtCO_2$ (or 2,540 GtC) (IPCC, 2007c) and according to the IPCC (2007c), baseline annual emissions scenarios published since SRES are comparable in range to those presented in the SRES scenarios (25 to 135 $GtCO_2$-equivalent per year in 2100).

The CCSP scenarios were created using models from three research groups:

> As instructed in the Prospectus for the study, the modeling teams used model input assumptions they considered *meaningful* and *plausible*. The resulting reference scenarios provide insights into how the world might evolve without additional efforts to constrain GHG emissions, given various assumptions about principal drivers of these emissions such as population increase, economic growth, land and labor productivity growth, technological options, and resource endowments.

The reference scenarios show growing emissions of GHGs over the century. Emissions of CO_2 from fossil fuel use and industrial processes increases from less than 7 GtC per year in 2000 to between 22.5 and 24.0 GtC per year by 2100, leading to CO_2 concentrations increasing by 2.5 to more than three times pre-industrial levels. Increases in non-CO_2 GHGs vary more widely across reference scenarios.

Two of the models used in the CCSP exercise also project that the global net sink of CO_2 from terrestrial systems, including net deforestation, will grow over time mainly because of reduced deforestation and CO_2 fertilization of plants. All three models project growing uptake of CO_2 by the global oceans over the century.

We recognize that the range of emissions associated with the available scenarios is large. This is because there is significant uncertainty in the major drivers of emissions (e.g., such as population growth, economic growth, and technological change). We do not believe it is possible or necessary to identify the most likely future emission level, although there are many reasons to posit that emissions will be higher than the lowest of the IPCC scenarios. Certainly, that is the conclusion of CCSP (2008b) in the absence of emissions mitigation policies.

With respect to the some of the specific concerns raised by commenters, contrary to the assertion that emissions projections have been too high, a key result in Raupach et al. (2007) was that emissions growth of CO_2 from 2000 to 2004 was above even the A1FI SRES scenario. Le Quéré et al. (2009) estimated that 2008 emissions were 8.7 GtC per year; this means that emissions have already exceeded the projected

2010 fossil CO_2 emissions from 33 out of 40 SRES scenarios provided at http://www.grida.no/publications/other/ipcc_sr/?src=/climate/ipcc/emission/ (IPCC, 2000), and exceeded the projected 2010 fossil CO_2 emissions from all three CCSP 2.1a scenarios. In the absence of emissions mitigation policies, the best estimates in the literature all project continued growth in emissions; we find that a plateau of 7 to 8 GtC a year is unrealistic (and obsolete, given the 2008 emissions).

As noted, the CCSP and IPCC projections all include projections of CO_2 sinks. Increasing CO_2 will continue to lead to ocean and terrestrial system uptake over the century (Denman et al., 2007). However, Meehl et al. (2007) found

> …unanimous agreement among the coupled climate carbon cycle models driven by emission scenarios run so far that future climate change would reduce the efficiency of the Earth system (land and ocean) to absorb anthropogenic CO_2. As a result, an increasingly large fraction of anthropogenic CO_2 would stay airborne in the atmosphere under a warmer climate.

We find that the biokinetic model developed by one commenter is not a realistic alternative to the carbon cycle models included in the assessment literature. This biokinetic model assumes that the rate of biomass growth is proportional to the difference between current CO_2 and pre-industrial CO_2, times the total biomass. This means that if concentrations are stabilized—rather than the ecosystem sink eventually dropping to zero (as it must, as the ecosystems equilibrate to the new concentration)—the biokinetic model sink actually continues to grow over time.

One commenter seems to have assumed that the airborne fraction (about 45%) means that CO_2 is accumulating at half the rate predicted by the IPCC. However, this airborne fraction is represented in the IPCC predictions already. For this commenter, and the commenter who claimed that the IPCC forecast for 2004 was 398 ppm, see the response in Volume 2 regarding the observations of CO_2 concentrations falling within the range of IPCC CO_2 concentration projections.

The TSD acknowledges the year-to-year variability in growth rate (see our response in Volume 2). A more in-depth discussion of this phenomenon can be found in the IPCC (Denman et al., 2007); Figure 7.4 demonstrates the historical variability graphically. Year-to-year variability averages out over several years and therefore does not matter for purposes of projections. Uncertainties in the carbon cycle on longer time periods are still important and are depicted in Figure 6.6 in the TSD, which shows a range of CO_2 concentrations for each emission scenario depending on different carbon cycle feedback assumptions.

Global recession can lead to a short-term decrease in carbon dioxide emissions, as highlighted by several commenters, though recent results (Le Quéré et al., 2009) suggest that this reduction was not drastic. Le Quéré et al. found that there was still an increase in global CO_2 emissions of 2% from 2007 to 2008, and they project that the 2009 decrease in emissions will only be a return to 2007 emission levels. Therefore, it seems that despite the recession, actual emissions will still be at the high end of projections in 2010. Emissions projections are also not designed to capture short-term fluctuations such as recessions.

We have reviewed the paper by Solomon et al. This paper shows that “the climate change that takes place due to increases in carbon dioxide concentration is largely irreversible for 1,000 years after emissions stop,**”** with possible consequences of this irreversible rise being “irreversible dry-season rainfall reductions in several regions comparable to those of the ‘dust bowl’ era and inexorable sea level rise.” The 1,000-year lifetime of certain CO_2 emissions is reflected in the TSD, which states that “the last 20% [of CO_2 concentrations] may remain in the atmosphere for thousands of years (Denman et al., 2007).” Additionally, we note the paper was referenced in Karl et al. (2009), which is one of the assessment reports summarized in the TSD.

With respect to the comment on dangerous carbon cycle feedbacks, we summarize information from the assessment literature on carbon cycle feedbacks explicitly in Section 6 (and Figure 6.6, which shows how uncertainty in the carbon cycle impacts future projections) of the TSD, and the TSD also references Field et al. (2007) that by the end of the 21st century, ecosystems in the northeast and southeast United States are projected to become carbon sources, while the western United States remains a carbon sink. The possibility of significant positive feedbacks resulting from the carbon cycle is clearly an important, though still uncertain area of science; the TSD also quotes Clark et al. (2008) on possible methane releases from wetlands and high latitude ecosystems:

> While the risk of catastrophic release of methane to the atmosphere in the next century appears very unlikely, it is very likely that climate change will accelerate the pace of persistent emissions from both hydrate sources and wetlands. Current models suggest that wetland emissions could double in the next century. However, since these models do not realistically represent all the processes thought to be relevant to future northern high-latitude CH_4 emissions, much larger (or smaller) increases cannot be discounted. Acceleration of persistent release from hydrate reservoirs is likely, but its magnitude is difficult to estimate.

Therefore, the emissions scenarios used within the assessment reports summarized in the TSD are credible projections for use in long-term climate modeling, even though there may be some deviations from observations (both high and low) of emissions or carbon sinks in the short term due to economic and natural variability. We note the reasons for concern regarding possible carbon feedbacks, but we conclude that the TSD reflects the current best estimates for CO_2 emissions and concentration growth in the absence of emissions mitigation policies, and that any attempt by the EPA to unilaterally revise the SRES or CCSP scenarios is neither necessary nor warranted.

Comment (4-38):

One commenter (1616.1) states that finite reserves of fossil fuels means that emissions will decrease soon. Another (3756.1) claims that models are over-predicting climate change because they do not take peak oil into account, citing studies by the Association for the Study of Peak Oil and the Energy Watch Group of Berlin for estimates of future production curves. Commenter 3756.1 also cites Monckton on a divergence between the growth rate of CO_2 concentration and IPCC projections as evidence that the rate of growth of CO_2 concentration is already slowing down.

Response (4-38):

While the availability of fossil fuel reserves limits the total possible emissions, many emissions projections account for these limited reserves (CCSP, 2007b). According to CCSP (2007b), the three models included in the study include “empirically based estimates of in-ground resources of oil, coal, and natural gas that might ultimately be available, along with a model of the costs of extraction.” These models also include a number of “unconventional” sources in their resource bases, such as tar sands, shale oil, coal-seam gas, and in the case of one model, coal-based synthetic oils. The models also include an assumption that some currently unused resources become economically exploitable over time due to advances in technology and higher prices. Fossil fuels remain the dominant energy source in the three reference scenarios considered in the CCSP report, but “all three reference scenarios include a transition from conventional oil production to some other sources of liquid fuels based primarily on other fossil sources.” Because these unconventional sources result in more CO_2 emissions per unit energy, this leads to a halt in the historic pattern of decline in carbon-to-energy ratios for the overall economy in these modeling scenarios.

The references by the commenter do not include any peer-reviewed sources or clear sources of data for the assertions of the timing of peak fossil fuel use. We recognize that there are still active debates about peak oil and the accuracy of the estimates of available resource by organizations such as the U.S. Geological Survey. The scenarios used by these modeling groups reflect the current best estimates of available reserves and have been reviewed in major assessment reports, and are therefore appropriate for inclusion in the TSD.

With regards to the commenter's argument regarding Monckton's estimates of IPCC projections, please see Volume 2.

Comment (4-39):

One commenter (3136.1) objects to the statement in the TSD that the SRES scenarios do not include the Kyoto Protocol; the commenter asserts that the statement implies that the protocol would have had a measurable effect. The commenter suggests the statement should be changed as follows: "…Kyoto Protocol, because that instrument would have done nothing measurable about global warming on the century time-scale." Another commenter (3722) submits Wigley et al. (1998) to show that implementing Kyoto would delay sea level rise by only four years, stating that this is a "negligible impact."

One commenter (3394.1) notes that several analyzed emissions projections do not include new mitigation policies and that only the CCSP (but not SRES) scenarios include the Kyoto Protocol, and that the TSD does not review speculative responses to climate change. The commenter objects that it is "indefensible to exclude existing and virtually certain mitigation measures from that assessment," and that exclusion of foreign, federal, state, and local reductions that are planned or expected is "arbitrary," a lack of balance, and just designed to evade difficult questions, given that the TSD is "willing to speculate as to large GHG emission increases."

Response (4-39):

We reviewed the discussion of Kyoto Protocol in Section 6 of the TSD and disagree with the commenter that our statements need to be revised. The statement in the TSD regarding the IPCC scenarios is accurate. The IPCC *Special Report on Emissions Scenarios* (SRES) was published in 2000 (IPCC, 2000), before the Kyoto Protocol entered into force, and, as stated in the TSD, "the IPCC SRES scenarios do not explicitly account for implementation of the Kyoto Protocol." The CCSP scenarios, in contrast, do include the Kyoto Protocol's 2008 to 2012 targets, as well as the 2012 U.S. intensity target, but not targets beyond that. This is also clearly summarized in the TSD (see Box 6.2: CCSP (2007b) Reference Case Emission Scenarios from Synthesis and Assessment Product 2.1.) Therefore, the TSD accurately summarizes the treatment of these policies in both the IPCC and CCSP scenarios, and there is no need to include the suggested revision by the commenter.

Wigley et al. (1998) was submitted with regard to a specific mitigation measure, the Kyoto Protocol. Wigley et al. show a 7% reduction in the amount of warming that is projected to occur by 2100 resulting from constraining Annex B nations to meet their Kyoto targets for the entire century, but allowing emissions to grow unconstrained in all non-Annex B nations. This scenario also results in a 4 to 7% reduction in sea level rise. Another similar study, Reilly et al. (1999) shows a 17% reduction in warming and sea level rise by 2100. The difference between the simulations relates to differences in emissions projections (including relative growth in emissions between Annex B and non-Annex B nations) and differences in underlying climate parameters such as climate sensitivity and ocean heat uptake. In either case, we find that a 4% to 17% reductions in these impacts are not negligible.

See the Finding, Section III.C, Adaptation and Mitigation, for our response to comments on the treatment of adaptation and mitigation in the finding.

Comment (4-40):
A commenter (3394.1) notes that the results of the storyline/scenario approach are not projections, and that they have no probabilities associated with them. The commenter asserts that this is insufficient for purposes of an endangerment analysis and states "EPA would have to describe the relative probabilities of various future emissions projections before it could determine that the projected effects of those emissions endanger public health or welfare" because the endangerment finding itself states that the Administrator bases her finding on the "current *and projected*" (emphasis added by commenter) levels of the six GHGs. Another commenter (3679.1) objects that the word "projected" was used 285 times despite the discussion of storyline/scenario vs. projection. However, another commenter (3722) objects that the IPCC relies on "projections" rather than "predictions."

Response (4-40):
While neither IPCC nor CCSP put probabilities on their various baseline scenarios, all of the scenarios are designed to be "meaningful and plausible" (CCSP, 2007b). The fact that every major assessment exercise (IPCC SRES, CCSP 2.1a, and the more recent Energy Modeling Forum-21 exercise, all referenced in the TSD) developed scenarios that fall within the same range of emissions is indicative that these do represent consistent estimates of future emissions. See also response number 4-37.

The language of *Mass v. EPA* on uncertainty was that if "the scientific uncertainty is so profound that it precludes EPA from making a reasoned judgment, it must say so. The statutory question is whether sufficient information exists for it to make an endangerment finding." There is no requirement for being able to place exact probabilities on certain outcomes. As the Finding states, because scientific knowledge is constantly evolving, the Administrator may be called upon to make decisions while recognizing the uncertainties and limitations of the data or information available, as risks to public health or welfare may involve the frontiers of scientific or medical knowledge.

While not included in any assessment exercise, and therefore not included in the TSD, we also note that some studies (e.g., Webster et al., 2002) have conducted more formal uncertainty analyses to produce a range of emissions projections, and the range of emissions was if anything at the high end of the IPCC range. Therefore, there are no reasons to indicate that any exercise by EPA to associate probabilities with future scenarios would provide any substantially different information to the Administrator than already provided by the existing scenarios represented in the assessment literature.

We also note that the April 2009 TSD incorrectly stated that "storylines are not projections" and we have replaced this with "storylines are not predictions" in the final TSD.

Comment (4-41):
A commenter (3747.1) argues that "[f]undamental to EPA's Proposal is the Agency's use of and reliance on IPCC modeling — "storylines" to construct and predict future scenarios. However, the authors of the IPCC reports explicitly note that these storylines are not predictors or forecasters of future economic, population, or related trajectories in the U.S. or elsewhere. Rather, as the phrase suggests, IPCC's storylines are illustrative tools, not predictions. For example, none of the IPCC storylines consider periodic economic cycles that are directly relevant to findings of impacts." The commenter argues that EPA did not adhere to the information quality guidelines under the IQA because the TSD uses IPCC emission scenario storyline as predictive models.

