CCSP Synthesis and Assessment Report 3.1

Climate Models: An Assessment of Strengths and Limitations for User Applications

Response to Peer Review Comments

Reviewer: Kerry H. Cook

COMMENT: Pg. 5, line 9, that *models*
RESPONSE: The text was corrected.

COMMENT: Pg. 5., lines 7-11.
Climate Model Construction, first paragraph. The second and third sentences in this paragraph seem to me to gloss over a basic issue about the development of climate models. These sentences suggest that climate model development has been driven by the need for applications related to people, and this really isn't the case – historically. For example, there has been very little attention paid to "storminess" over the decades of GCM development since the models can't resolve storms. GCMs were interesting to construct because they taught us how the climate system works – their development has much more of a "basic science" motivation than this paragraph indicates. More recently, of course, development is being driven by the global warming problem.

RESPONSE: We agree that, historically, the motivations for climate model development were not applications related. The "Climate Model Construction" section, which begins on page 15 of the Public Review Draft (PRD), refers to current development motivations. The reviewer acknowledges this in the last sentence of the comment.

COMMENT: Pg. 5, lines 14-18. To say that a "good" climate model "must" accomplish these feats is not correct. Plenty of climate models – probably all of them, depending on the strictness of your measures of success – fail at most of these tasks.
RESPONSE: The "Climate Model Construction" section was significantly altered and the text was replaced with correct language and better descriptions.

COMMENT: Pg. 5., line 20. A more complete definition of "climate", carefully distinguishing it from weather, would be good here. People often need to understand how we can claim to have some skill in predicting climate change in 100 years or more when we can't produce very skillful seasonal or interannual forecasts.
RESPONSE: The text has been changed as suggested, on pages 15-17 of the PRD.

COMMENT: Pg. 6, lines 1-2 and 7. The tone of the piece is uneven. These lines give an example, with lines 1-2 seeming to be directed to other scientists (maybe a physicist, for example) and line 7 sounding like a middle-school text book (particularly "by scientists").
RESPONSE: We agree with the criticism. Improved and expanded text replacing this paragraph begins on line 9, page 16 of the PRD.

COMMENT: Pg. 7, line 7. Suggested rewording: … and the physical laws that govern the exchanges of mass and energy
RESPONSE: The suggested change was made.

COMMENT: Pg. 7, line 13, *the* primitive equations … with *the* hydrostatic
RESPONSE: The paragraph was rewritten. Improved text begins on line 1, page 25 of the PRD.

COMMENT: Pg. 9, lines 4 – 5: Cumulus *convection* … *is* …
RESPONSE: The term was changed to "cumulus convective transports" (line 23, page 22 of the PRD).

COMMENT: pg. 11, line 13, referred to *as* a …
RESPONSE: The sentence has been changed on line 21, page 26 of the PRD.

COMMENT: pg. 12, line, 9: delete *separate*
RESPONSE: That section was rewritten starting on page 21 of the PRD. The sentence referred to by the reviewer was removed.

COMMENT: pg. 17, line 26: define and use "OGCM", similar to AGCM
RESPONSE: OGCM is now defined on line 5, page 26 of the PRD.

COMMENT: pg. 28, lines 1-21. As I think is implied by an author's parenthetic note, these paragraphs are out of place and/or redundant with material at the beginning. There are some fresh thoughts here, though, they should not be lost.
RESPONSE: Response is included with the response to the next comment.

COMMENT: Pg. 28, lines 24-31. I like this paragraph and think it conveys something useful about how GCMers work and think.
RESPONSE: The section titled "Component coupling and coupled model evaluation" that began on page 27 and continues through page 28 of the Reviewer's Draft has been shortened and rewritten in the PRD. The new text begins on page 45. The questionable section and the text the reviewer commended were both removed. Nevertheless, an improved discussion of model tuning and evaluation gives significant insight into the model development process, which we believe was the point the reviewer was making. The subject of sensitivity is entirely contained in Chapter IV.

COMMENT: Pg. 47, lines, 5 and 9, and numerous other spots: Regional climate model applications should not be uniformly referred to as "downscaling" simulations, since this gives the impression that their only use is to provide more detail in conjunction with GCM simulations (as fancy interpolators, for example). This is one use of regional models, but they are also used to simulate climate and climate change independently of GCMs as well – in present day, future, and paleoclimate applications.

RESPONSE: The broader application of regional simulations to study climatic processes not resolved by atmosphere-ocean GCMs has been noted at the end of the first paragraph of Chapter III. (line 19, page 61 of the PRD).

COMMENT: Pg. 50, line 21-23. RCMs have been successfully run without convection parameterizations with grid spacing on the order of 5 km. This is noted on pg 51, lines 12-13, contradicting the pg 50, line 21-23 statement.

RESPONSE: The text has been altered to recognize that convection parameterization is not always used at the highest RCM resolutions. (line 30, page 65 pf the PRD).

COMMENT: Pg. 79, lines 1-2. messed up text

RESPONSE: Revised text discussing the ITCZ problem and precipitation errors starts at line 15, page 93 of the PRD.

General comments:

COMMENT: While the detailed comparison of the U.S. models is useful to provide depth, singling out 3 U.S. models (modeling groups, more accurately) does not give an accurate overview of the ability of GCMs in general. The U.S. models are all terrific and absolutely state-of-the-art, but GCM modeling is a global endeavor at this point – requiring international collaboration - and that could be more strongly related in the text.

RESPONSE: Chapter II of the PRD contains an expanded discussion of the CMIP3 class of models, including a more complete accounting of model improvement over time and a brief discussion of model metrics. We continue, however, to emphasize the three US models that participated in the CMIP3 coordinated experiments, because the report's audience is primarily based in the US.

COMMENT: It would be useful to strengthen the discussion throughout of how observations support modeling activity, with specific examples. For example, a statement that satellite observations are essential for validating models and understanding climate processes in remote land areas and over the oceans. Also, as global models evolve to finer grid spacings, and currently for regional model simulations, information about structure at the surface (soil moisture, vegetation, soil temperatures) is going to become increasing important – either for constraining the models or for validating. This will require both land-based and space-based observing systems. Another possible example concerns observations of the large-scale ocean circulation, e.g., the THC. How accurately is it known, including the features of its natural variability? In general, how does a scarcity of observations map onto model development and improvement?