Response (4-41):
The use of the IPCC, SRES, and Energy Modeling Forum projections for examining possible future scenarios is well-accepted and routine in the scientific community. Please see other responses in this Volume on the quality of these scenarios. We considered a wide range of scenarios, consistent with the approaches taken by the IPCC and CCSP. There is no indication in the EPA *Guidelines for Ensuring and Maximizing the Quality, Objectivity, Utility and Integrity of Information Disseminated by the Environmental Protection Agency* that there is a requirement that the Administrator rely only on “predictions.” According to the IPCC terminology, a “prediction” is the “most likely” projection. While no one scenario or projection provided has been branded “most likely,” the range of scenarios and projections does allow assessment of likely futures in the absence of mitigation policies. As noted in a previous response, papers have been published that generate scenarios probabilistically, and the median estimate of one example, Webster et al. (2002) falls within the range of the SRES, CCSP, and Energy Modeling Forum scenarios. For responses on the specific issue of considering periodic economic cycles, see response 4-37: just as weather prediction is not necessary for climate prediction, short term economic cycle prediction is not necessary for long term emissions projections. We conclude that the approach followed in the assessment literature, and summarized in the TSD, is accurate and sound, given the range of socio-economic factors (i.e., drivers of GHG emissions) that could play out over time. Further, we find the TSD’s discussion of the emissions scenario storylines and how they were used to be appropriate and consistent with how the assessment literature intended them to be used, and therefore consistent with EPA’s guidelines.

Comment (4-42):
Commenter (3136.1) inquires as to why the MiniCAM projection “is clearly just a straight line,” and that this is important information to include in the TSD because it means complicated emission modeling is unnecessary.

Response (4-42):
The commenter is apparently referring to Figure 6.2 in Section 6 of the TSD, and we disagree that the MiniCAM projection is “clearly just a straight line.” Although global CO_2 emissions in the MiniCAM reference scenario appear linear, this does not imply that complicated emission modeling is unnecessary. This result just happens to be the outcome of the interactions between a particular set of emission drivers selected for the reference case. More information on the modeling approach and underlying assumptions for this (and other) scenarios can be found in CCSP (2007b). We summarized the range of emission projections available in the assessment literature in the TSD because these scenarios provide important insights into the drivers that lead to various future emission pathways.

Comment (4-43):
Several commenters (e.g., 3596.1, 3596.2) request that EPA discuss the recent stabilization of methane (CH_4) concentrations. A commenter (3596.3) argues that this stabilization is in opposition to IPCC projections and means future projections of climate change are “way off base,” and discusses the possibility of decreasing CH_4 concentrations as “a logical progression” in the rate of CH_4 changes over time.

Response (4-43):

Please refer to Volume 2 for EPA's detailed response on the stabilization of CH_4 concentrations over the last several years. Here we confine our response to the issue of methane projections.

We disagree with the commenter that the stabilization in methane concentrations in the first half of this decade implies that CH_4 concentrations will not grow in the future. As noted in the response in Volume 2, more recent data indicate that CH_4 concentrations are growing again; Rigby et al. (2008) found a small increase in concentrations in 2007, which rose again in 2008. Further, examination of the underlying drivers of anthropogenic CH_4 emissions indicates that future growth is certainly possible. Methane is emitted by multiple activities in the energy, waste, and agricultural sectors, and emissions from all of these sources could increase because of population and economic growth. Moreover, some of the major natural sources of CH_4 emissions are highly dependent on temperature, and there is already evidence that methane emissions from Arctic permafrost are increasing. The TSD refers to Clark et al. (2008), which finds that "it is very likely that climate change will accelerate the pace of persistent emissions from both hydrate sources and wetlands."

Thus, we find that the emissions scenarios summarized in the TSD reflect the best estimates in the literature and are a reasonable range of possible emission futures for methane, reflecting the uncertainty for future emissions. Two of the CCSP models project continued growth in CH_4 emissions throughout the 21^{st} century "as a consequence of the growth of CH_4-producing activities such as ruminant livestock herds, natural gas use, and landfills." However, the third emission scenario, despite expansion of these activities, assumes that emission control technologies will be deployed in response to local environmental regulations and the increasing value to capturing CH_4 as natural gas prices increase. For this reason, CH_4 emissions peak and decline in this third scenario. The IPCC SRES scenarios also cover a significant range of possible CH_4 future emission pathways. Therefore we find that the assessment literature provides an appropriate range of scenarios given the uncertainty involved.

For all these reasons, we disagree that the recent stabilization of CH_4 concentrations means that any future projections showing growth in CH_4 concentrations are "way off base."

Comment (4-44):
A commenter (4632) objects to the emission scenarios produced by the IPCC because of critiques by Castles and Henderson (2003a, 2003b, 2005) that highlight the use of market exchange rates (not purchasing power parity) and the implausible assumption that poor nations will equalize per capita emissions with rich nations.

Response (4-44):
Both IPCC (2007) and CCSP (2007b) address the issue of using market exchange rate (MER) versus purchasing power parity (PPP) approaches in determining future gross domestic product (GDP) growth rates, in response to the critiques by Castles and Henderson (2003a, 2003b, 2005). The IPCC (Fisher et al., 2007) states the following:

> In the debate on the use of exchange rates, market exchange rates (MER) or purchasing power parities (PPP), evidence from the limited number of new PPP-based studies indicates that the choice of metric for gross domestic product (GDP), MER or PPP, does not appreciably affect the projected emissions, when metrics are used consistently. The differences, if any, are small compared to the uncertainties caused by assumptions on other parameters, e.g. technological change (high agreement, much evidence).

The IPCC adds the caveat that, unlike emissions, the numerical expression of GDP does depend on conversion methods. CCSP (2007b) notes that while MER is used to set the base year of the models in that assessment, “growth prospects and other parameters for the world’s economies were assessed relative to their own historical performance” in order to avoid potential issues arising from interactions between the MER/PPP issue and assumptions regarding convergence.

While we find that both the IPCC and CCSP approaches yield credible estimates of future emissions that have been well supported by the literature, the robustness of conclusions based on emission projections developed through different means adds even more confidence that the TSD is appropriately summarizing the best existing science.

Comment (4-45):
One commenter (2057) argues that the assumption that GHG levels will constantly increase is currently speculation. The commenter then asks if the EPA would propose a minimum CO_2 level if future advances in carbon capture or energy usage lead to decreases in CO_2.

Response (4-45):
We have reviewed the TSD and assessment literature in light of this comment and find that the projected increase in GHG emissions, as expressed in the assessment literature and summarized in the TSD, is not speculative, and the commenter presents no evidence to suggest otherwise. Rather, we find that the range of projections have been well vetted, peer reviewed, and fully assessed within the scientific literature. The endangerment finding focuses on the risks if mitigation does not happen: it does not address the change in risk due to mitigation. Thus, the question of a minimum CO_2 level is not pertinent to this finding.

4.3 Future Projections of Temperature

Comment (4-46):
A number of commenters (e.g., 0700.1, 1499.1, 2898.1, 3136.1, 3596.2) argue that the equilibrium climate sensitivity (ECS) is, or is likely to be, lower than the most likely value of about 3˚C reported in the IPCC AR4. [2]

One commenter (3136.1) argues that “[t]he secular climate history calls for a re-assessment of the likely equilibrium sensitivity to a carbon dioxide doubling.” According to the commenter, observed temperatures patterns point to “a likely value *below* 3.0˚C.” The commenter states that evidence suggests that much of the early 20th-century warming was largely driven by natural variations of the sun’s output. If this is true, the commenter writes, then only the warming from 1976–1998 is ascribable largely to the CO_2, suggesting that ECS is around 2.1˚C.

One commenter (3596.2) makes a similar argument regarding natural variability and additionally cites several studies that suggest climate sensitivity is lower than the most likely value reported by IPCC. The commenter notes that Chylek and Lohmann (2008) derived a 95% confidence range of 1.3–2.3˚C for the climate sensitivity for a doubling of CO_2 based on ice core records from the Last Glacial Maximum. The commenter states that Wyant et al. (2006a) finds that climate sensitivity declines to values at or below the low end of the IPCC range when better cloud processes are incorporated into climate models and that “[s]imilar conclusions indicating that observed (rather than modeled) cloud cover changes (and their impacts on atmospheric moisture content) suggest a lower climate sensitivity than IPCC estimates have been made by Spencer et al. (2007), Spencer and Braswell (2008), and Paltridge et al. (2009).” Finally,

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[2] In IPCC reports, equilibrium climate sensitivity refers to the equilibrium change in the annual mean global surface temperature following a doubling of the atmospheric equivalent CO_2 concentration.

the commenter argues that observational evidence supporting a low climate sensitivity value is included in work by Swanson and Tsonis (2009) and Michaels et al. (2009).

Another commenter (2898.1) argues that research conducted by Richard Lindzen and colleagues (Lindzen, 2009) using data from the Earth Radiation Budget Experiments and National Centers for Environmental Prediction shows a strong negative feedback and indicates that the sensitivity of the actual climate system "is narrowly constrained to about 0.5˚C." This commenter references several studies that the commenter says call into question the IPCC analysis of ECS and help explain the findings of Lindzen and colleagues. Specifically, the commenter states that work by Dr. William Gray (2009) indicates that observations of upper tropospheric water vapor over the last three to four decades "show that upper tropospheric water vapor appears to undergo a small decrease while outgoing longwave radiation undergoes a small decrease" and that observations indicate that the specific and relative humidity of the middle and upper troposphere have been decreasing over the last four to five decades. The commenter additionally states that Spencer et al. (2007) found a strong negative cirrus cloud feedback mechanism in the tropical troposphere and that a study by Spencer and Braswell (2008) suggests that increases in sea-surface temperature could be an effect of natural cloud variations. The commenter quotes Dr. Spencer as stating the following on his Web site: "[W]hen the effect of clouds-causing-temperature-change is accounted for, cloud feedbacks in the real climate system are strongly negative. In fact, the resulting net negative feedback was so strong that, if it exists on the long time scales associated with global warming, it would result in only 0.6 deg. C of warming by late in this century." In addition, the commenter states that "an appeal to the authority of the IPCC would not suffice as a rebuttal to Lindzen, because the issue in dispute is precisely whether IPCC sensitivity assessments are consistent with the actual data."

Response (4-46):

In light of these comments, we have reexamined the relevant assessment literature, the studies submitted by commenters, and other recent peer-reviewed literature on ECS. On the basis of this review, we have determined that the TSD accurately summarizes the findings of the scientific community on the subject of ECS, as it is expressed in the assessment literature. We have reviewed the studies and presentations referenced by commenters to support claims that the true ECS value is likely to be substantially below 3˚C. On the basis of this review, we have determined that important aspects of a number of these studies are inconsistent with much of the published literature and that the studies do not provide a sufficient basis on which to conclude that the IPCC conclusions are flawed.

As noted in the TSD, IPCC (Meehl et al., 2007) has concluded that ECS is very likely greater than 1.5˚C and likely to lie in the range of 2˚C to 4.5˚C, with a most likely value of about 3˚C. Although ECS values somewhat lower than the most likely value of about 3˚C reported in the IPCC AR4 are plausible, we disagree that ECS is "likely"[3] to be lower than 3˚C, as several commenters stated or implied. The ECS value of 2.1˚C proposed by several commenters (3136.1, 3596.2) falls within the range of likely ECS values assessed by IPCC; however, as discussed further below, the commenters do not compellingly demonstrate that an ECS value of less than 3˚C is any more likely than an ECS value of 3˚C or greater.

As noted by IPCC, studies to constrain climate sensitivity have used several approaches, including the use of AOGCMs, examination of the transient evolution of temperature (surface, upper air and ocean) over the last 150 years, examination of the rapid response of the global climate system to changes in the forcing caused by volcanic eruptions, and estimates based on palaeoclimate studies (Solomon et al., 2007). These independent lines of evidence indicate similar most likely values and ranges for ECS and collectively form the basis for the key IPCC conclusions summarized in TSD. An extensive discussion of

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[3] According to IPCC terminology, "likely" conveys a 66 to 90% probability of occurrence. See Box 1.2 of the final TSD for a full description of IPCC's uncertainty terms.

each of these lines of evidence is found in Chapter 10 of the IPCC AR4 (Meehl et al., 2007), and they are briefly described here.

One main category of methods examines climate sensitivity in GCMs. Meehl et al. (2007) describe a number of GCM-based studies that indicate similar most likely values and ranges for ECS. Three probability density functions (PDFs) were obtained by comparing different variables of the simulated present-day climatology and variability against observations in a perturbed physics ensemble. ECS was found to be most likely around 3.2˚C and very unlikely to be below about 2˚C, with the upper bound sensitive to how model parameters are sampled and to the method used to compare with observations.

A second category of methods uses observed changes in climate to constrain ECS. Some studies (e.g., Forest et al., 2006) have used instrumental records of surface, ocean, and atmospheric temperature changes to constrain climate sensitivity, while others (e.g., Wigley et al., 2005) analyze the forcing and response to major volcanic eruptions. A summary of PDFs of climate sensitivity from these methods, proxy data over the last millennium, estimates of radiative forcing, satellite data or subsets thereof is shown in Box 10.2 in Meehl et al. (2007). According to IPCC (Hegerl et al., 2007), "Results from studies of observed climate change and the consistency of estimates from different time periods indicate that ECS is very likely larger than 1.5˚C with a most likely value between 2˚C and 3˚C." Hegerl et al. (2007) report that this supports the overall assessment based on modeling and observational studies that the most likely value for ECS is approximately 3˚C. Hegerl et al. (2007) additionally note that while considering all available evidence on ECS together provides a stronger constraint than individual lines of evidence, remaining uncertainties that are not accounted for in individual estimates and possible dependencies between individual lines of evidence make the upper 95% limit of ECS uncertain at present.

One reason a high sensitivity is difficult to rule out is that a high aerosol forcing could nearly cancel GHG forcing over the 20th century (Hegerl et al., 2007), and a large ocean uptake can delay the emergence of a warming signal. As noted in the TSD, an ECS higher than 4.5˚C cannot be ruled out, though agreement for higher values is generally worse compared with values in the 2–4.5˚C range.

We have reviewed the studies and presentations referenced by several commenters (2898.1, 3136.1, 3596.2) to support their claims that the true ECS value is likely to be substantially below 3˚C. In the following paragraphs, we address the main issues these studies and presentations raise with specific regard to ECS.

Regarding commenters' claims about observations of middle- and upper-level tropospheric water vapor over the last three to four decades based on Gray (2009): As noted in Volume 3, Section 3.2.1: Other Substances with Radiative Forcing Effects, the analysis in the IPCC AR4 (Trenberth et al., 2007) stated: "Due to instrumental limitations, long-term changes in water vapour in the upper troposphere are difficult to assess," but nonetheless concluded, "To summarise, the available data do not indicate a detectable trend in upper-tropospheric relative humidity. However, there is now evidence for global increases in upper-tropospheric specific humidity over the past two decades, which is consistent with the observed increases in tropospheric temperatures and the absence of any change in relative humidity."

Regarding commenters' arguments based on the work of Lindzen and Gray that the water vapor feedback is strongly negative: As discussed in Volume 3, the hypothesis that increased CO_2 forcing will lead to a counterbalancing decrease in water vapor is highly speculative and is not supported by the vast body of scientific literature. Randall et al. (2007) report that observations provide ample evidence of regional-scale increases and decreases in tropical upper-tropospheric relative humidity in response to changes in convention; however, they also note that these changes provide little insight into large-scale thermodynamic relationships important for the water vapor feedback unless considered over entire circulation systems. Randall et al. additionally note that broadly similar changes are found in a range of models of different complexity and scope, and that indirect evidence for model water vapor feedback strength also comes from experiments that show that suppressing humidity variation from the radiation code in an AOGCM produces unrealistically low interannual variability. According to Randall et al. (2007): "Together, upper-tropospheric observational and modeling evidence provide strong support for a combined water vapour/lapse feedback of around the strength found in GCMs [approximately 1 W m^{-2} per degree global temperature increase]."

Regarding Dr. Spencer's findings about a negative cirrus cloud feedback mechanism and the clouds-causing-temperature-change hypothesis: The IPCC acknowledges that, "there is some consistency between ISCCP [International Satellite Cloud Climatology Project], ERBS [Earth Radiation Budget Satellite], SAGE II [Stratospheric Aerosol and Gas Experiment] and surface observations of a reduction in high cloud cover during the 1990s relative to the 1980s." Trenberth et al. (2007) note, however, that the variability in total surface-observed total cloud cover is not consistent with ISCCP; that there are substantial uncertainties in decadal trends in all data sets; and that "at present there is no clear consensus on changes in total cloudiness over decadal time scales." Randall et al. (2007) note that cloud feedbacks remain the largest source of uncertainty in assessing ECS but that progress has been made in the identification of the cloud types, the dynamical regimes and regions of the globe responsible for the large spread of cloud feedback estimates among current models.

On the basis of the key assessment literature conclusions described above and the numerous peer-reviewed studies that form the basis for these conclusions, we find that Lindzen's (2009) assertion that climate sensitivity "is narrowly constrained to about 0.5C" is inconsistent with the vast majority of scientific evidence on ECS. Our review of the cited presentation revealed that the methods he used to conclude climate sensitivity is constrained to about 0.5˚C are not well described and that the analysis appears to be based on an examination of only 16 years of data from two particular datasets (the monthly record of sea surface temperatures from the National Centers for Environmental Prediction and fluctuations in radiative flux from the Earth Radiation Budget Experiment). Commenter 2898.1 is in this instance attempting to use a part of a single presentation that did not undergo peer review and is based on research with significant temporal and spatial limitations to call into question assessment conclusions that are based on a large variety of peer-reviewed studies, many of which examine data over longer time periods.

We have noted commenter 2898.1's view that "an appeal to the authority of the IPCC would not suffice as a rebuttal to Lindzen, because the issue in dispute is precisely whether IPCC sensitivity assessments are consistent with the actual data." We strongly disagree, however, that the IPCC conclusions about climate sensitivity are somehow inappropriate for purposes of a robust response to the comment. The IPCC assessment of climate sensitivity rests on several independent lines of evidence and is based on a rigorous assessment by numerous scientists of scores of peer-reviewed papers on the topic. Further, several very recent peer-reviewed papers on ECS reach conclusions that are highly consistent with the IPCC assessment, as described later in this response. Therefore, it is reasonable to continue to rely on the IPCC conclusions despite the existence of one presentation that when compared to the scope and breadth of the assessment literature is narrow and incomplete in its consideration of the issues.

As previously noted, one commenter (3596.2) referenced a number of peer-reviewed studies that the commenter states support a low ECS value, including: Paltridge et al. (2009), Spencer and Braswell (2008); Wyant et al. (2006a), Chylek and Lohmann (2008) and Swanson and Tsonis (2009). We have reviewed these studies and find that they do not provide a sufficient basis on which to conclude that that an ECS value of less than 3˚C is any more likely than an ECS value of 3˚C or greater.

Wyant et al. (2006a) calculated climate sensitivity from simulations with a SST that was increased by 2 degrees K uniformly. They also used the same method to calculate climate sensitivity from two different versions of the parent model. Although the model with the new cloud resolving superparameterizations[4] had a 20% smaller climate sensitivity than one version of the parent model, it showed no decrease at all in climate sensitivity compared to the other version of the parent model. Therefore, it is not clear whether the reduction of climate sensitivity resulting from including cloud resolving superparameterizations is a robust finding, even for one model type. Additionally, the absolute value of climate sensitivity should not be compared to other estimates, as it is clear from Wyant et al. (2009) and Wyant et al. (2006b) that climate sensitivity calculated from a doubled-CO_2 experiment usually has less negative cloud feedback than climate sensitivity calculated from a +2K SST experiment, as in Wyant et al. (2006a). Additionally, there is some evidence that the more realistic the ocean, the larger the climate sensitivity (Danabasoglu and Gent, 2008); though the effect from moving from a slab ocean to a full-depth ocean was small, it is unclear what this effect would be for a more dramatic move from a constant SST (as in Wyant et al., 2006a) to a slab ocean.

It is important to note that the IPCC conclusions about ECS are not based solely or even primarily on calculations from climate model physics; rather, historical data from a number of sources and time periods is often used to constrain the ECS parameter (Hegerl et al., 2007). Therefore, while this is interesting work that does suggest that climate sensitivity, at least in one specific model, may decrease when more realistic cloud resolving routines are included, there is reasonable evidence to suggest that this effect may be small and in any case does not apply to many other methods of estimating climate sensitivities.

Paltridge et al. (2009) examine radiosonde-derived humidity data on tropospheric humidity from the National Centers for Environmental Prediction and find that the face-value 35-year trend in zonal-average annual-average specific humidity is significantly negative at all altitudes above 850 hectopascals (hPa) in the tropics and southern mid-latitudes and at altitudes above 600 hPA in the northern mid-latitudes. The authors do not quantitatively assess climate sensitivity (which is not the main focus of their study), but do offer several important qualifications to their conclusions on tropospheric humidity. For example, the authors note that radiosonde-derived data must be treated with “great caution” and that their findings about upper-level negative trends in specific humidity are inconsistent with climate-model calculations and largely inconsistent with satellite data. By ignoring these nuances and the range of studies cited in IPCC (Trenberth et al., 2007) that indicate radiosonde trends are suspect due to the poor quality of, and changes over time in, the humidity sensors, the commenter exaggerates the significance of the study vis-à-vis the large body of literature that underlies the IPCC conclusions, much of which focuses more directly on ECS.

Chylek and Lohmann use the temperature, CO_2, CH_4, and dust concentration record from the Vostok ice core to deduce the aerosol radiative forcing during the Last Glacial Maximum (LGM) to Holocene transition and the climate sensitivity, and state that their results suggest a 95% likelihood of warming between 1.3 and 2.3˚K because of doubling of atmospheric concentration of CO_2. In our review of the Chylek and Lohmann study, we determined that two peer-reviewed comments on the paper found serious issues with its methodology. Ganapolski and Deimling (2008) found that Chylek and Lohmann: a) did not properly account for the uncertainties of estimating climate sensitivity, and b) that the role of glacial aerosols on contributing to the LGM-Holocene cooling must have been much smaller than what Chylek and Lohmann inferred. Among other things, the paper notes that Chylek and Lohmann consider an Antarctic Last Glacial Maximum (LGM) cooling of 10.2˚C without attaching an uncertainty estimate to the value and that modeling results are not consistent with Chylek and Lohmann’s assumption that LGM cooling had not exceeded 5˚C.

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[4] Superparameterization (also known as Multi-Scale Modeling Framework) uses a cloud-resolving model implemented at each GCM grid column. According to Wyant et al. (2006a), the use of cloud-resolving models permits explicit simulation of smaller-scale vertical convective motions associated with clouds and allows radiation to be calculated on a finer scale.

Hargreaves and Annan (2008) found similar flaws, and also state that Chylek and Lohmann's analysis is based on the selection of local extrema in time series, which shows high temporal variability, and that the data points they used are not temporally coincident. Hargreaves and Annan (2008) conclude as follows: "When the noise of short-term natural variability is reduced by temporal averaging, the results come into line with previous analyses of these and similar data (e.g. Hansen et al., 1993)."

Though commenter 3596.2 states that Swanson and Tsonis (2009) present observational evidence supporting a low climate sensitivity, our examination of this paper revealed that the authors' own assessment of their results does not support the commenter's statement. Swanson and Tsonis (2009) state the following: "Finally, it is vital to note that there is no comfort to be gained by having a climate with a significant degree of internal variability, even if it results in a near-term cessation of global warming. It is straightforward to argue that a climate with significant internal variability is a climate that is very sensitive to applied anthropogenic radiative anomalies [cf. Roe, 2009]. If the role of internal variability in the climate system is as large as this analysis would seem to suggest, warming over the 21st century may well be larger than that predicted by the current generation of models, given the propensity of these models to underestimate climate internal variability [Kravtsov and Spannagle, 2008]." Thus, we conclude that this paper is more supportive of a high ECS value than it is a low one.

Several recent peer-reviewed studies have reached conclusions consistent with the IPCC findings regarding ECS. For example, Forest et al. (2008) compare observed changes in surface, upper-air, and deep-ocean temperature changes against simulations of 20th-century climate in which climate model parameters were systemically varied and estimate the effective climate sensitivity for 11 of the IPCC AR4 AOGCMs. They derive an estimated 90% range of 2–5˚K, with no corresponding upper bound on ECS, which is largely consistent with the PDFs for ECS presented in Hegerl et al. (2007). They also carried out probabilistic projections of 21st century warming using the SRES scenarios from AR4. Their results are reported in a separate paper and "imply a significantly stronger warming in the 21st century than the AR4 model ensemble."

Royer et al. (2007) estimated the long-term ECS by modeling CO_2 concentrations over the past 420 million years and comparing their results with a proxy record. They found that a long-term radiative forcing by CO_2 of less than 1.5˚C is highly unlikely because lower temperatures would require unreasonably high levels of atmospheric CO_2 to maintain the necessary feedback effect.[5] The authors find that the best fit between the standard version of the model and proxies for atmospheric CO_2 over the Phanerozoic occurs for a temperature delta of 2.8˚C and that a temperature delta of at least 1.5˚C "has been a robust feature of the Earth's climate system over the past 420 Myr [million years], regardless of temporal scaling."

Hansen et al. (2008) note that the ECS studies described by the IPCC generally do not include slow-climate feedbacks, such as loss of ice and spread of flora over the vast high-latitude over the high-latitude land area in the Northern Hemisphere, or carbon cycle and CH_4 emission responses to temperature changes. They use palaeoclimate data to show that the long-term (i.e., millennial-scale) sensitivity could be in the range of 6˚C; however, it is important to note that climate sensitivity estimates based on palaeoclimate must be viewed with a degree of caution because of data uncertainties and the fact that climate sensitivity changes with the mean climate state and the nature of the forcing (Hegerl et al., 2007).

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[5] Royer et al. (2007) note that a critical factor in their approach is the effect of atmospheric CO_2 level on the rate of CO_2 uptake by weathering of calcium and magnesium silicate minerals; they report that a rise in temperature, accompanying a rise in CO_2, increases the rate of silicate weathering, which in turn accelerates atmospheric CO_2 consumption, forming a negative feedback loop.

Please see Volume 3 for EPA’s responses regarding natural variability and attribution of the observed warming and Section 4.1 of this volume for EPA’s responses to comments on the validity of model-based climatic projections. Comments of specific relevance to the role of positive feedbacks in the model-based studies that provide one source of climate sensitivity estimates are covered in response number 4-17 of this volume. The Spencer and Braswell (2008) study cited by a number of commenters with regard to feedbacks and climate sensitivity is also addressed in this response.

In summary, we have reviewed the relevant assessment literature, the studies submitted by commenters, and other recent peer-reviewed literature on ECS in light of the comments received on this topic. Our review indicates that the studies referenced by commenters to support claims that the true ECS value is likely to be substantially below 3˚C are inconsistent with the large majority of peer-reviewed studies on ECS underlying the IPCC conclusions, and that the discussion of ECS in the TSD accurately reflects the current state of scientific knowledge on ECS as reflected in the assessment literature. Thus, the treatment of ECS in the TSD is accurate and sound.

Comment (4-47):

Numerous commenters (e.g., 0700.1, 2853.1, 2898.1, 3215.1, 3217.1, 3224_CP, 3394.1, 3411.2, 3446.1, 3729.1, 3747.1, 4003, 4509, 9733, 10499) argue that the temperature projections described in the TSD are not in accordance with the observed temperature record. Some of these commenters argue that global climate models are overestimating global temperature increases, particularly since 2000, compared with satellite observational data (i.e., Remote Sensing Systems [RSS], University of Alabama in Huntsville [UAH]). Commenters state that recent satellite data indicate a leveling or negative trend in global temperatures above the surface in this decade, despite an increase in CO_2 emissions and concentrations during that same period. Commenters claim that declines occur both in surface temperatures and in atmospheric temperatures measured from satellites. One commenter (3217.1) states: “Before EPA approvingly cites the projections from global climate models, the projections should agree with actual temperature data. Currently the global climate models over-project global temperatures. The lack of temperature increase calls into question the skill of global climate models and whether they bear a relation to the real world.” Additionally, several commenters refer to Keenlyside et al. (2008) and Swanson and Tsonis (2009) as evidence that some scientists believe that there may be multidecadal pauses in warming.

Response (4-47):

Please refer to Volume 2 for EPA’s complete responses to comment on observed temperature. Here we confine our discussion of the observed temperature record to its relationship with future temperature projections.

We have reviewed relevant literature in light of these comments, and we have determined that the temperature projections described in the TSD are in accord with the observed temperature record. It is true, as several commenters (e.g., 3136.1) point out, that the rate of observed temperature change in the last decade is, for the HadCRUT dataset, less than the projected warming of about 0.2˚C per decade reported in the TSD for a range of SRES emission scenarios; according to Knight et al. (2009), the least squares trend for January 1999 to December 2008 calculated from the HadCRUT3 dataset is +0.07±0.07˚C. However, as noted in the TSD (see Box 4.1: Updated Global Surface Temperature Trends Through 2008), the National Oceanic and Atmospheric Administration’s (NOAA’s) and National Aeronautics and Space Administration’s (NASA’s) trends do not show the same marked slowdown for the 1999–2008 period. The NOAA trend was ~0.12 C per decade while the NASA trend was ~0.19 C per decade.

As noted in the TSD, all three data sets show similar trends over the long term. However, as in this case, short-term temperature trends can differ more substantially between data sets than long-term ones, owing to the lower number of data points and because differences arise from the diversity of spatial averaging techniques and from the treatment of gaps in the data (Trenberth et al., 2007). Scientific best practice dictates that whenever possible more than one independent data set should be used to analyze both long and short term trends in a given variable. For this reason, the TSD includes data and calculations from not only the HadCRUT but also NOAA and NASA datasets. As discussed in Volume 2, short-term data sets do not appropriately inform long-term climate change trend questions and the relatively flat trend in some surface and satellite datasets over the last seven to 10 years does not fundamentally alter the longer term warming signal.

Commenters' arguments that the period of relatively flat warming of the past seven to 10 years apparent in certain datasets implies that climate projections are fundamentally flawed do not take into account the role of internal variability. As discussed in the assessment literature and elsewhere in this Response to Comments document (see Volume 3), inter-annual and inter-decadal temperatures are substantially influenced by natural modes of internal climate variability (e.g., ENSO) that can either amplify or mask the long-term warning signal.

Several studies have assessed temperature trends over the last decade in relation to these modes of internal variability, and these studies have determined that recent temperatures are not inconsistent with the IPCC temperature projections.