RESPONSE: The emphasis of the report is on the characterization of how well current models simulate the climate, and most of Chapter V is devoted to comparisons of model results to observations. Chapter VI of the PRD has multiple examples of how improved process representation (cloud microphysics, soil biogeochemistry, ecosystem representations) in future models depends on the quality and availability of data.

Reviewer: Roberto Mechoso:

General Comments

COMMENT: The report focuses primarily on the physical climate models that were used for the most recent international Coupled Model Intercomparison Project's (CMIP) coordinated experiments (Meehl, et al., 2006), sponsored by the World Climate Research Programme (WCRP). Nevertheless, several other models are mentioned along the text. In my opinion, this limitation is too restrictive.

RESPONSE: The CMIP3 models are generally acknowledged to be well-tested state-of-the-science climate simulations codes. Unless a modeling system has gone through the CMIP3 set of experimental protocols, it would be difficult to assume much about their general suitability for a wide range of applications, which is the focus of this report. While other models may be mentioned in the report, these additions were for clarity or to better expose a point. We acknowledge that other models are used for certain applications, but this report deals with the ability of models in general to address many applications.

COMMENT: What I have in front of me is a long text in a very bland format, similar to the one used for a scientifically oriented readership. A good technical editor can make the text much more readable by highlighting key sentences and inserting attractive figures. The process will lead to an evaluation of the report, as outstanding aspects become isolated from the large amount of information available.

In the same way, the text consistency can be greatly improved. The text refers to a different number of models in different parts. Also, the models selected for discussion are referred to in different ways: American models, leading models, US AOGCMs…One can recognize pieces of other reports in the overall text. This roughness can be easily smoothed out.

RESPONSE: The report has been extensively reorganized and major sections have been rewritten. The information is now more evenly and more logically presented, and we have tried to reduce jargon where possible and attain consistency where it is retained.

COMMENT: I find many statements without an adequate reference. It is understood that if all references are included in a text dealing with so many diverse topics, the list may become longer than the paper itself. Nevertheless, in some cases the need of a reference is obvious. (For example, the statement of the anti-correlation between rainfall over the Sahel and the Amazon requires a reference.) There is a very nice discussion on climate sensitivity. The list of strengths and weaknesses of current models is long and there is always room for one more.

RESPONSE: The reviewers' draft contained many missing references, which was regrettable. Following review, multiple proofreads were performed to identify sections where references were missing, as well as sections that required additional documentation. The section referring to the Sahel-Amazon relationship was removed from the report to address concerns of another reviewer (Meehl).

COMMENT: My version does not have an executive summary.

RESPONSE: An executive summary was included in the current version.

COMMENT: The report focuses primarily on climate models that were used for the most recent international Coupled Model Intercomparison Project's (CMIP). Thus, the emphasis is mostly on the coupled atmosphere-ocean system. The decision to restrict the scope of the report is a good one, and the way it is done is justified at the present time. I am sure that the authors are expecting some challenges to their decision, so here they go in the form of questions.

a. What are the fundamental differences in the modeling approaches of the different institutions? Are some efforts more innovative than others? Can we get the feeling of an integrated national approach to climate modeling and simulation?

b. Why aren't any university lead efforts mentioned? It is acknowledged that university groups played a leading role in the development of climate models. What is happening nowadays? Are there universities producing new modeling paradigms, and if not, why not? (I think they are!) Where is instruction on climate modeling happening?

c. Why isn't there a section on the stratosphere? The Antarctic Ozone Hole is a success story since science motivated an international agreement. The role of climate models in this problem has not been, to my knowledge, properly discussed. Obviously the model could not predict the feature due to the lack of the proper chemistry. The problem is not completely gone; can climate models help to understand why?

d. The access by users of a computational infrastructure to run large codes can be briefly reviewed. This is, of course, in the understanding that work with GCMs is not confined to the large national laboratories. Even if this were the case, are national laboratories satisfied with their computer facilities?

e. The efforts lead by NASA and NCAR to create a software infrastructure to facilitate the use of climate models can be mentioned. The Earth System Modeling Framework (ESMF) promises to enhance the use of climate models.

RESPONSE: Although the reviewer asks many pertinent questions that are relevant to the science of climate modeling and its further development, particularly in the United States, we feel that most of his questions are beyond the scope of the present document, which is primarily a review of the current state-of-the-science of current models as a guide to using the output from them for applications. As to why there are differences among the models, we have tried to cover that at the appropriate level in Chapter 2, without going into too much detail. As the reviewer correctly notes, there are university and other research groups that are active in the climate modeling enterprise. In the scoping of the document, we intentionally concentrated on the CMIP3 class of models, particularly those based in the United States, because the results from the CMIP3 simulations are the source of climate model output for nearly all recently published climate model applications.

Chapter I. Introduction

COMMENT: I do not have any comments of note on this chapter. The text gives a feeling for a more or less monotonic improvement of models from the point of view of science, in a way that increased complexity results almost exclusively from increased computer power. I believe that mentioning just one of the milestones in climate modeling (e.g., the Phillips two-layer experiment) will enhance the reader's appreciation of the science issues.

RESPONSE: We improved the chapter, by adding some detail and improving the flow of the historical development section. We appreciate the reviewer's concern for the appropriate historical context and considered including the Phillips' simulation referred to above. Nevertheless, it was considered too much of a detail from a climate modeling perspective, although it was major accomplishment in atmospheric general circulation modeling.

Chapter II. Description of Global Climate System Models

Atmospheric general circulation models

COMMENT: The descriptions in this chapter are authoritative. However, they fall in a middle ground that is of little use to either the general reader and the specialist. The clearest example is in the paragraph on cumulus convection. The expert reader will learn little from the description of the schemes. For the non-expert, a mention of the quasi-equilibrium assumption without a minimum context will be meaningless.

Why is it that the majority of AGCMs use variants of the Arakawa-Schubert parameterization? This magnificent accomplishment is almost 30 year old; what has happened in the meantime? Is this an issue that ought to be brought up in this review?