As discussed in response 4-26 in this volume, Knight et al. (2009) assess model ensembles with different modifications to the physical parameters of the model within known uncertainties and find that recent temperature trends are well within model variability. Knight et al. conclude as follows: "Given the likelihood that internal variability contributed to the slowing of global temperature rise in the last decade, we expect that warming will resume in the next few years, consistent with predictions from near-term climate forecasts."

Easterling and Wehner (2009) find similar results. The study fits least-squares trends to running 10-year periods in the global surface air temperature time series for the observed record, an ensemble of long control simulations, an ensemble of 20th-century simulations, and an ensemble of simulations forced with an A2 forcing from the SRES scenarios. Predictably, the control runs are symmetrical around a zero trend. For the 20th-century simulations, there is a shift to more positive values but still a significant chance of a negative decadal trend and, notably, a similar distribution to the observed record. For the 21st century simulations, there is still about a 5% chance of a negative decadal trend even without volcanic eruptions. As Easterling and Wehner (2009) note, the results demonstrate that "it is reasonable to expect that the natural variability of the real climate system can and likely will produce multi-year periods of sustained 'cooling' or at least periods with no real trend even in the presence of long-term anthropogenic forced warming."

On the basis of the above evidence and the analysis of the relationship between internal variability and temperature contained in the assessment literature [see, e.g., Trenberth et al. (2007) and Karl et al. (2006)], we conclude that the observed temperature record is neither inconsistent with the temperature projections discussed in the TSD nor calls into question the validity of climate models. To place recent temperature trends in context and clarify their relationship to long-term climate change, EPA has added the following text to Section 6(b) of the TSD:

> According to the NOAA report *The State of the Climate in 2008* (Peterson and Baringer, 2009), the recent slowdown in observed climate warming (see Box 4.1) in some datasets has led some to question climate predictions of substantial 21st century warming. The

> study finds that climate models possess internal mechanisms of variability capable of reproducing the current slowdown in global temperature rise. It concludes that "[g]iven the likelihood that internal variability contributed to the slowing of global temperature rise in the last decade, we expect that warming will resume in the next few years, consistent with predictions from near-term climate forecasts."

We have reviewed the Keenlyside et al. (2008) and Swanson and Tsonis (2009) papers referenced by commenters. The Swanson and Tsonis (2009) paper is addressed in several locations within this Response to Comments document including response number 4-46 within this volume. The significance of the Keenlyside et al. (2008) paper vis-à-vis climate models is covered in Section 4.1 of this volume.

Please see Volume 2 for EPA's response to comments specific to the relationship between satellite records of lower atmosphere temperature and surface temperature records.

Comment (4-48):

A number of commenters (e.g., 2853.1, 3215.1, 3217.1, 3729.1) argue that there is a discrepancy between model simulations and trend observations in the tropical troposphere that calls into question model projections of future climate. For example, a reference document provided by commenter 5058 (Douglass et al., 2007) states the following: "On the whole, the evidence indicates that models trends in the troposphere are very likely inconsistent with observations that indicate that, since 1979, there is no significant long-term amplification relative to the surface. If these results continue to be supported, then future projections of temperature change, as depicted in the present suite of climate models, are likely too high."

Response (4-48):

Please refer to Volume 3 for EPA's detailed response to comments specific to the issue of simulated and observed temperatures in the tropical troposphere. On the basis of that response and the assessment literature conclusions underlying it, we conclude that the TSD is relying on the best available science in regard to this issue and its relationship to projected warming.

Comment (4-49):

A number of commenters (e.g., 3394.1, 4509) argue that flaws in the observed temperature record could substantially affect climate projections and result in inaccurate temperature projections. For example, one commenter (3394.1) argues that a number of recent studies have uncovered significant flaws, uncertainties, and biases in the multidecadal surface air temperature record and that these findings "require reanalysis of modeling results previously relied on by the IPCC and other synthesis reports cited by EPA."

Response (4-49):

Please refer to Volume 2 for EPA's responses to comments relating to the accuracy of the observed temperature record. Please refer to response number 4-47of this volume for our response to comments on whether the temperature projections described in the TSD are in accordance with the observed temperature record. These responses show that the observed temperature record does not contain flaws of a magnitude that would significantly affect climate projections.

Comment (4-50):
A number of commenters (e.g., 2895, 2898.1, 3217.1, 3701.1, 4509, 4632R18) argue that the temperature projections described in the TSD are not accurate because the underlying global climate models are unreliable or have not been properly validated. These commenters argue that models contain a number of structural and theoretical flaws, including an improper accounting of climate feedbacks. For example, one commenter (3330.1) claims that observational results suggest that the water vapor feedback is negative and reduces the warming effects of CO_2, and another commenter (4509) argues that "the inability of the computer models to accurately model cloud effects invalidates their use in climate projections."

Response (4-50):
Please see Section 4.1 of this volume for EPA's responses to comments on the validity of model-based climatic projections.

Comment (4-51):
Numerous commenters (e.g. 3455.1, 3570.1, 3575, 4184) state their support for the Findings, noting future increases in global temperature as an effect of anthropogenic GHG emissions that affects health and welfare.

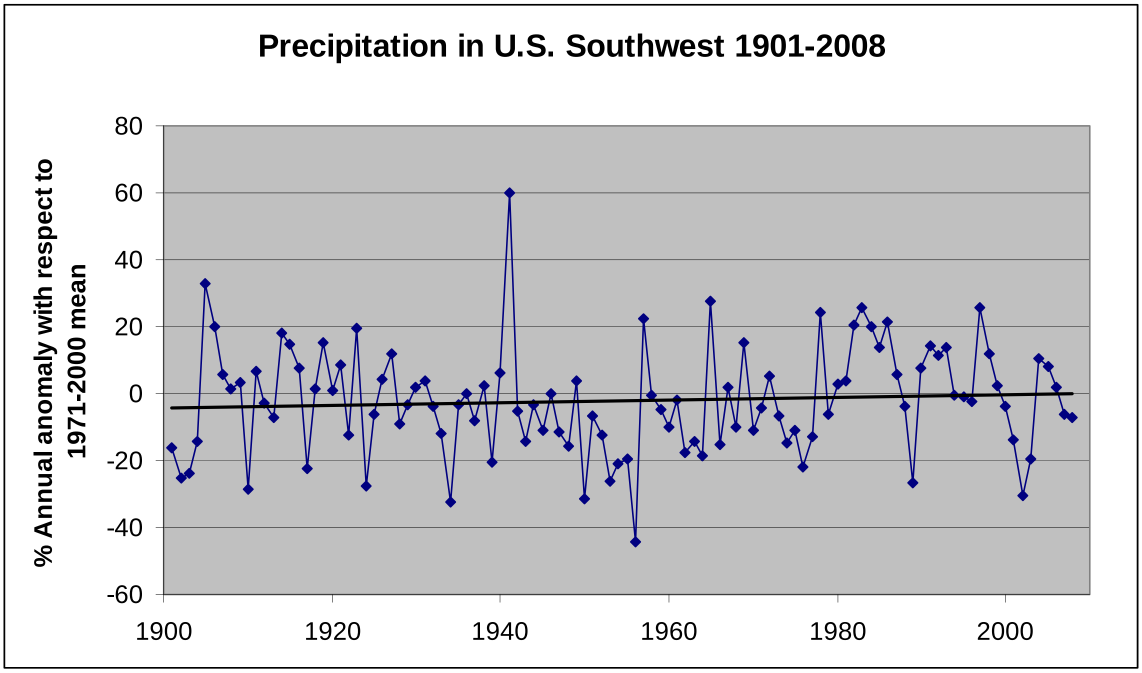
Response (4-51):
EPA agrees that the projected future increases in global temperature summarized in the TSD are linked to atmospheric GHG concentrations and have implications for public health and welfare. Please refer to Section IV.B. of the Findings for a description of the Administrator's findings about the risks associated with rising temperature and why they support a finding of endangerment.

4.4 Future Projections of Precipitation

Comment (4-52):
Two commenters (3136.1 and 3596.2) note that the IPCC chapter Meehl et al. (2007) is referenced in the TSD to discuss a projected greater risk of drought in mid-continental areas of North America and to provide probabilities of extreme drought under the A2 SRES scenario. These commenters use this citation to in turn take issue with the implications of Figure 10.12 in Meehl et al. (2007). Part of Meehl et al. (2007) Figure 10.12 depicts a projected precipitation decrease for the U.S. Southwest, based on SRES scenario A1B, of roughly 0.2 millimeters (mm) per day. Both commenters imply that such a projection does not make sense by stating that "in general, there has been a secular increase in precipitation over this region as the planet warmed," and provide a figure using National Climatic Data Center (NCDC) data to show that average annual precipitation in New Mexico (in one comment) and in the U.S. Southwest (in the other comment) increased slightly from 1895 to 2008. Additionally, the commenters note that 0.2 mm per day is less than three inches per year over the course of 100 years. One commenter (3136.1) states additionally that, based on the New Mexico figure provided, a 3-inch-per-year decline in precipitation is less than 1.5 inches below where precipitation was at the beginning of the 20th century, and that given other pressures on the water supply such as immigration and population growth, this does not represent a "significant endangerment." Finally, a commenter (3136.1) argues that EPA should modify the aforementioned TSD text to refer specifically to the United States and to place the IPCC figures in historical context.

Response (4-52):
We have reviewed the relevant assessment literature in light of these comments. The annual precipitation in the U.S. Southwest has increased over the last century, as reflected in the version of the TSD released

in April 2009, which stated that from 1901 to 2006, annual precipitation for the U.S. Southwest increased at an average rate of 1.3% per century. We have now revised Section 4(e) of the TSD to include updated information that indicates an average rate of precipitation increase for the U.S. Southwest of 3.7% from 1901 to 2008. However, along with this average annual increase, it is important to note the significant temporal variability of the precipitation in the U.S. Southwest during that time, as evidenced in the graph below (data from NOAA's National Climatic Data Center and used in the TSD Section 4[e]), available for download from www.epa.gov/climatechange/endangerment/data.html):



An example of this variability is where the TSD notes that "a severe drought has affected the southwest U.S. from 1999 through 2008."

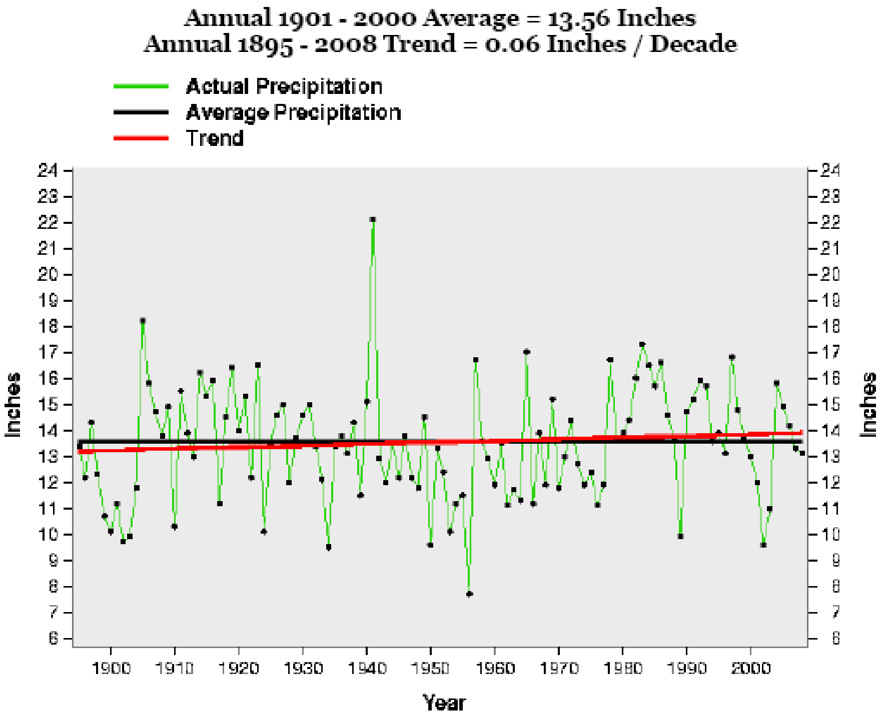
We disagree with the commenters' implication that a projected decrease in annual precipitation for the U.S. Southwest from future climate change is implausible or invalid given a trend over the last century of increasing annual precipitation, especially in light of the precipitation variability over the last century in the region and evidenced in the graph above. A clear conclusion from the assessment literature is that changes in precipitation associated with elevated atmospheric GHG concentrations will be highly regional along both temporal and geographic lines. To further clarify this key point, the following text has been added to Section 6(c) of the TSD in relation to this topic:

> Karl et al. (2009) report model projections of future precipitation in the United States generally indicate northern areas will become wetter, and southern areas, particularly in the West, will become drier. In some northern areas, warmer conditions will result in more precipitation falling as rain and less as snow. In southern areas, significant reductions in precipitation are projected in winter and spring as the subtropical dry belt expands, particularly in the Southwest (Karl et al., 2009).

We also disagree with the commenter's assertion that a roughly 3-inch-per-year decline in precipitation (roughly what Meehl et al. [2007] projects) for the U.S. Southwest is insignificant, relative to the average annual rainfall in the region for the last century. We reproduced the graph provided by a commenter (3596.2) using the NOAA Web site (NOAA, 2009:

http://www.ncdc.noaa.gov/oa/climate/research/cag3/sw.html), which we present at the end of this response, and found that the average annual precipitation for 1895–2008 for the U.S. Southwest was 13.45 inches. A 3-inch decline by 2100 represents a decrease of greater than 20% in annual precipitation relative to the 1895–2008 average, which would represent a significant climate change impact to the U.S. Southwest, particularly given simultaneous increasing temperatures and, as the commenter points out, the additional nonclimatic pressures on water in the region.

Finally, we reviewed the TSD in light of the commenter's request for modifications to refer specifically to the United States and to place the IPCC figures in historical context, and we do not find such modifications necessary. The TSD already provides U.S.-specific information on precipitation in Section 4e (U.S. Changes in Precipitation) and Section 6c (Projected Changes in U.S. Temperature, Precipitation Patterns, Sea Level Rise). These discussions are thorough and have been revised as necessary in response to specific comments on their content.



Comment (4-53):

One commenter (3136.1) criticizes the TSD for stating the following on page 62: "For the contiguous U.S., a study in Christensen et al. (2007) finds widespread increases in extreme precipitation events under SRES A2 (high emissions growth)." The commenter states that EPA has "exaggerate[d] for effect" by citing only SRES scenario A2 while the "IPCC tend[s] to give A2, A1B, and B2 in its text."

Response (4-53):

We have reviewed the relevant text and disagree with the commenter's assertion that we have exaggerated anything. The sentence the commenter references is found in Section 6(e) of the TSD statement, and it came directly from the IPCC (Christensen et al., 2007). The paper in question, that is referenced in Christensen et al. (2007), is Diffenbaugh et al. (2005). Diffenbaugh et al. (2005) only compared two scenarios: a reference scenario (1961–1985) and a future scenario (SRES A2 2071-2095). The results were cited in IPCC 2007 and subsequently used in the TSD.