RESPONSE: We have substantially rewritten Chapter 2 to make the descriptions more readable, useful and at an even level of detail. We believe a detailed discussion of the Arakawa-Schubert parameterization is beyond the scope of the document, and multiple papers are referenced should a reader desire further information. The text beginning with the last paragraph of page 22 of the Public Review Draft was revised from the original to: (1) remove some of the jargons such as quasi-equilibriums, and (2) give a more complete discussion of why the mass flux scheme has prevailed. This section is intended for readers who do not have a background in climate modeling, but with sufficient science knowledge to get an appreciation of the constructs of atmospheric models.

Ocean general circulation models

COMMENT: The text refers to the relatively coarse horizontal resolution of the models. It is indicated that "eddy scales" are parameterized. I think it is important to clarify that these scales do not correspond to the turbulent eddies that are parameterized in AGCMs. Contemporary OGCMs do not resolve mesoscale eddies, which can be originated by baroclinic-barotropic instabilities of ocean currents. These can play an important role in closing the ocean mixed layer budget by providing shoreward heat and material transport that balance the upwelling supply of cold water and the air-sea heat exchange. There are also standing eddies associated with alongshore coastline and bathymetric irregularities. The difficulties in closing the budgets may be key in many places, such as the eastern part of the tropical oceans.

RESPONSE: We agree with the criticism. The expanded discussion on page 31 of the Public Review Draft addresses these concerns.

Evaluation of AGCMs and OGCMs

COMMENT: This section is one paragraph long and is not balanced with the others in the report. Perhaps it could be merged with the longer discussion in Chapter V.

RESPONSE: We agree with the criticism and have significantly expanded the discussion of model evaluation in Chapter 2 in several places, most notably in a section on coupled model evaluation and metrics on pages 53-57 of the Public Review Draft.

Land Surface Models

COMMENT: This is a straightforward description of the different aspects of land surface modeling.

I find intriguing that "PILPS has lead to a better agreement among land models". Is it implied that the models were basically the same except for "tunable" parameters? The statement that "The latest generation of land surface models exhibit relatively smaller differences compared to previous generations" reinforces this impression. Are there major differences between land surface models?

RESPONSE: Current land models reflect different pathways followed for adding processes and increasing realism, so they differ by more than tunable parameters. This factor is now noted in the third paragraph on page 33 of the Public Review Draft, where reference is also made to the variety of land models used by current GCMs.

Sea Ice Models, including parameterizations and evaluation

COMMENT: The two dominant paradigms in sea ice modeling are discussed here practically side by side. This is a useful strategy.

RESPONSE: None required

Component coupling and coupled model simulations

COMMENT: This section includes the development plans at the 3 US groups that contributed to the 4th Assessment of the IPCC. It is good to find the plans in one place, but is unclear whether this compilation adds to the information already on the institutions web sites.

RESPONSE: We believed the information would be useful to the audience of the report, who we anticipate will be mostly US based readers who may be unfamiliar with the complexity of the model development process.

Reductive vs holistic evaluation of models

COMMENT: This section is very different from the others in terms of scope and style. The speculative style seems to be at odds with the matter-of-fact style in the remainder of the text. I gather that the concepts to be transmitted are three. First, ensemble simulations must be performed in order to consider the spread and characteristics of variability of the individual realizations. Second, our "confidence in its explanatory and predictive power of climate models grows based on their ability to simulate many aspects of the climate system simultaneously with the same set of physically based rules." Third, one cannot "tune" the model for one region of the world since all regions are simulated. Perhaps this can be done very efficiently in a few sentences.

RESPONSE: This section was completely rewritten and integrated into the model evaluation section (page 53 of the PRD). It illuminates some of the tradeoffs required in climate model development that are mostly unknown outside of the modeling community. We believe that this information is important to those who my apply climate models or use their results.

Chapter III – The added values of regional climate simulations

Types of downscaling simulations

COMMENT: I liked this section; it brings up many of the concerns on the topic and that are not easy to find in a single source.

There are a couple of spots that I found to be rough. In reference to the different performance of parameterizations in global and regional models, it is stated, "This factor is part of a larger issue, that parameterizations may have regime dependence, performing better for some conditions than others" (page 51). I can understand dependence on grid size, but I am not quite clear on different physical regimes for the same grid size. Or, doesn't the difference sensitivity of parameterizations to physical process have an impact in all grid sizes and the impact becomes exaggerated for some grid sizes?

RESPONSE: The issue of regime dependence was discussed in the examples of convective parameterizations immediately following the quoted statement. How regime dependence might change with resolution has not been explored in the literature.

Chapter IV– Model Climate Sensitivity

COMMENT: This is a very important chapter, and I believe that the job was well done.

RESPONSE: No response required

Chapter V – Model simulation of major climate features

Mean climate

COMMENT: This section has a long paragraph on the "double ITCZ" problem without a single reference. This is an important problem of high relevance to climate simulation and prediction. The links to the model difficulties with ENSO are evident.

The last paragraph of the section is that "AOGCMs generally simulate large-scale mean climate with considerable accuracy, but the models are not reliable for aspects of mean climate in some regions, especially precipitation." The last paragraph at the end of a long section will attracts a lot of attention from the readers, and requires more elaboration and an attempt to synthesis.

RESPONSE: We have revised the mean climate section, but left the last paragraph intact without expansion. The section is concise, so an extensive summary would be redundant. The short paragraph is for transition.

Monsoons

COMMENT: A more current view describes monsoons as involving both atmosphere and oceans. The presentation here is more traditional and looks at the atmosphere as reacting to changes in different time scales.

RESPONSE: We modified our description of the monsoon to focus upon the seasonal wind reversal, which places less importance upon the ocean, implicitly emphasizing the role of the atmosphere. Our presentation here is limited to the seasonal cycle, because this time scale has been emphasized by the model diagnoses.

COMMENT: A reference is needed on the processes that limit the extent of the monsoons. Are the authors referring to the ventilation paradigm?

RESPONSE: We have added a citation to Prive and Plumb (*J. Atmos. Sci.,* in press), a modeling study that discusses the advection of maritime air to feed convection over land.