Additionally, the latest assessment report on U.S. climate change impacts (Karl et al., 2009) states that: "Climate models project continued increases in the heaviest downpours during this century." This statement is consistent with the quoted study results.

Because the statement that the commenter criticized in the TSD is very clear that the study used a high-emission scenario, as well as the fact that the statement above from Karl et al. (2009) is consistent with the study results, we disagree that we have "exaggerated for effect." However, to increase clarity in the TSD regarding the issue of projected U.S. extreme precipitation events, we have added to the TSD the above statement from Karl et al. (2009).

Comment (4-54):
A number of commenters (e.g., 3722, 2750) argue that the projected changes in drought severity and frequency discussed in the TSD are not credible. One commenter (2750) asserts that drought prediction is impossible because there is "no pattern to drought occurrence." Another commenter (3722) provides a graph from NOAA that, according to the commenter, "certainly indicates that modest future warming does not present a significant risk of increased drought."

Response (4-54):
The TSD (April 2009 and final) reports the IPCC (Meehl et al., 2007) projection of a tendency for drying in mid-continental areas during summer, indicating a greater risk of droughts in those regions. The final TSD includes additional detail on this topic, noting the USGCRP conclusion (Karl et al., 2009) that droughts are likely to become more frequent and severe in some regions, particularly the Southwest. Commenters did not provide any literature or other compelling evidence in contradiction to these robust conclusions from the assessment literature. Therefore, we disagree that the projections described in the TSD lack credibility.

We note that the graph provided by one commenter (3722) covers the entire contiguous United States. As such, it is not relevant to the drought projections discussed in the TSD, which are regionally specific. As the TSD notes, changes in precipitation will be regionally variable. In Section 6(e), the TSD states that droughts are likely to become more frequent and severe in some regions, particularly the Southwest; it does not state the droughts are likely to become more frequent or severe throughout the entire contiguous United States. Therefore, we disagree with the comment that the graph supports the conclusion that future warming does not present a significant risk of increased drought, though we note that the risk of increased frequency and severity of droughts is more relevant to some regions than others.

Please refer to Volume 2 for EPA's responses to comments on observed extreme weather events, including droughts. As noted in this section, EPA does not make the claim that drought has, in the aggregate, increased in the United States.

Comment (4-55):
Several commenters (e.g., 3394.1) object that projections of precipitation and regional temperature are too uncertain to make projections of drought, especially given a number of related uncertainties in attribution of historic droughts, net trends, and ground water management. Additionally, commenters state that EPA has failed to describe the uncertainties associated with the precipitation projections in the TSD.

Response (4-55):
Although precipitation is indeed difficult to model, the assessment literature, as summarized in the TSD, indicates that certain robust conclusions can be drawn. For example, models consistently project increased drought in the American Southwest (Seager et al., 2007). This trend will, of course, be overlaid on continuing natural variability. Karl et al. (2009) states the following on this topic:

> Projections of changes in precipitation largely follow recently observed patterns of change, with overall increases in the global average but substantial shifts in where and how precipitation falls. Generally, higher latitudes are projected to receive more precipitation, while the dry belt that lies just outside the tropics expands further poleward, and also receives less rain. Increases in tropical precipitation are projected during rainy seasons (such as monsoons), and especially over the tropical Pacific. Certain regions, including the U.S. West (especially the Southwest) and the Mediterranean, are expected to become drier. The trend towards more heavy downpours is expected to continue, with precipitation becoming less frequent but more intense. More precipitation is expected to fall as rain rather than snow.

With regard to the criticism of EPA's description of precipitation projection uncertainties, we note that precipitation projections are described in two different locations in the TSD: Section 6b (Projected Changes in Global Temperature, Precipitation Patterns, Sea Level Rise, and Ocean Acidification) addresses global changes, and Section 6(c) addresses projected changes in the United States. An examination of both of these sections confirms that they include information on the uncertainties in the projections. In Section 6(b), for example, the TSD states: "Models simulate that global mean precipitation increases with global warming (Meehl et al., 2007). However, there are substantial spatial and seasonal variations. Increases in the amount of precipitation are *very likely* in high latitudes, while decreases are *likely* in most subtropical land regions, continuing observed patterns in recent trends in observations." With regard to U.S. precipitation projections, we have added the following text to Section 6(c) of the TSD to better describe the relevant uncertainties and trends: "Overall, annual mean precipitation in the northeastern U.S. is very likely to increase and likely to decrease in the southwestern U.S."[6] Based on our review, we conclude that the characterization of uncertainty regarding global and U.S. precipitation projections in the TSD is reasonable and appropriate.

Comment (4-56):
Several commenters (3475, 6096) submit or refer to Solomon et al. (2009) as one of a number of examples of studies showing that the IPCC AR4 was too conservative in its estimation of emissions and impacts. One commenter (3475) states that Solomon et al. (2009) "predicted that peak GHG concentration levels of 450-600 ppmv [parts per million by volume] in this century will result in irreversible dry-season rainfall reductions in several regions comparable to those of the 'dust bowl' era…"

Response (4-56):
We have reviewed the Solomon et al. (2009) paper and we find that its conclusions are generally consistent with the trends and projections described in the TSD. Solomon (2009) was incorporated by Karl et al. (2009), which is a USGCRP assessment report and therefore included in the TSD.

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[6] According to IPCC terminology, "likely" conveys a 66 to 90% probability of occurrence. . See Box 1.2 of the final TSD for a full description of IPCC's uncertainty terms.

<u>Comment (4-57):</u>
One commenter (3136.1) states that the 2007 IPCC report “does not adequately review the topic of precipitation variability and its causes across the U.S.” The commenter argues that the TSD therefore cannot rely on the IPCC for this topic and a more in-depth look at the relevant science is required as the basis for an endangerment finding.

<u>Response (4-57):</u>
Please refer to Volume 3 for EPA’s responses to comments generally focused on attribution, including of precipitation. Here, we focus on the component of the comment that is specific to precipitation variability and the IPCC review of this topic.

Although significant challenges exist in projecting patterns of future precipitation, in particular, in projecting precipitation variability at less than a continental scale (as discussed in Section 6(b) [Projected Changes in Global Temperature, Precipitation Patterns, Sea Level Rise, and Ocean Acidification] of the TSD), we disagree with the commenter that the AR4 (Trenberth et al., 2007) does not provide an adequate review of precipitation variability and its causes in the United States. Trenberth et al. covers in detail the observed precipitation from 1900 to 2005. Time series are presented for 19 world regions (including four areas in the United States), along with trends at the 5° latitude/longitude grid level, and it is noted where the trend is significant at the 5% level. Additionally, the IPCC chapter Christensen et al. (2007) details the projected changes in precipitation for North America along with the degree of agreement among the climate models. Figure 11.12 in Christensen et al. (2007), in particular, displays the projected fractional changes in annual, winter, and summer precipitation specific to North America. Additionally, Figure 11.12 shows, by color shading, the number of models out of 21 that project increases in precipitation.

Further, as noted in the TSD, we do not rely solely on the IPCC; the TSD summarizes a number of other peer-reviewed assessment reports, including those by the USGCRP, CCSP, and NRC. These reports also review precipitation variability in the United States. For example, Figure 4.1 (reproduced below) in Lettenmaier et al. (2008), found in CCSP (2008e), displays the mean and coefficient of variation of annual precipitation in the continental United States and Alaska.

Lettenmaier et al. (2008) concludes that:

> Precipitation variability generally is lowest in the humid areas, and highest in the arid and semi-arid West, with a tendency toward lower variability in the Pacific Northwest, which is more similar to that of the East than the rest of the West.

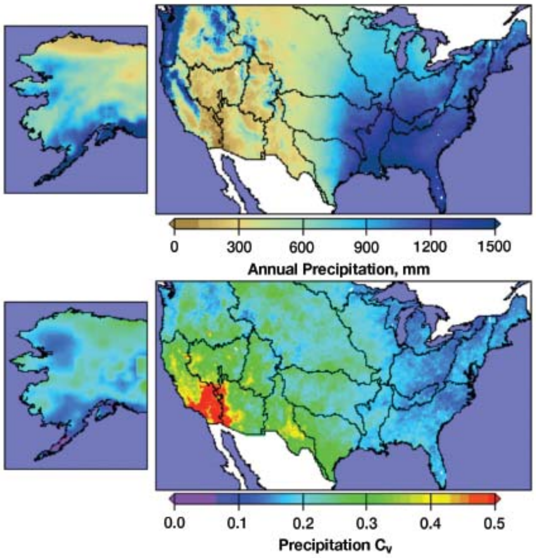

Figure 4.1 *Mean and coefficient of variation of annual precipitation in the continental U.S. and Alaska. Data replotted from Maurer et al. (2002).*

CCSP (2008c) directly addresses the current skills of climate models in projecting precipitation:

> Climate model simulation of precipitation has improved over time but is still problematic. Correlation between models and observations is 50 to 60% for seasonal means on scales of a few hundred kilometers. Comparing

> simulated and observed latitude-longitude precipitation maps reveals similarity of magnitudes and patterns in most regions of the globe, with the most striking disagreements occurring in the tropics.

Based on our review of the TSD and the underlying assessment literature in light of this comment, we conclude that the TSD provides an accurate and sound summary of the state of the science. It discusses to the extent permitted by current understanding and modeling ability, the observed and projected precipitation variability in the United States.

Comment (4-58):
One commenter (7037) references Shepherd (2005), and writes that the article conveys the view that "Increased precipitation, not drought, is possible over urban areas because of the presence of the urban heat island coupled with a moister and more unstable atmosphere, resulting in more wet deposition."

Response (4-58):
We agree that the microclimate in urban areas differs from that of neighboring rural areas, and that observational studies have linked urban effects to precipitation increases over background values. This is discussed in the AR4 (Trenberth et al., 2007), which we cite in the TSD. Trenberth et al. (2007) states that "Urban effects can lead to increased precipitation (5 to 25% over background values) during the summer months within and 50 to 75 kilometers (km) downwind of the city." Thus, because the IPCC is aware of and discusses this effect, the large-scale precipitation projections which we cite in the TSD are not called into question by the commenter's point, and the TSD's discussion of precipitation projections is reasonable and sound.

4.5 Future Projections of Extreme Weather Events

Comment (4-59):
A number of commenters (e.g., 3136.1, 3347.1, 3394.1) argue that hurricane frequency is likely to decrease with rising temperature. Several of these commenters (e.g., 3136.1, 3394.1) refer to a study by Knutson et al. (2008), which, according to the commenters, indicates that future anthropogenic warming is anticipated to reduce the frequency of tropical storms and hurricanes throughout the Atlantic basin (3136.1), or that hurricanes "are likely to be substantially *rarer* events under projected climate change" (3394.1). One commenter (3136.1) states that Knutson's results show that the simulated number of hurricanes making landfall decreases by a greater percentage than the number of hurricanes itself.

Similarly, a commenter (3347.1) argues that a key reference (Karl et al., 2008) EPA used to describe how climate change could affect hurricanes "shows that the numbers of and frequency of hurricanes making landfall in North America has not changed at all" and that, "[t]he Administrator's factual claims are fundamentally inconsistent with the science on which she explicitly relies."

Response (4-59):
We have reviewed the submitted references and the relevant assessment reports in light of these comments, and we find that the TSD accurately summarizes the key conclusions from the assessment literature on this topic. The TSD already notes that "[f]requency changes in hurricanes are currently too uncertain for confident projections." Thus, while it is true that tropical cyclone (i.e., tropical storms and hurricanes) frequency may not increase and could potentially decrease because of climate change, we disagree with the more definitive statements commenters make that tropical cyclone frequency will

"likely"[7] decrease. As noted in the TSD, the assessment literature indicates that projections in frequency changes in tropical cyclones are currently too uncertain for confident projections.

Section 6(e) includes a clear summary of the findings of the literature on this topic:

> Karl et al. (2008) indicate projections in frequency changes in tropical cyclones are currently too uncertain for confident projections. Some modeling studies have projected a decrease in the number of tropical cyclones globally due to increased stability of the tropical atmosphere in a warmer climate, characterized by fewer weak storms and greater numbers of intense storms (Meehl et al., 2007). A number of modeling studies have also projected a general tendency for more intense but fewer storms outside the tropics, with a tendency towards more extreme wind events and higher ocean waves in several regions associated with these deepened cyclones (Meehl et al., 2007).

We have reviewed the Knutson et al. (2008) study referenced by several commenters. The study assesses the changes in large-scale climate that are projected to occur by the end of the 21st century by an ensemble of global climate models and finds that Atlantic hurricane and tropical storm frequencies are reduced. The authors state that their results "do not support the notion of large increasing trends in either tropical storm or hurricane frequency driven by increases in atmospheric greenhouse-gas concentrations," a finding that is not inconsistent with the findings of Karl et al. (2008) in the CCSP report *Weather and Climate Extremes in a Changing Climate* (CCSP, 2008i) or Meehl et al. (2008), which are cited in the above paragraph. Further, we note that although the simulated results of Knutson et al. (2008) suggest that the frequency of both tropical storms and major hurricanes in the tropical Atlantic ocean will decrease in the 21st century, several other studies (Ouuchi et al., 2006; Sugi et al., 2002) assessed by Gutowski et al. (2008) in CCSP (2008i) suggest that a future increase of tropical cyclone frequency for the North Atlantic is also plausible. In addition, two other studies assessed in by Gutowski et al. (Chauvin et al., 2006; Emanuel et al., 2008) find that, in multimodel experiments, the sign of changes in tropical cyclone frequency in the North Atlantic basin depends on the climate model used. Based on a thorough review of the literature, Gutowski et al. (2008) conclude that future projections of tropical cyclones depend on not only global mean climate considerations but also regional factors that can potentially affect tropical cyclone behavior, such as the local potential intensity, influences of vertical wind shear and other circulation features.

With respect to frequency projections for tropical cyclones globally, we agree that a number of studies suggest that the total number of cyclones could decrease because of the effects of elevated atmospheric GHG concentrations. The summary from the TSD quoted above cites Meehl et al. (2007) in making this point.

In light of the uncertainties involved with tropical cyclone frequency projections and the fact that Karl et al. (2009), the most recent major assessment report to address the issue, reaches conclusions consistent with previous assessment reports, we have concluded that no changes to the TSD are necessary on this topic. We continue to find that the following conclusion from Gutowski et al. (2008), which is based on their review of more than eight separate modeling studies and other relevant information, accurately captures the current state of knowledge: "It is unknown how late 21st century tropical cyclone frequency in the Atlantic and North Pacific basins will change compared to the historical period (approximately 1950-2006)."

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[7] According to IPCC terminology, "likely" conveys a 66 to 90% probability of occurrence. . See Box 1.2 of the final TSD for a full description of IPCC's uncertainty terms.

For EPA's responses to comments on the validity of observed and measured data on extreme events, please refer to Volume 2, Section 2.5: Extreme Weather Events.

Comment (4-60):
A number of commenters (e.g., 0339, 1927, 3136.1, 3217.1, 3291.1, 3347.3, 3394.1,3397, 4003, 11453.1) argue that the projections of increasing hurricane intensity reported in the April 2009 TSD are uncertain, outdated, or not credible. One commenter (3136.1) argues that recent research by Vecchi and Soden (2007a, 2007b) suggests that future changes in the pattern of sea surface temperature may act to reduce tropical cyclone intensity. The commenter additionally claims that the IPCC AR4 is "an outdated reference" on the topic of both hurricane frequency and intensity, and that, "[EPA] simply can *not* use the IPCC AR4 as a reference for expectations of future Atlantic tropical cyclones and their impacts."

Another commenter (3347.3) argues as follows: "EPA's assumption that climate change will increase hurricane frequency and intensity is not supported by the evidence EPA cites." In particular, the commenter claims that EPA did not address the "assumptions and validation" of the following statement from IPCC (2007d) quoted in the TSD: "Based on a range of models, it is likely that tropical cyclones (tropical storms and hurricanes) will become more intense, with stronger peak winds and more heavy precipitation associated with ongoing increases of tropical sea surface temperature."

Response (4-60):
We have reviewed the submitted studies and disagree that the projections of increasing tropical cyclone intensity[8] reported in the April 2009 TSD were not credible or have been supplanted by conflicting information. The discussion of tropical cyclone intensity in the April 2009 TSD was based principally on the IPCC AR4 (2007d) and the CCSP (2008i) report *Weather and Climate Extremes in a Changing Climate*. We have re-reviewed these assessment reports, the TSD, and the most recent assessment report to analyze tropical cyclone intensity, Karl et al. (2009), and we have concluded that the information from the April 2009 TSD continues to accurately summarize the most current and compelling science available on this topic as expressed in the assessment literature. Therefore, we have retained the key conclusions about tropical cyclone intensity from the April 2009 TSD in the Final TSD. The TSD states the following in Section 6(e):

> Based on a range of models, it is likely that tropical cyclones (tropical storms and hurricanes) will become more intense, with stronger peak winds and more heavy precipitation associated with ongoing increases of tropical sea surface temperatures (IPCC, 2007d). Karl et al. (2008) analyze model simulations and find that for each 1.8ºF (1ºC) increase in tropical sea surface temperatures, core rainfall rates will increase by 6 to 18%, and the surface wind speeds of the strongest hurricanes will increase by about 1 to 8%. Storm surge levels are likely to increase because of increasing hurricane intensity coupled with sea level rise (Karl et al., 2009).

We disagree with the comment (3136.1) that the IPCC AR4 is an outdated reference and note that the key IPCC conclusions on tropical cyclone intensity are consistent with the conclusions of both the CCSP report *Weather and Climate Extremes in a Changing Climate* (2008i) and Karl et al. (2009). Consistent with the relevant IPCC report (2007d), Karl et al. (2009) note the following: "As ocean temperatures continue to increase in the future, it is likely that hurricane rainfall and wind speeds will increase in response to human-caused warming." Karl et al. (2009) also retains the conclusion from CCSP (2008i) that model simulations suggest that for each 1˚C increase in tropical sea surface temperatures, the surface wind speeds of the strongest hurricanes will increase by about 1-8%.

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[8] Consistent with CCSP (2008i), EPA's statements regarding hurricane intensity refer to maximum surface wind speeds.

We have reviewed the studies that commenters reference in support of their arguments regarding hurricane intensity, and we have determined that these studies neither invalidate nor contradict the overall conclusions from the assessment literature summarized in the TSD. Gutowski et al. (2008) in CCSP (2008i) and Karl et al. (2009) assess the Vecchi and Soden (2007a, 2007b) studies. Gutowski et al. (2008) note that the results of Vecchi and Soden (2007b) show increases in potential intensity[9] for about two-thirds of the area of the Atlantic basin and slight decreases in the other one-third, along with a clear tendency for increased vertical wind shear and reduced mid-tropospheric relative humidity in some regions. According to Gutowski et al., "The net effect of these composite changes remains to be modeled in detail, although existing global modeling studies (Oouchi *et al*., 2006; Bengtsson *et al.*, 2007) suggest increases in the intensities and frequencies of the strongest storms."

As mentioned in response 4-59, several commenters reference the hurricane frequency results for the North Atlantic from Knutson et al. (2008). This study also obtained model results for hurricane intensity. In regard to the intensity of the strongest hurricanes, Knutson et al. (2008) report the following: "In agreement with previous studies, the frequency and intensity of the strongest hurricanes simulated by the model are increased for both surface wind speed (Fig. 1 b,c) and central pressure."

As discussed in response 4-59, there are significant uncertainties involved with projecting tropical cyclone frequency. This is in regard to tropical cyclone intensity as well. Gutowski et al. (2008) discuss the key uncertainties (e.g., the limited capacity of climate models to adequately simulate intense tropical cyclones and the precise influence of factors such as vertical wind shear for different regions) in some detail. These uncertainties are reflected in the conclusions of the assessment literature and acknowledged in the TSD. Consistent with the assessment literature, the TSD does not make any definitive statements about tropical cyclone intensity but rather states that it is "likely" that tropical cyclones will become intense (see Box 1.2 of the TSD for a description of the communication of uncertainty).

In response to the comment that we did not explicitly address the "assumptions and validations" involved with model-based projections of hurricane intensity within the April 2009 TSD, we have added the following paragraph to Section 6(e) of the final TSD:

> Sources of uncertainty involved with projecting changes in tropical cyclone activity include the limited capacity of climate models to adequately simulate intense tropical cyclones and potential changes in atmospheric stability and circulation (Karl et al., 2008). Taking these uncertainties into consideration, Karl et al. (2009) reached the following conclusion on the basis of both model- and theory-based evidence: "As ocean temperatures continue to increase in the future, it is likely that hurricane rainfall and wind speeds will increase in response to human-caused warming."

Please refer to Section 4.1 of this volume for EPA's responses to comments on the validity of using models to project future climate change.

Comment (4-61):

A number of commenters (e.g., 1927, 3136.1, 3347.1, 3394.1) look to historical data as evidence that hurricane intensity will not increase due to elevated atmospheric GHG concentrations.

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[9] Potential intensity refers to the maximum intensity that can be achieved by a cyclone for a given atmospheric/oceanic thermodynamic state. According to CCSP (2008i), tropical cyclones usually do not reach this state due to various factors that may include vertical shear of the horizontal wind and oceanic cooling by cyclone-induced cooling cool water from below the mixed layer to the surface.

Response (4-61):

Please refer to Volume 2, Section 2.6: Extreme Weather Events for EPA's responses to comments on the observed record of hurricane intensity. On the basis of the science discussed in that section and within response number 4-60 in this section, we conclude that trends in observed tropical cyclone activity—though uncertain in some regions and especially for earlier time periods (prior to the satellite era)—are not inconsistent with the future projections for hurricane intensity summarized in the TSD. Our review indicates that the TSD provides a sound and reasonable summary of the scientific evidence on hurricane intensity as assessed by the IPCC and CCSP/USGCRP.

Comment (4-62):

A number of commenters (e.g., 2750, 3136.1) argue that the projections of potential increases in flooding discussed in the TSD are not well supported by the evidence or lack credibility. According to at least one commenter (3136.1), there is evidence that our climate is not changing, despite increases in GHGs, in such a way as to increase flood events. The commenter states that several studies (Small et al., 2006; Lins and Slack, 1999), "do not find increases in the highest streamflow (the ones associated with flooding)." Another commenter (3596.3) cites several studies (Starkel, 2002; Noren, 2002; and Schimmelmann, 2003) that the commenter states "attribute flooding to solar phenomena and not increases in greenhouse gases."

Response (4-62):

We have reviewed the TSD in light of the comments and literature submitted. Section 6(e) of the TSD summarizes the conclusion from Meehl et al. (2007) that the intensity of precipitation events is projected to increase globally, particularly in tropical and high-latitude areas that experience increases in mean precipitation, as well as the finding from Meehl et al. (2007) that increases in heavy precipitation events have been linked to increases in flooding. In addition, the TSD now summarizes information from the most recent assessment report to assess precipitation projections and their implications for flooding, Karl et al. (2009). Summarizing the findings of Karl et al., the TSD states:

> Climate models consistently project that parts of the eastern United States will experience increased runoff, which accumulates as streamflow and can cause flooding when heavy precipitation persists for weeks to months in large river basins (Karl et al., 2009).

As noted in the TSD, Karl et al. (2009) report that heavy downpours that are now one-in-20-year occurrences are projected to occur about every four to 15 years by the end of this century, depending on location, and that the intensity of downpours is projected to increase by 10 to 25% by the end of the century relative to today. According to Karl et al., floods are likely to become more common and more intense as regional and seasonal precipitation patterns change and rainfall becomes more concentrated into heavy events. Karl et al. report that floods are likely to be amplified by climate change in most regions.

We reviewed the Small et al. (2006) and Lins and Slack (1999) studies referenced by commenters, as well as other recent literature. While the studies submitted by the commenters do not find increases in the highest streamflows associated with flooding, but rather that increases in low to moderate streamflows can occur without a concomitant increase in flooding, they on their own do not constitute a compelling counterargument to the conclusion from the assessment literature, as summarized in the TSD, that climate is changing in a way that is reasonably expected to lead to increased flooding in some locations.

Small et al. (2006) analyzed trends in annual seven-day low, average, and high flows along with seasonal precipitation that is averaged over individual basins in the eastern United States. The study finds that fall precipitation and low flows have simultaneously increased across the eastern United States but that spring precipitation and high flows "do not show a widespread trend." Lins and Slack (1999) reach a more general conclusion that "median or average streamflow is increasing, but annual maximum flows (including floods) are neither increasing nor decreasing." Lins and Slack (1999), however, do not reach the same conclusion as the commenter regarding the implications of their results, stating that the question of whether increasing atmospheric CO_2 will lead to more floods and droughts "is a more problematic issue, and one on which current climate modeling studies may soon be able to shed some light."

A recent study (Collins, 2009) finds evidence in contradiction to the findings of Small et al. (2006) for the New England region. It investigates 28 long-term annual flood series for New England watersheds with dominantly natural streamflow and finds that 25 of the series show upward trends, of which 40% are statistically significant. The authors note that the apparent disparity with the results of Small et al. (2006) may be due to evaluating greater number of gage records and additional years of data. They additionally note that their findings "are consistent with those of Hodgkins and Dudley (2005) and McCabe and Wolock (2002), and also make sense in light of the recent research documenting increased frequency and increased intensity of events in the upper 10th percentile of the daily precipitation distribution in the Northeast United States over the last century…"

We note that the CCSP (2008i) report *Weather and Climate Extremes in a Changing Climate* states that other methodologies have resulted in opposite judgments about trends in high streamflow trends as compared with Lins and Slack (1999). CCSP reports the following:

> A series of studies by two research groups (Lins and Slack, 1999, 2005; Groisman *et al.*, 2001, 2004) utilized the same set of stream gages not affected by dams. This set of gages represents stream flow for approximately 20% of the contiguous U.S. area. The initial studies both examined the period 1939-1999. Differences in definitions and methodology resulted in opposite judgments about trends in high streamflow. Lins and Slack (1999, 2005) reported no significant changes in high flow above the 90th percentile. On the other hand, Groisman *et al.* (2001) showed that for the same gauges, period, and territory, there were statistically significant regional average increases in the uppermost fractions of total streamflow. However, these trends became statistically insignificant after Groisman *et al.* (2004) updated the analysis to include the years 2000 through 2003, all of which happened to be dry years over most of the eastern United States. They concluded that "… during the past four dry years the contribution of the upper two 5-percentile classes to annual precipitation remains high or (at least) above the average while the similar contribution to annual streamflow sharply declined. This could be anticipated due to the accumulative character of high flow in large and medium rivers; it builds upon the base flow that remains low during dry years…" All trend estimates are sensitive to the values at the edges of the time series, but for high streamflow, these estimates are also sensitive to the mean values of the flow.

CCSP (2008i) includes additional detail on changes in runoff that is consistent with the information summarized in the TSD. It notes that changes in runoff have been observed in many parts of the world, with increases or decreases corresponding to changes in precipitation. It additionally reports that climate models suggest that runoff will increase in regions where precipitation increases faster than evaporation, such as high Northern latitudes.

Our review of the literature submitted by commenter 3596.3 revealed that it has essentially no relevance to the question of whether human-induced climate change will exacerbate the risk of flooding. The submitted studies do not focus on this topic but rather other factors related to past flooding episodes in the relatively recent and distant past. We agree with the commenter that flooding can occur due to a variety of factors other than CO_2-induced global warming but note that this fact in no way implies that human-induced climate change will not or could not exacerbate the risk of flooding.

In summary, we have reviewed the TSD in light of comments on streamflow and flooding trends and conclude that it accurately represents the current state of the science as expressed in the assessment literature. In light of the conclusions of the assessment literature, as well as recent studies that are directionally consistent, we agree that peak streamflows will likely not increase in all regions or seasons, and the TSD does not state that they are expected to. However, the broad and sweeping arguments several commenters make regarding streamflow and flooding in general, on the basis of several qualified and narrowly focused studies, does not provide a compelling or sufficient basis on which to conclude that the conclusions of the assessment summarized in the TSD are flawed. Therefore, we disagree that the projections of potential increases in flooding discussed in the TSD are not well supported by evidence or lack credibility.

Comment (4-63):

Several commenters (0339, 3136.1, 3722, 13091) argue that (nontropical) storms will not become more intense as a consequence of elevated atmospheric GHG concentrations. For example, one commenter (1309.1) states: “Since global warming tends to warm the polar regions more than the equatorial zones, one would expect a decrease in the severity of storms.” Another commenter (3722) makes a similar argument, quoting Lindzen (2005) as stating the following: “According to any textbook on dynamic meteorology, one may reasonably conclude that in a warmer world, extra-tropical storminess and weather variability will actually decrease.”

Response (4-63):

Projected future extra-tropical cyclone activity varies by region and the TSD does not suggest a global increase in extra-tropical cyclone frequency. In fact, Section 6(e) of the TSD states:

> The IPCC (Meehl et al., 2007) concludes model projections show fewer mid-latitude storms (or extratropical, primarily cold season) averaged over each hemisphere, associated with the poleward shift of the storm tracks that is particularly notable in the Southern Hemisphere, with lower central pressures for these poleward shifted storms. Over North America, Gutowski et al. (2008) indicate strong mid-latitude storms will be more frequent though the overall number of storms may decrease.

CCSP’s (Gutowski et al., 2008) projections are drawn citing six studies published between 2005 and 2007, all of which produce consistent results. CCSP finds these projections are also consistent with observed trends over the last half of the 20th century (see Volume 2, Section 2.5 regarding these observed trends).

We have reviewed Dr. Lindzen’s statements regarding extra-tropical storminess and weather variability and other literature referenced by the commenters, and we agree that a decrease in the meridional (north-to-south) temperature gradient could act to decrease some extra-tropical storm activity, especially equator-ward of the high latitudes. Meehl et al. (2007) refer to a modeling study that projects reduced extra-tropical cyclone activity over the mid-latitudes due to reduced baroclinicity (temperature contrast) in the lower troposphere. However, it is also clear that many other factors influence extra-tropical cyclone activity in addition to the north–south temperature contrasts. For example, CCSP (Gutowski et al., 2008) indicate that the increased storm strength in the northeast Atlantic found by some studies may be linked to the poleward retreat of arctic ice and a tendency toward less frequent blocking and more frequent positive phase of the Northern Annular Model (NAM). On the basis of our re-examination of the TSD and assessment literature in light of the comments, we conclude that summary of the assessment literature projections on extra-tropical storms in the TSD is reasonable and sound.