COMMENT: It seems to me that one basic problem in monsoon simulation is not addressed, and that this problem poses serious questions on whether the climate model monsoons are proxies of reality. Monsoons comprise processes at the planetary, continental and meso scales. Among the latter are the "low-level jets". These differ in the monsoons: 1) The Somali Jet, which flows in summer at all times, 2) the South American Low Level Jet, which flows along the lee of the Andes during the entire year, and 3) the Great Plains Low Level Jet, which flows in North America at night during the warm season. These mesoscale features are captured poorly by global GCMs. The associated problem is that water advection is underestimated. If simulated precipitation is realistic, then local processes such as evaporation have to be exaggerated. Consistently, the role of soil processes may be over-emphasized. Can monsoon projections be trusted in view of these uncertainties in the water budget?

RESPONSE: We have added a brief discussion of unresolved spatial scales in the last paragraph.

COMMENT: Another problem that is attracting a lot of attention is the GCM difficulties in simulating the diurnal cycle and its variability in monsoon regions. It has become clear that the peak precipitation amplitude is too early in the day. This feature is likely associated with the PBL parameterization, which receives little attention in the text.

RESPONSE: While this is an important issue for agriculture (with implications for transpiration and runoff), we are currently aware of no articles that evaluate the diurnal cycle of the recent IPCC models. We acknowledge this point in the last paragraph.

COMMENT: Monsoon researchers have recognized that tropical cyclones contribute significantly to precipitation, primarily in the North American monsoon. AGCMs mentioned in the report cannot resolve such features, but others are claiming that they do it to some extent. Any opinions on this?

RESPONSE: We acknowledge this point in the discussion of subgrid transports in the last paragraph.

Polar Climates

COMMENT: Add "in the polar regions" before the reference to Uotila et al. (2007).

RESPONSE: The text is now included.

COMMENT: Please clarify in which way "stable boundary layers remain an important area for model improvement." There is little discussion of PBL in the report and this may be a place where this limitation can be at least partially addressed.

RESPONSE: The greater difficulty in simulating the stable PBL versus the unstable PBL seems to be well known, which is the basis for this statement.

COMMENT: The well-know problem of the "cold lower stratosphere of GCMs" receives little or no attention. This affects the zonal wind and planetary wave behavior, and hence low-frequency variability. The paper by Pawson et al (BAMS 2000) addresses this problem.

RESPONSE: Most CMIP3 models do a poor job in stratospheric simulation. This is briefly described on page 108 of the Public Review Draft.

COMMENT: Please clarify what is meant by "Because both the northern and southern polar regions are within circumpolar atmospheric circulations, their synoptic coupling with other regions is more limited than is the case with midlatitude regions embedded in the westerlies".

RESPONSE: We agree that there were problems with the original text. There is a much better description of polar circulations and annular modes in the Public Review Draft on page 6, and in a section beginning on page 107.

Modes of variability

COMMENT: I believe that the section on El Niño-Southern Oscillation (ENSO) must be adjusted a little since it appears to be originally intended to discuss many more (15) models than the ones selected for this report. We read a very important statement: "We find that even among the models with the most realistic simulation of ENSO and seasonal variability there is no consensus on the anticipated change in climate within the tropical Pacific." (Presumably the realistic ENSOs are those obtained for current climate conditions.) This is difficult to justify by inspection of just the three models that selected at the beginning.

RESPONSE: We note in the revised version that the conclusions about projected ENSO changes are based on a worldwide suite of models, and that the American models are cited simply to illustrate the central scientific issues. We note that the most realistic models are identified based upon their simulation of the present-day climate.

COMMENT: I am unclear on the argument about the upwelling "dilution" associated with the coarse grid spacing of CGCMs. According to the argument, the dilution limits the amplitude of resulting ocean temperature fluctuations. A current hypothesis is that the most important aspect of coarse horizontal resolution is the inability to resolve mesoscale ocean eddies that result from the baroclinic-barotropic instability along the upwelling front. The mesoscale eddies transport cold and fresh water off shore, thus extending the effect of upwelling in a scale far larger than the grid size.

RESPONSE: We have tried to explain more clearly how coarse resolution reduces the cooling effect of water rising to the surface at the equator. To be sure, mesoscale eddy transports will also be affected by the coarse resolution, although we are not aware of a study that compares this effect with that of unrealistically low upwelling.

COMMENT: In regard to climate prediction, what is the relative skill of physical models based on coupled GCMs in relation to simpler dynamical models and to statistical models? This is an important issue, although the report aims to time scales longer than the interannual.

RESPONSE: As the reviewer implies, these 'simpler dynamical models and statistical models' are generally intended for prediction on interannual (and shorter) time scales. In contrast, this report focuses on the ocean-atmosphere coupled models that are used for multidecadal and centennial projections of climate over the entire globe. Comparing the skill of these two classes of models is difficult. The global coupled models are not tuned for interannual prediction, but they are intended to predict slow, multidecadal changes in the mean state that are important for variability and that might be held constant in the simpler class. In response to the reviewer's comment, we simply note the distinction between these two classes of models.

Extreme events

COMMENT: "Extreme events" here refers to largest simulated values of precipitation or surface temperature. The limitations of current GCMs in this area are so clear (i.e. inability to simulate tropical cyclones, at least in climate simulations) that the text can be trimmed to emphasize issues of consensus on the information provided. Several researchers have already evaluated model performance in the context of "extreme events". I would like to see a discussion on the usefulness of these studies in reference to 1) climate science, 2) model performance.

This is actually attempted in parts of the text. For example, the issue that thunderstorms are responsible for many intense events is raised and could be discussed further. It is mentioned that this is related to the parameterization of convection, which is only one aspect of the problem.

RESPONSE: Although the limitations of GCMs are indeed clear to modelers and many other climate scientists, they are not the target audience of this report and thus we feel that it is important to elaborate on this issue. We are unsure how to respond to the rest of this comment. We raise the issues of resolution and convective parameterizations; these have been the subject of research studies as we point out. We are unclear as to what other "aspects of the problem" is the reviewer referring. We note that CCSP Synthesis and Assessment Report 3.3 evaluates our understanding of extreme events in detail, including current capability for simulating extremes and their changes with climate change.

Chapter VI – Future Model Development

COMMENT: The description of CRMs and their future reads well. Maybe it gives the impression that these models are more ready for climate studies than they actually are. Higher resolution changes the parameterization problem, but it doesn't make it necessarily easier. The full effect of increased model resolution provided by CRMs will be experienced when comparable or even higher resolution is also available in the boundary conditions, which must be provided by other models.

There is another paradigm for multiscale problems that will be likely attempted in the next decade. This is the nesting of coupled regional models of the atmosphere and the ocean within global coupled GCMs. The difficulties in nesting regional and global models are discussed in Chapter III of the report. However, some of those difficulties may be reduced in regions that are key to the climate system, and yet interactions with other regions at the synoptic scale are not intense. I am referring to the eastern part of the tropical oceans, where coupled GCMs fail with the stratocumulus and their radiative effects. It seems to me that, in the near future, there will be a strong interest in coupling regional models of the atmosphere-ocean system. Some of the work has started, but the full potential of the approach remains to be evaluated.

RESPONSE: The reviewer's points are well taken. This now is covered in the discussion of CRMs that begins on page 161 of the Public Review Draft.

Reviewer: Gerald Meehl

Authors' Note: The version of the Reviewers' Draft reviewed by Dr. Meehl did not have page or line numbers. His page references are based on a MS Word version of the document, which has slightly altered formatting, and therefore page numbers, from the PDF version that was created from that MS Word document. We appreciate Dr. Meehl's extra effort in working with the earlier version.

General comment

COMMENT: The report is a quite thorough overview of the state of current climate modeling. In fact there is probably a bit too much text book type material that could be trimmed. There is some duplication of material (e.g. ENSO is described in two different places). With the exception of the section that had implications for ethical practices by modeling groups that included speculation and hearsay that modeling groups essentially cheat by tuning equilibrium climate sensitivity, I found the draft to be an otherwise high quality and comprehensive review of climate modeling.
RESPONSE: In the revision, we have evened the presentation of the different topics and provided a consistent level of detail for the intended audience. The ENSO information was consolidated and begins on line 24, page 143 of the Public Review Draft (PRD). The sections on model evaluation and tuning were rewritten and the questionable text removed. The revised text begins on line 6, page 52 and continues through page 58 of the PRD.

Specific comments:

COMMENT:P. 7, top: There is a statement here that "typical AGCMs have spatial resolution of 200 kilometers in the horizontal and 20 levels…" This certainly isn't "typical" of current AGCMs used in coupled climate models. The current crop is closer to 150 km with about 30 levels (more details are given on the PCMDI CMIP web site).
RESPONSE: We do not consider "typical" to refer to a strict number. Of the three US models, only the CCSM used relatively high resolution of T85 and L26. The GISS group used 4 degrees by 5 degrees with 15 levels; GFDL used 2.5 degrees by 2 degrees with 24 levels. Most models in other countries also have resolutions coarser than the CCSM model, with the notable exception of the Japanese groups. We therefore retained the original wording.

COMMENT:P. 8, top: Perhaps it could be mentioned that very recent work on CRMs will be covered later in the report.
RESPONSE: This is a good suggestion. We revised the text as recommended in an early revision after receiving the review. The reference was subsequently deleted inadvertently in a later edit and will be restored in the final draft.

COMMENT:P. 10, bottom: It is stated here that these ocean models have resolution of "about 1/3 of a degree at the equator". However, this is not a complete portrayal, and it should be mentioned that usually these models have increasing resolution in the equatorial tropics usually between about 5N and 5S
RESPONSE: A sentence was added at line 29, page 26 of the PRD that corrects this omission.

COMMENT:P. 18, middle: When talking about the "bucket", it should be mentioned that the so-called bucket is actually meant to represent a physical quantity, namely field capacity of the soil
RESPONSE: The definition was added in the paragraph that begins on line 11, page 43 of the PRD.

COMMENT:P. 26, bottom: This should read "IPCC Fourth Assessment Report (AR4)"
RESPONSE: The omission of the word "Report", in the sentence, unfortunately, remains in the current PRD on page 45. This will be corrected in the next version.

COMMENT:P. 27, bottom: This discussion is at best inaccurate in implying that modeling groups "engineer" a particular value of climate sensitivity. The particularly regrettable sentence is: "Especially if one is willing to compromise on some measures of fitness, one can control the models' sensitivity to some extent (ref to Hadley center)". No reputable modeling group I am aware of does this. In fact, this discussion of modeling groups that "hold various views on the most likely value of climate sensitivity, but rarely with much conviction [sic]" is outdated given the analysis of equilibrium climate sensitivity in Ch. 10 of the IPCC AR4 where a best estimate of actual climate sensitivity is 3.0C, with a likely range of 2.0 to 4.5C. Modeling groups end up with climate sensitivity of their model at the end of their model development process. To imply that somehow groups tune their climate sensitivity at the outset is inaccurate, and, to the best of my knowledge, is simply not true. This falls into the category of speculation and is not appropriate in a CCSP report. In fact, what is implied on p. 27 (that modeling groups "cheat") is directly refuted by description of an actual model development process on P. 29 where indeed climate sensitivity was an outcome of model development, not an a priori goal. I suggest the authors avoid speculation on model developers' ethics, and stick to a discussion of the facts regarding current assessment of climate sensitivity as given, for example, in the IPCC AR4.
RESPONSE: This section has been revised and the somewhat provocative language removed. The section titled "Component coupling and coupled model evaluation" that began on page 27 and continues through page 28 of the Reviewer's Draft has been shortened and rewritten in the PRD. The new text begins on page 45. An improved discussion of model tuning and evaluation gives significant insight into the model development process. The subject of sensitivity is entirely contained in Chapter IV.

COMMENT:P. 30, near bottom: The authors use the term “transient climate sensitivity”. This is incorrect. The actual term in common usage (see the TAR and the AR4) is “transient climate response”, or TCR.
RESPONSE: The term “transient climate response” is used in the sensitivity section that begins on page 72. The text cited by the reviewer was contributed by GFDL and was not changed in the PDR (line 6 page 48), as it is a description of their model development process. We will verify with the contributors for the appropriate language before the next draft.