Comment (4-64):
A number of commenters (e.g., 2174, 3383.1) state their support for the Findings, noting future increases in storm intensity as one of the environmental effects of climate change.

Response (4-64):
We agree that the intensity of certain types of storms is likely to increase. Please refer to Section IV.B. of the Findings for a description of the Administrator’s findings about projected changes in storm intensity due to climate change and why the risks associated with these changes provide support for the endangerment finding.

Comment (4-65):
Many commenters (e.g., 3383.1, 3455.1, 3570.1, 3574.1, 3995, 4171, 4184, 9786, 10809) state their support for the Findings, noting future increases in precipitation and flooding as one of the environmental effects of climate change.

Response (4-65):
We agree that projected climate changes will alter precipitation patterns and is likely to amplify flooding in some regions. Please refer to Section IV.B of the Findings for a description of the Administrator’s findings about projected changes in precipitation and flooding due to climate change and why the risks associated with these changes provide support for the endangerment finding.

4.6 Future Projections of Sea Level Rise

Comment (4-66):
Several commenters (e.g., 3136.1, 3747.1) argue that since sea level rise rates are closely tied to global temperature rise, the lower-than-expected rise in global temperatures (observed temperatures have, according to the commenters, tracked along the low end of the IPCC range of projections) implies a sea level rise also near the low end of the IPCC projected range (i.e., closer to 7 inches than to 23 inches). A number of other commenters (e.g., 3224, 11459) argue that contrary to the IPCC projections, sea level will not actually rise, or that the IPCC projections are not supported by facts. Commenter 3397 argues that in discussing sea level rise projections, the TSD relies on inadequate scientific information and does not adequately address uncertainty.

Response (4-66):
We have reviewed the referenced studies, the TSD and the underlying assessment literature in light of these comments. On the basis of our review, we conclude that the TSD properly summarizes the assessment literature regarding the current state of scientific knowledge, uncertainties regarding the potential effects of dynamic ice sheet processes on global sea levels and other factors, and statements of likelihood and probability.

First, we disagree that recent temperatures imply lower-than-projected future temperature change for reasons discussed in Volume 2 and response number 4-47 in this volume.

With regard to sea level rise projections, several very recent CCSP assessment studies suggest that, if anything, future sea level rise is likely to be near or above the high end of the IPCC projections. In Section 6(b), the TSD notes that “Dynamic processes related to ice flow not included in current models but suggested by recent observations could increase the vulnerability of the ice sheets to warming, increasing future sea level rise.” Further, the TSD summarizes CCSP (2008a), which finds that the Greenland and West Antarctic ice sheets show acceleration of flow and thinning, and that inclusion of these processes in models will likely lead to sea level projections for the end of the 21st century that substantially exceed the projections presented in the IPCC (2007). According to the CCSP 4.1 (2009b) sea level rise report and Karl et al. (2009) and as noted in the TSD, Rahmstorf (2007) and others have suggested that a global sea level rise of 1 m (and up to 1.4 m) is plausible within this century if increased melting of ice sheets in Greenland and Antarctica is added to the factors included in the IPCC estimates. In fact, CCSP (2009b) concludes: “Therefore, thoughtful precaution suggests that a global sea level rise of 39 inches (100 cm) to the year 2100 should be considered for future planning and policy discussions.”

In light of these projections from the assessment literature, we disagree that the recent temperature trend implies future sea level near the low end of the IPCC projected range or that sea level will not actually rise, as the commenters contend. We find that the discussion of sea level rise in the TSD is reasonable and sound and appropriately reflects key conclusions from the assessment literature.

Comment (4-67):
A number of commenters (e.g. 3394.1, 3596.2, 3722) argue that the TSD presents incorrect or biased information about the likelihood that sea level rise will accelerate over time. Several of these commenters cite van de Wal et al. (2008) and/or Joughin et al. (2008) as evidence for their arguments. Commenter 3596.2, for example, cites van de Wal et al. (2008) and Joughin et al. (2008) as evidence that it is unclear that high rates of sea level rise should be expected in the future.

Response (4-67):
We have reviewed the referenced studies, the TSD and the assessment literature in light of these comments. On the basis of this review, we conclude that the cited studies are not inconsistent with the assessment conclusions on future sea level rise summarized in the TSD.

The TSD reports the IPCC (Meehl et al., 2007) conclusion that for all SRES emission scenarios the average rate of sea level rise during the 21st century very likely[10] exceeds the 1961-2003 average rate (1.8 ± 0.5 mm yr^{-1}). In Section 6b (Projected Changes in Global Temperature, Precipitation Patterns, Sea Level Rise, and Ocean Acidification), the TSD states:

> By the end of the century (2090–2099), sea level is projected by IPCC (2007d) to rise between 7 and 23 inches (18 and 59 cm) relative to the base period (1980–1999). These numbers represent the lowest and highest projections of the 5 to 95% ranges for all SRES scenarios considered collectively and include neither uncertainty in carbon cycle feedbacks nor rapid dynamical changes in ice sheet flow. In all scenarios, the average rate of sea level rise during the 21st century very likely exceeds the 1961 to 2003 average rate (0.071 to 0.02 inches [0.18 ± 0.05 cm] yr^{-1}). Even if GHG concentrations were to be stabilized, sea level rise would continue for centuries due to the time scales associated with climate processes and feedbacks (IPCC, 2007d). Thermal expansion of ocean water contributes 70 to 75% of the central estimate for the rise in sea level for all scenarios (Meehl et al., 2007). Glaciers, ice caps, and the Greenland Ice Sheet are also projected to

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[10] According to IPCC terminology, “very likely” conveys a 90 to 99% probability of occurrence. . See Box 1.2 of the final TSD for a full description of IPCC’s uncertainty terms.

> add to sea level. The IPCC projects a range of sea level rise contributions from all glaciers, ice caps, and ice sheets between 1.6 to 9 inches (4 to 23 cm), not including the possibility of rapid dynamical changes. The Antarctic ice sheet is estimated to be a negative contributor to sea level rise over the next century under these assumptions (Meehl et al., 2007).

The van de Wal et al. (2008) and Joughin et al. (2008) studies referenced by commenters do not call into question any of the projections quoted above. Based on our review of these studies, we find that commenters misrepresent their narrowly focused conclusions.

The Joughin et al. (2008) study examined the flow rates of the Jakobshavn Isbrae glacier, several smaller marine terminating outlet glaciers, and a several-hundred kilometer-long stretch of surrounding ice sheet along the Western flank of the Greenland Ice Sheet in order to asses the effect of surface melt-water lubrication on ice flow. The study, which concentrates on one specific melt mechanism in one part of Greenland, finds that "seasonal melt's influence on ice flow is likely confined to those regions dominated by ice-sheet flow." The authors make no statements on the much broader topic of sea level rise projections, which were not the focus of their study. However, the study does mention recent melting speedups and their potential causes. It states:

> It is unlikely that recent outlet-glacier speedups (14, 19–21) were directly caused by increased surface meltwater lubrication during recent warmer summers. Instead, these large speedups were likely driven by processes that caused ice-front retreat and reduced back-stress (17, 19, 20, 22), such as declining sea-ice extent near calving fronts (18). The recent period of warmer summers (23) might also enhance ice front retreat through increased hydro-fracturing in water-filled crevasses near the calving front (18), which represents a process whereby surface meltwater influences glacial flow through means other than directly enhancing basal lubrication.

In regard to the specific melting mechanism the study focuses on, the authors note that "Our results thus far suggest that surface meltwater–enhanced lubrication likely will have a substantive but not catastrophic effect on the Greenland Ice Sheet's future evolution." They also note that ice loss on the ice sheet's western flank, which is comparatively free of outlet glaciers, is due largely to melt, and that "surface meltwater–induced speedup may influence large regions of the ice sheet in a warming climate."

The van de Wal et al. (2008) study presents ice velocity measurements from the major ablation area along the western margin of the Greenland ice sheet since 1991 and finds that a positive-feedback mechanism between melt rate and ice velocity "appears to be a seasonal process that may have only a limited effect on the response of the ice sheet to climate warming over the next decades." Like Joughin et al. (2009), the study focuses on a specific process (the interaction between meltwater production and ice velocity) and therefore provides very little support for the broad and sweeping views expressed by commenters regarding future sea level rise.

It is important to note that the SRES-based projections underlying the IPCC conclusion that the average rate of sea level rise during the 21st century will very likely exceed the 1961-2003 average rate explicitly exclude the possibility of rapid dynamic changes in the Greenland and WAIS ice sheets. Yet these possible dynamic changes are the precise topic the van de Wal et al. (2008) and Joughin et al. (2008) studies focus on, and the studies concentrate narrowly on a few specific mechanisms that have been proposed as potential contributors to dynamic changes. As discussed in the TSD [Section 6(b)], CCSP (2008a) found that inclusion of dynamic ice flow processes in models would likely lead to projections that substantially exceed the IPCC projections. The van de Wal et al. (2008) and Joughin et al. (2008) studies found that some of these processes may not have important contributions to glacial flow, but the studies acknowledge that other mechanisms may increase the rate of ice loss (and therefore sea level rise) in response to warming.

In summary, our review of the Joughin et al. (2008) and van de Wal et al. (2008) papers revealed that commenters' interpretation of these studies is not in accord with the nuanced and narrowly focused conclusions of the papers. On the basis of our review of the assessment literature, the referenced studies, and the TSD, we find that the TSD provides an accurate and sound summary of the current state of scientific knowledge about future sea level rise.

Finally, we refer the commenters to Section IV.B of the Findings, which describes the information on which the Administrator relied. The discussion of sea level rise in the Findings is fully consistent with the assessment literature conclusions summarized in the TSD.

Comment (4-68):
One commenter argues that the IPCC's most likely estimate of sea level rise is only 14 inches.

Response (4-68):
IPCC (2007) provided projections of future sea level rise in the form of ranges for each emission scenario. For example, for the projected range for SRES A1B was approximately 8 to 19 inches (0.21–0.48 meters). IPCC did not provide best estimates for individual scenarios or across all scenarios. To ensure consistency with the findings of the assessment literature, EPA used ranges for sea level rise projections in the TSD. Although the best estimate suggested by the commenter is within the range provided by IPCC, the commenter did not provide evidence as to how they reached the conclusion that the "most likely" estimate of sea level is 14 inches. We are not aware of any study that has reached this conclusion. Additionally, as discussed in response 4-67, the IPCC range does not include dynamic processes related to ice flow that could lead to future sea level rise in excess of the upper end of the IPCC estimate.

Comment (4-69):
One commenter (3394.1) cites Bamber et al. (2009) as a reference for the following statement: "[T]he TSD and the Proposed Endangerment Finding rely on outdated projections of future sea level rise. The most recent study assessing sea level rise modeling results concludes that previous estimates, including those calculated by the IPCC, are roughly double currently supportable sea level rise expectations."

Response (4-69):
We have reviewed the Bamber et al. (2009) study referenced by the commenter, which reassesses the potential contribution to eustatic (the global change due to water mass added to the oceans) and regional sea level from a rapid collapse of the West Antarctic Ice Sheet (WAIS). Our review revealed that the commenter's interpretation of the study is only slightly reflective of its actual findings.

Bamber et al. (2009) find that previous assessments have substantially overestimated the likely primary contribution of a disintegration of the WAIS, obtaining a value for the global, eustatic sea level rise contribution of about 3.3 meters, with important regional variations. Thus, the study suggests that the IPCC projections for sea level rise due to a collapse of the WAIS (which IPCC notes would occur over many centuries) may be somewhat too high, though we note that this remains an active area of research and that the study has not yet been incorporated into the assessment literature. However, the commenter's broad and sweeping statement that "previous estimates [of sea level rise], including those calculated by IPCC, are roughly double current supportable sea level rise expectations" has no basis in the results of the study. This statement could be read as implying that the study suggests that the IPCC sea level rise projections for this century or owing to other sources may be too high, when in fact it does nothing of the sort. Rather, Bamber et al. (2009) focus on the much narrower question of the potential contribution to sea level rise from an eventual collapse of the WAIS that would occur over a period of centuries. The study does not address near-term sea level rise projections and explicitly excludes mass losses from other sources such as Greenland, glaciers, and ice caps. In addition, the study finds that the maximum increase due to a collapse of the WAIS is concentrated along the Pacific and Atlantic seaboard of the United States, "where the value is about 25% greater than the global mean, even for a case of a partial collapse." Therefore, for the United States, the expected change from a collapse of the WAIS would, according to the study, be about 4.1 meters, or only about 18% less than the lower bound of the 5–6 meter estimate reported by IPCC (Meehl et al., 2007).

Comment (4-70):
Many commenters (e.g., 0362, 1672, 3383.1, 3424.1, 3455.1, 3574.1, 4171, 4184, 4249, 10809, 10838, 11342) state their support for the Findings, noting future rise in sea levels as one of the environmental effects of climate change.

Response (4-70):
We agree that climate change is causing sea level to rise. Please refer to Section IV.B of the Findings for a description of the Administrator's findings about projected sea level rise and why they provide support for the endangerment finding.

4.7 Abrupt Climate Change

Comment (4-71):
A number of commenters (e.g., 3291, 3722, 5846, 5858, 7037, 10394,11459) argue that there is little or no possibility that sea levels will rise by a large amount due to climate change, even over long-time scales. Several commenters focus their claims specifically on 20 feet of sea level rise, arguing that such an increase is entirely implausible. A book submitted or referenced by a number of commenters (Singer and Avery, 2008) makes this claim; it argues that there is not enough ice to trigger a 20 foot rise in sea level.

One commenter (3722) states that "many scientists have discredited predictions of widespread deglaciation, large sea level rises and resultant coastal flooding." Citing Ollier (2007), the commenter goes on to argue that "[c]atastrophic scenarios of sea inundation caused by collapsing ice sheets are predicated on models of hypothetical glaciers and ice sheets, and not those that actually exist in Greenland and Antarctica. As such, these conjectures are not premised in the physics of glaciers."