COMMENT:P. 32-37: This section, titled “reductive vs. holistic evaluation of models” sits uneasily in this report. I suggest it be revised to reflect current usage of terminology and common practice. What is actually described here, more or less, are model sensitivity experiments, and this term is commonly used in the field. In fact, this section is overly long and could be reduced by at least a factor of two. A simple discussion of the methodology of sensitivity experiments where various factors are altered in systematic ways to assess model response and the role of physical processes could be summarized in a page or two. A lot of this arcane discussion complicates a fairly simple procedure commonly used to study processes and responses in climate models. Also, value judgments such as “hidden behind the surface of this seemingly unremarkable time series is the profound imprint of these variations on economies and societies, in this case especially the stark human suffering associated with the drought period in the 70s and 80s” strikes me as inappropriate in a CCSP report on climate science. Additionally, the discussion at the bottom of p. 36 confuses model evaluation with model analysis that occurs after model development.

RESPONSE: This section was completely rewritten and integrated into the model evaluation section (page 53 of the PRD). It illuminates some of the tradeoffs required in climate model development that are mostly unknown outside of the modeling community. We believe that this information is important to those who my apply climate models or use their results.

COMMENT:P. 41, section titled “idealized climate simulations”: I suggest this section be revised such that it reflects a more appropriate title for this section, namely “climate model response metrics”. Then a simple discussion of the two main metrics, namely equilibrium climate sensitivity and transient climate response, would then follow.
RESPONSE: We appreciate the reviewer’s comment, but did not make the suggested change. The simulations are idealized, and we believe the current title is appropriate. Furthermore, we have included a discussion on model metrics in the model evaluation section, and suggested title might be confusing to some readers.

COMMENT:P. 42, near bottom: The authors have used a word here that is not common usage to my knowledge: "paleocalibrate". Such inventions should be discouraged in a CCSP report that reflects current practice and terminology, unless use of this word has slipped past me. If it is in common usage, perhaps the authors could provide a few substantiating references. In fact, this section should stress that paleoclimate simulations are an important part of model evaluation, since it is a severe test of a climate model to be able to simulate a past climate accurately.
RESPONSE: We appreciate the reviewer's perspective, however, we believe this is a question of style. The word is used in quotation marks, which implies that it is not a commonly used term. Its use is appropriate when used in the context of the paragraph (line 14 page 60 of the PRD). We will reconsider this point again in the next edit.

COMMENT:P. 43, near top: In a discussion of "numerical downscaling", the method of statistical downscaling should be mentioned
RESPONSE: This has been noted in the first paragraph of Chapter III (line 7, page 61of the PRD), where we introduce statistical downscaling as an alternative to downscaling by numerical simulation.

COMMENT:P. 43, about half way down: It is stated here that RCMs "require lateral boundary conditions from observations". However, the application being discussed here is when RCMs are embedded in AOGCMs, in which case the RCMs require lateral boundary conditions from the global model in which they are embedded
RESPONSE: Simulation with observation-based boundary conditions is important for segregating intrinsic RCM error from errors supplied by a GCM through the lateral boundary conditions. The second paragraph of Chatper III was modified to include this point (line 23, page 61 of the PRD).

COMMENT:P. 44, top: An important recent modeling study with a global 20 km model should be described here: Oouchi, K., J. Yoshimura, H. Yoshimura, R. Mizuta, S. Kusunoki, and A. Noda, 2006: Tropical cyclone climatology in a global-warming climate as simulated in a 20km-mesh global atmospheric model: Frequency and wind intensity analyses. J. Met. Soc. Japan, 84, 259-276.
RESPONSE: On line 26, page 66 of the PRD, the citation was added in the discussion about the importance of targeting mesoscale phenomena for regional simulation.

COMMENT:P. 52: Somewhere here the authors should discuss the prospects and obstacles involved with two-way nesting with an RCM embedded in an AOGCM, with the AOGCM forcing the RCM, and the RCM giving information back to the AOGCM, and so on.
RESPONSE: This area of work is still very exploratory. We note that some have been attempting it in the second paragraph of Chapter III (line 23, page 61 of the PRD). Because this work has been so limited, we do not feel it warrants extensive discussion beyond this point in this document.

COMMENT:P. 56, near top: The authors err here in not using current terminology. They use the term “equilibrium warming”, but the TAR and AR4, in assessing the literature on this topic for the past 10 years or so, use the term “equilibrium climate sensitivity”. This CCSP report should be consistent with that usage and change “equilibrium warming” to “equilibrium climate sensitivity” everywhere in this report.
RESPONSE: The term “equilibrium warming” is explicitly defined on page 73 of the PRD and is used consistently in the text that follows. We will reconsider the terminology in the next version.

COMMENT:p. 56, middle: The authors need to include the recent assessment of equilibrium climate sensitivity from the IPCC AR4, Ch. 10.
RESPONSE: A reference to the range of sensitivity found in the CMIP3 archive was added in the paragraph that begins on line 1 page 75 of the PRD. The AR4 report was not available for citation at the time the section was rewritten, but is now. An appropriate citation will be added in the next revision.

COMMENT:P. 59-60-61-62: See comment immediately above; this entire discussion needs to be updated and replaced given the AR4 assessment of equilibrium climate sensitivity following multiple lines of evidence from a host of models and observational studies (see AR4, Ch. 10)
RESPONSE: As noted above, the AR4 report was not available for citation at the time the section was rewritten, but is now. An appropriate discussion and citation will be added in the next revision.

COMMENT:P. 65 and elsewhere: This draft perpetuates terminology that the WGCM is trying to correct, and I suggest the authors follow their request to call the multi-model dataset at PCMDI the “CMIP3 multi-model dataset assessed in the IPCC AR4”, in place of the “AR4 coupled models” (here and throughout the report)
RESPONSE: We have attempted to change the description of the multi-model dataset from AR4 to CMIP3 throughout the report, although a few references to AR4 remain. These will be corrected and the language will be consistent in the next draft.

COMMENT:P. 66, top: Somewhere here the authors should discuss results from a major project to assess cloud forcing called CFMIP (e.g. Webb M.J., C.A. Senior, D.M.H. Sexton, W.J. Ingram, K.D. Williams, M.A. Ringer, B.J. McAvaney, R. Colman, B.J. Soden, R. Gudgel, T. Knutson, S. Emori, T. Ogura, Y. Tsushima, N. Andronova, B. Li, I. Musat, S. Bony and K.E. Taylor, 2006: On the contribution of local feedback mechanisms to the range of climate sensitivity in two GCM ensembles. *Clim. Dyn.* **27** (1): 17-38 doi:10.1007/s00382-006-0111-2.)
RESPONSE: We agree. The cloud feedback discussion was revised and appropriate references added, stating at line 8, page 78 of the PRD.

COMMENT:P. 66, middle: The authors should recognize that black carbon aerosols can also absorb solar radiation
RESPONSE: The specific subject of black carbon aerosols was not included in the report because its inclusion is inconsistent in the CMIP3 models. Most of the discussions of anthropogenic aerosols throughout the report recognize the uncertainties surrounding their representations and impacts.

COMMENT:P. 72, near bottom: Here is yet another variation, "PCMDI/AR4 simulations"; they should be called the "CMIP3 multi-model dataset assessed in the IPCC AR4"
RESPONSE: We have attempted to change the description of the multi-model dataset from AR4 to CMIP3 throughout the report, although a few references to AR4 remain. These will be corrected and the language will be consistent in the next draft.

COMMENT:P. 80: Since this is a U.S. CCSP report, it is odd that the authors use a somewhat dated result from the U.K here. Perhaps a more appropriate figure would be a more recent one from a U.S. model: Meehl, G. A., W. M. Washington, C. Amman, J. M. Arblaster, T. M. L. Wigley, and C. Tebaldi, 2004: Combinations of natural and anthropogenic forcings and 20th century climate. *Journal of Climate,* **17**, 3721–3727; see Fig. 2d.
RESPONSE: The figure mentioned has been removed and replaced with results from twentieth century simulations from the three US CMIP3 models (section starts at line 15, page 99 of the PRD). We have removed the attribution of climate change discussion.

COMMENT:P. 81, top: The authors should cite the attribution results by region shown in the IPCC AR4 ch. 9 which is the most recent and complete assessment of this topic
RESPONSE: The attribution discussion was removed because it was deemed unnecessary to the CCSP report objectives. As the reviewer notes, this topic is covered extensively in the IPCC AR4.

COMMENT:P. 86: Here is the first ENSO discussion…
RESPONSE: The first discussion was limited to trends in SST within various regions. The ENSO information was consolidated and begins on line 24, page 143 of the PRD.

COMMENT:P. 97: The section on monsoons includes way too much text book material, in my opinion. The authors should concentrate the text on the topic at hand, climate model simulations. This would shorten this section by at least a factor of two or more.
RESPONSE: In the revised section that begins on page 127 of the PRD, we have removed some of the opening pedagogical material, but we hope the remainder will smooth the way for readers whose expertise is in policy and applications, even if climate modelers find it unnecessary. The text book material is intended to define the monsoon and provide a context for the climate model diagnoses. In the revised version, we have tried to make more apparent the relation of the pedagogical discussion to the climate model evaluations.

COMMENT:P. 99: Another significant result is a projected increase in monsoon interannual variability (Hu, Z.-Z., M. Latif, E. Roeckner, and L. Bengtsson, 2000: Intensified Asian summer monsoon and its variability in a coupled model forced by increasing greenhouse gas concentrations. Geophys. Res. Lett., 27, 2681-2684.; Räisänen, J., 2002: CO2-induced changes in interannual temperature and precipitation variability in 19 CMIP2 experiments. J. Clim., 15, 2395-2411.; Meehl, G.A., and J.M. Arblaster, 2003: Mechanisms for projected future changes in south Asian monsoon precipitation. Clim. Dyn., 21, 659-675.)
RESPONSE: We have included the Raisanen citation on page 129 of the PRD. Because we do not intend to review the entire monsoon literature, we have limited our discussion of modeling articles to those comparing a number of models. Thus, we do not include the first and third references

COMMENT:P. 102: A recent study has attributed the intensification of the SAM mostly to decreases in stratospheric ozone, with some contributions from increasing GHGs (Arblaster J. M., and G. A. Meehl, 2006: Contribution of various external forcings to trends in the Southern Annular Mode, *Journal of Climate*, **19**, 2896–2905.)
RESPONSE: This work is now cited in the annular modes section at line 9, page 108 of the PRD.

COMMENT:P. 104, top: In the discussion of melting permafrost, the relevant key reference should be Lawrence and Slater (2005). Oddly, this reference is given later on this page in regards to vegetation changes, though its key and most important result pertains to permafrost melting.
RESPONSE: Lawrence and Slater (2005) now are first cited earlier, in discussion of frozen soil in the paragraph that begins on line 25, page 133 of the PRD.

COMMENT:P. 112: …and here is the second ENSO discussion
RESPONSE: The second discussion was devoted to the full suite of climate variability associated with ENSO. The ENSO information was consolidated and begins on line 24, page 143 of the PRD.

COMMENT: The “El Nino-like” response to increasing CO2 was first identified by Meehl and Washington in 1996 (Meehl, G. A., and W. M. Washington, 1996: El Niño-like climate change in a model with increased atmospheric CO2 concentrations. Nature, 382, 56–60). This type of response has subsequently been addressed by, for example, by Cubasch, U., G.A. Meehl, G.J. Boer, R.J. Stouffer, M. Dix, A. Noda, C.A. Senior, S. Raper, and K.S. Yap, 2001: Projections of future climate change. In: Climate Change 2001: The Scientific Basis. Contribution of Working Group I to the Third Assessment Report of the Intergovernmental Panel on Climate Change [J.T. Houghton, et al. (eds.)]. Cambridge University Press, Cambridge, pp. 525-582;
Collins, M., and The CMIP Modelling Groups, 2005: El Niño- or La Niña-like climate change? Clim. Dyn., 24, 89-104; and
Yamaguchi, K., and A. Noda, 2006: Global warming patterns over the North Pacific: ENSO versus AO. J. Met. Soc. Japan, 84, 221-241.

RESPONSE: We appreciate the references and have incorporated the Meehl and Washington citation on page 145 and the Collins, et al citation on page 149 of the PRD..