Response (4-71):
In light of these comments, we have reviewed the referenced materials, the assessment literature, and the TSD (extreme sea level rise is discussed in Section 6(f), Abrupt Climate Change and High-Impact Events). We find that the TSD properly summarizes the assessment literature regarding the state of the science on this issue and that the commenters do not provide any substantive evidence that the assessment literature has reached flawed conclusions. The TSD states:

> The rapid disintegration of the Greenland Ice Sheet (GIS), which would raise sea levels 23 feet (7 meters), is another commonly discussed abrupt change. Clark et al. (2008) report that observations demonstrate that it is extremely likely that the Greenland Ice Sheet is losing mass and that this loss has very likely been accelerating since the mid-

> 1990s. In the CCSP (2009c) report *Past Climate Variability and Change in the Arctic and at High Latitudes*, Alley et al. (2009) find a threshold for ice-sheet removal from sustained summertime warming of 9 F (5°C), with a range of uncertainties from 3.6 to 12.6 F (2° to 7°C). Meehl et al. (2007), in the IPCC report, suggest the complete melting of the GIS would only require sustained warming in the range of 3.4 to 8.3 F (1.9°C to 4.6°C) (relative to the pre-industrial temperatures) but suggest it would take many hundreds of years to complete.
>
> A collapse of the West Antarctic Ice Sheet (WAIS), which would raise seas 16 to 20 feet (5 to 6 meters), has been discussed as a low probability, high-impact response to global warming (NRC, 2002; Meehl et al., 2007). The weakening or collapse of ice shelves, caused by melting on the surface or by melting at the bottom by a warmer ocean, might contribute to a potential destabilization of the WAIS. Recent satellite and in situ observations of ice streams behind disintegrating ice shelves highlight some rapid reactions of ice sheet systems (Lemke et al., 2007). Clark et al. (2008) indicate that while ice is thickening over some higher elevation regions of Antarctica, substantial ice losses from West Antarctica and the Antarctic Peninsula are very likely occurring and that Antarctica is losing ice on the whole. Ice sheet models are only beginning to capture the small-scale dynamic processes that involve complicated interactions with the glacier bed and the ocean at the perimeter of the ice sheet (Meehl et al., 2007). These processes are not represented in the models used by the IPCC to project sea level rise. These models suggest Antarctica will gain mass due to increasing snowfall (although recent studies find no significant continent-wide trends in snow accumulation over the past several decades; Lemke et al., 2007), reducing sea level rise. But it is possible that acceleration of ice discharge could become dominant, causing a net positive contribution. Given these competing factors, there is presently no consensus on the long-term future of the WAIS or its contribution to sea level rise (Meehl et al., 2007).

We have reviewed the materials commenters referenced in support of their contention that the science summarized in the TSD is inadequate and find that these references are inconsistent with the vast majority of peer-reviewed literature on this topic. Singer and Avery (2008) has not been peer-reviewed or undergone a thorough scientific evaluation of its claims. Our review of this book revealed no basis in the peer-reviewed literature for Singer and Avery's claim that a sea level rise of 20 feet "*cannot* happen because there's not enough ice to trigger it."

Based on our review of the relevant literature, we find that the Ollier (2007) paper, which also did not undergo peer-review, is narrow and incomplete in its assessment of the issues when compared to the scope and breadth of the assessment literature. Although the Ollier (2007) paper is correct that temperature and present climate are not the only determinants of ice sheet flow and mass (other factors such as grounding-line deposition also have important effects), a clear conclusion from the body of peer-reviewed literature is that large-scale changes in climate, such as continued increases in global average temperature, would significantly affect ice sheet mass over time.

As the assessment conclusions summarized in the TSD make clear, the probability of a large-scale collapse of the Greenland ice sheet occurring within the next several centuries is low. We note, however, that the body of literature contains considerable evidence sea level *could* rise 20 feet within the next several centuries were such an event to occur.

Comment (4-72):

One commenter (3136.1) argues that "[i]n research published subsequent to the IPCC AR4, the threat of a very rapid sea level rise from a rapid deglaciation has been greatly downplayed." The commenter argues that IPCC did not include loss from dynamic flow processes in Greenland or Antarctica because "a basis in the published literature is lacking" and states that since the publication of the IPCC report, several studies (Joughin et al., 2008; van de Wal et al., 2008) have appeared in the scientific literature suggesting that glacial flow rate changes in Greenland may not be great cause for concern because warming should not lead to catastrophic ice discharge from increasing rates of glacial flow. The commenter concludes: "Results such as these provide added support of the modest IPCC estimates of 21st century sea level rise and greatly weaken the argument for disastrous sea level rise."

Several other commenters (e.g., 2898.1, 3323.1, 4003, 4041.1, 4932.1, 5158) similarly argue that the concern for warming temperatures causing Greenland and/or the WAIS to rapidly shed their ice has been diminished by new results indicating little evidence for the operation of such processes. One commenter (2898.1) cites Anandakrishnan et al. (2007); Alley et al. (2007); and Anderson (2007) as evidence that "[t]he WAIS is more stable than scientists had previously assumed."

Response (4-72):

We have reexamined the TSD and assessment literature in light of these comments and the referenced literature. On the basis of this review, we find that the findings of the studies referenced by commenters are largely consistent with the conclusions from the assessment literature that are summarized in the TSD.

As discussed in response 4-67, the Joughin et al. (2008) study referenced by one commenter (3136.1) finds that one set of glaciers along the Western flank of the Greenland Ice Sheet is relatively insensitive to one specific feedback mechanism (surface-meltwater enhanced lubrication). The study concludes:

> Surface meltwater–enhanced basal lubrication has been invoked previously as a feedback that would hasten the Greenland Ice Sheet's demise in a warming climate (*6*–*8*). Our results show that several fast-flowing outlet glaciers, including Jakobshavn Isbrae, are relatively insensitive to this process. Previously reported acceleration and near-doubling of speed of many outlet glaciers from other effects such as back-stress reduction are more than an order of magnitude greater (*14*, *19*–*21*) than the observed melt-induced acceleration. South of Jakobshavn, however, the ice sheet's western flank is comparatively free of outlet glaciers, and ice loss is largely due to melt. Numerical models appropriate to this type of sheet flow and that include a parameterization of surface meltwater–induced speedup predict 10 to 25% more ice loss in the 21st century than models without this feedback (*8*). Thus, surface meltwater–induced speedup may influence large regions of the ice sheet in a warming climate. Although our data provide the most comprehensive observations to date, more data are needed to quantify the relative importance of melt- and calving-front–induced changes in ice flow in controlling near-future ice-sheet mass balance. Our results thus far suggest that surface meltwater–enhanced lubrication likely will have a substantive but not catastrophic effect on the Greenland Ice Sheet's future evolution.

In addition to noting that surface melt-water enhanced lubrication likely will have a substantive effect, the authors note that other mechanisms of acceleration are still present and that surface meltwater-induced feedbacks are still applicable to other portions of the ice sheet.

As described in response 4-67, Van de Wal et al. (2008) presents ice velocity measurements from the major ablation area along the western margin of the Greenland ice sheet since 1991 and finds that a positive-feedback mechanism between melt rate and ice velocity "appears to be a seasonal process that may have only a limited effect on the response of the ice sheet to climate warming over the next decades." However, the authors also conclude that "Longer observational records with high temporal resolution in other ablation areas of the ice sheet are necessary to test the importance of the positive-feedback mechanism between melt rates and ice velocities. At present, we cannot conclude that this feedback is important. We do see a significant increase of the ablation rate, which is likely related to climate warming, but it remains to be seen if this is likely to be amplified by increasing annual ice velocities."

Based on our review of these studies, we conclude that neither the paper by Joughin et al. nor that by van de Wal et al. contradict the data that shows that the net mass balance of the Greenland ice sheet is currently negative, nor that there is acceleration of ablation and thinning of the ice sheets, nor that there is a temperature above which the Greenland ice sheet will be committed to disintegration.

Further, we find that other recent studies such as Mernild et al. (2009), Shepherd and Wingham (2007), and Rignot et al. (2008) provide additional analysis of the response of Greenland's ice sheet to warming. Rignot et al. conclude:

> We reconstructed the Greenland ice sheet total annual mass budget from 1958–2007. The ice sheet was losing mass during the warm period before the 1970s, was close to balance during the relatively cold 1970s and 1980s, and lost mass rapidly as climate got warmer in the 1990s and 2000s with no indication of a slow down. Hence, the temporal variability in mass balance is significant and closely follows climate fluctuations. Most likely, the ice sheet mass deficit in the 1925–1935 warm period was larger than in 1958. In the last 11 years, the total mass deficit tripled.

The study reveals that changes in glacier flow have an important influence on ice sheet mass and lends further support to the conclusions of the assessment reports as summarized in the TSD, including the statement from Clark et al. (2008) that "observations demonstrate that it is extremely likely that Greenland Ice Sheet is losing mass and that this loss has very likely been accelerating since the mid-1990s."

We have reviewed the papers submitted by commenter 2898.1 and find that these papers do not call into question the assessment literature conclusions on the WAIS as summarized in the TSD. The Alley (2007) paper finds that recent Antarctic changes cannot be attributed to sea level rise, thus "strengthening earlier interpretations that warming has driven ice-sheet mass loss." The paper additionally notes that the study shows that the similar behavior of various ice sheets with active sedimentary systems over time indicates common climatic forcing, "which demonstrably can have very large and rapid effects on ice sheets." The Anandakrishnan (2007) paper similarly focuses on the effects of sea level rise on glacial stability, and it does not directly address the effects of warming on ice-sheet mass loss. The papers suggest that the threat of sea level rise to ice sheet stability may not be as serious as once thought but do not assess other sources of instability such as surface and ocean warming.

Anderson (2007) discusses both the Alley (2007) and Anandakrishnan (2007) papers. He notes that both papers demonstrate that grounding-line (the juncture between the ice shelf and the part of the ice sheet that is thick enough to ground on the sea floor) deposition serves to stabilize ice streams, suggesting a decreased role for sea level in explaining past and current changes, but also that "[r]ecent changes in the ice sheet have raised concern that it may be retreating again." He adds that future research "should focus on other ice streams, especially those that currently display signs of instability, to get at the causes of this instability."

Shepherd and Wingham (2007) assess both the Antarctic and Greenland ice sheets with regard to glacial acceleration (increases in the flow rate into the sea). The authors note that satellite observations reveal "that glacier accelerations of 20 to 100% have occurred over the last decade." The study notes that there are a number of uncertainties involved in assessing whether the observed accelerations of particular ice streams and glaciers may be sustained, or even increase, in the future but clearly suggests that a general acceleration is possible. The abstract states the following:

> After a century of polar exploration, the past decade of satellite measurements has painted an altogether new picture of how Earth's ice sheets are changing. As global temperatures have risen, so have rates of snowfall, ice melting, and glacier flow. Although the balance between these opposing processes has varied considerably on a regional scale, data show that Antarctica and Greenland are each losing mass overall. Our best estimate of their [combined balance is about 125 gigatons per year of ice, enough to raise sea level by 0.35 millimeters per year. This is only a modest contribution to the present rate of sea level rise of 3.0 millimeters per year. However, much of the loss from Antarctica and Greenland is the result of the flow of ice to the ocean from ice streams and glaciers, which has accelerated over the past decade. In both continents, there are suspected triggers for the accelerated ice discharge—surface and ocean warming, respectively—and, over the course of the 21st century, these processes could rapidly counteract the snowfall gains predicted by present coupled climate models.

In summary, we find that the referenced literature does not contradict any of the principal conclusions on abrupt climate change from the assessment literature, as summarized in the TSD. On the basis of our review of the TSD, the assessment literature, the referenced studies, and other recent literature, we find that the TSD provides a reasonable and sound summary of the most current and compelling science on potential future changes in the Greenland and West Antarctic Ice Sheets.

Comment (4-73):

Two commenters (3394.1, 3427.1) claim that EPA conducted a "crystal ball inquiry" in terms of its discussion of abrupt climate change. A commenter (3394.1) further objects that if the Administrator is going to refer to such "high impact events" in the finding, then the TSD needs to evaluate the probability of such events. The commenter further argues that EPA's analysis is "little more than speculation," and that most of the historical abrupt climate change examples cited were due to natural variability and that therefore abrupt climate change that may occur in the future could be caused naturally as well.

Response (4-73):

With regard to potential abrupt change, the TSD summarizes the findings of the assessment literature regarding possible mechanisms for abrupt climate change. The TSD is clear that the potential for abrupt climate change is not well understood. In fact, it states: "Potential abrupt climate change implications in the United States are not addressed in Section 7 through 14 (the U.S. sectoral impacts) because they cannot be predicted with confidence, particularly for specific regions."

It does not follow, however, that there is no place for a summary of the science on abrupt climate change in the TSD, which appropriately reflects the level of certainty in the science. To the contrary, we find that this discussion is both reasonable and necessary. As the NRC (2002) states:

> …greenhouse warming and other human alterations of the Earth system may increase the possibility of large, abrupt, and unwelcome regional or global climatic events. The abrupt changes of the past are not fully explained yet, and climate models typically underestimate the size, speed, and extent of those changes. Hence, future abrupt changes cannot be predicted with confidence, and climate surprises are to be expected.

The TSD references natural causes of historic abrupt change events because until recently there were no anthropogenic forcings that were of the same magnitude as, for example, the orbital changes which have forced abrupt climate changes of the past. While it may be difficult to determine the probability of any given mode of abrupt change, given the historical record of other abrupt changes it would be surprising if a forcing of several Watts per meter square (W/m^2) did not lead to some such changes in the future. We note that the Administrator is not relying on the possibility of abrupt change in making her endangerment finding.

Comment (4-74):

Some commenters argue that EPA understated the risks of abrupt climate change. One commenter (2895) objects that because the TSD in Sections 7 through 14 do not account for abrupt change (in Section 6), the TSD is understating likely risks to health and welfare and has made a "very conservative" finding. One commenter (3500.1) provided a reference (Lenton et al., 2008) listing a number of tipping points in the earth system. Commenter (8015) worries that we do not yet know when dangerous feedback loops involving retreating ice or permafrost CH_4 releases will "trigger precipitous climate change which may lead to catastrophic conditions for billions of humans," and urges immediate corrective actions, bemoaning the delay of at least a decade since the science has been well understood.

Response (4-74):

Although we understand the commenters' concern that the TSD may be underestimating the probability and severity of a number of risks, the approach we have taken has been to rely on the scientific consensus as expressed throughout the major assessments. The TSD includes references to a number of the potential tipping points described in Lenton et al., such as changes in weather patterns and megadroughts, slowing of the meridional overturning circulation, disintegration of major ice sheets, interactions with the polar ozone holes, disappearance of summer sea ice, and release of CH_4 from clathrates and permafrost soils. Although these tipping points are of great concern, the TSD primarily summarizes the findings of the assessment literature for reasons discussed in the Findings and Volume 1 of this Response to Comments document, which for the most part do not yet make definitive statements about the probability of some of these events.

Comment (4-75):

Several commenters voice their support for the findings and express concern regarding the potential to reach a "tipping point" (e.g., 0276, 0731, 2895, 3500.1, 4817, 8015) in GHG concentrations. Specifically, they mention the potential to set off large-scale events such as the collapse of the Greenland ice sheet, significant CH_4 releases from thawing permafrost (e.g., 0742, 1520), and glacier melting leading to rising sea levels (4991, 9347). One commenter (9405) stated that extrapolating current trends into the future reveals the potential for "catastrophic harm."

Response (4-75);

We agree that climate change has the potential to cause large-scale changes such as a collapse of the Greenland ice sheet, though, as noted in the TSD, such a change would take many hundreds of years to complete. Please refer to Section 6(f) of the TSD for EPA's summary of the current scientific understanding of abrupt climate change and high-impact events as reflected in the assessment literature.

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