COMMENT: In the IPCC AR4, Ch. 10, there is a figure (10.6) relating El Nino-like vs. La Nina-like response across a number of the current models. It is instructive that the majority show an El Nino-like response to increasing CO2.
RESPONSE: We are glad that the reviewer raised this issue and we have tried to describe the mean response more precisely in the revised draft, starting with the paragraph that begins on line 25, page 145 of the PRD. Two studies (Oldenborgh et al 2005 and Collins et al 2005, cited by the reviewer above) conclude that if only the most realistic models are surveyed, then the mean SST seems likely to warm uniformly across the equatorial Pacific. The El Nino-like response cited by the reviewer arises when less realistic models are added to the average. Compared to these two studies, Figure 10.16 from the IPCC AR4 report uses a slightly different metric to show that the eastern Pacific is expected to warm by an extra half degree Celsius compared to the west. We have concluded that a uniform or slightly greater warming in the east is expected, but that the disagreement among the most realistic models is as large as the consensus change and may depend upon the precise metric chosen to characterize the warming.

COMMENT:P. 114, table: it should be noted that the spacing of grid points is at the equator, since the models typically increase the resolution starting near 5N and 5S to maximum resolution at the equator
RESPONSE: We have modified the table caption (Table V 5, page 146 of the PRD) to include this comment.

COMMENT: Near P. 117-118: This is very much in the nature of a text book discussion and could be trimmed quite a bit
RESPONSE: We would agree were this report intended for climate modelers with expertise in ENSO like the reviewer. However, our ostensible audience studies impacts of climate variations, include the far-flung effects of changes to the equatorial Pacific ocean. We included this section in an attempt to explain to more casual readers why climate projections for the tropical Pacific are inconsistent. We are open to removing or shortening this section, now on pages 146-147 of the PRD, if we get similar complaints during the public review (where impact and policy scientists are likely to comment).

COMMENT:P. 126: Some papers on extremes have model evaluation of either climatology of extremes (e.g. heat wave intensity over North America and Europe in Meehl, G. A., and C. Tebaldi, 2004: More intense, more frequent and longer lasting heat waves in the 21st century. *Science*, **305**, 994–997), or observed frost day trends over the U.S. (e.g. Meehl, G. A., C. Tebaldi, and D. Nychka, 2004: Changes in frost days in simulations of 21st century climate. *Climate Dynamics,* **23**, 495–511. doi: 10.1007/s00382-004-0442-9).
RESPONSE: A discussion of the Meehl and Tebaldi Science paper has been added to the "Heat and Cold Wave" section that begins on line 8, page 160 of the PRD. The Meehl et al. paper on frost days is already discussed in the "Heat and Cold Wave" section.

COMMENT:P. 132: It should be noted that in all models studied so far, inclusion of carbon cycle introduces a positive feedback to the system from increase CO_2 (i.e. Friedlingstein, P., et al., 2006: Climate-carbon cycle feedback analysis: Results from the C4MIP model intercomparison. J. Clim., 19, 3337-3353.)
RESPONSE: The point was made in the Reviewers' Draft with the text "A recent study examined carbon cycle feedbacks in eleven coupled AOGCM / carbon cycle models using the same forcing (Friedlingstein et al., 2006). There was unanimous agreement among the models that global warming will reduce the fraction of anthropogenic carbon absorbed by the biosphere, but the magnitude of this feedback varied widely among the models, leading to additional global warming (when the models included an interactive carbon cycle) ranging between 0.1 to 1.5 °." The text was retained on page 167 of the PRD.

COMMENT:P. 140: use "end-to-end" example from ACPI Pilot Project described in the special issue of *Climatic Change*, vol. **62**
RESPONSE: This example has been added to the Applications chapter in the section titled, "Water Resources in the Western U.S.," which begins on line 25, page 174.

Reviewer: Philip Mote

Dr. Mote’s review arrived on April 18, 2007, when the Public Review Draft was in the final stages of editing, therefore the detailed comments were not considered in the revisions. Nevertheless, we did account for his “General Comments,” which were similar to those of other reviewers.

COMMENT: The report needs considerable additional polishing to make it readable. Figures are improperly labeled and captioned, placeholder comments pepper the document, and the document needs careful proofreading and copy-editing. In addition, the readability -- particularly the use of disciplinary jargon -- of the document is uneven. The section on ocean modeling is rife with words that would be unfamiliar to a generally well-educated audience, like enthalpy.

RESPONSE: We agree. The Reviewers’ Draft was very rough and the Public Review Draft (PRD) has eliminated these problems.

COMMENT: Another point concerns the organization of the report. Portions of Chapter 2 would fit better in Chapter 1;

RESPONSE: The report has been substantially reorganized to be more consistent.

COMMENT: Chapter 7 is just a snippet, too short to be a chapter. Navigation would be much easier with a table of contents.

RESPONSE: Chapter VII was expanded.

Reviewer: Brad Udall

COMMENT: This review will thus be limited to very high level question of does the Draft meet the requirements of the Prospectus.

Two portions of the Prospectus deserve highlighting and will be discussed below. The Prospectus specifically states: *"The topics addressed by this Climate Change Science Program (CCSP) product are the strengths and limitations of climate models at different spatial and temporal scales. Its purpose is to provide this information on the strengths and limitations of the results from climate models,* ***in ways that will allow the potential user of the information to evaluate how best it may be applied or not applied*** *(CCSP Strategic Plan, page 19)."* (Emphasis mine.)

It also states: ***The intended audiences of this CCSP product are decisionmakers and researchers who use climate model output as input to studies or analyses in their respective, non-climatic disciplines (e.g., ecosystem science, hydrology and water resources, economics, human health, and agriculture and forestry)****. In order to facilitate application and decisionmaking using climate model information, an evaluation and assessment of the state of science of climate models is essential. This product is directed towards this goal.* (Emphasis mine.)

RESPONSE: Potential users span a wide range. The product summarizes a large amount of knowledge in a reasonably concise form to provide those familiar with aspects of the science, but not necessarily modelers, a perspective on the status of the field. We believe that the product can stand on its own as a reference on climate modeling to potential users of climate model results to a wide range of applications.

We respectfully submit that through his emphasis on the single sentence in the second paragraph, the reviewer does not fully appreciate the full context of the paragraph. In particular, the sentence that follows the one, which he emphasized, explicitly declares that use of climate model information for policy requires an assessment of the state-of-the-science. The product cannot specifically address all applications, nor anticipate future ones.

General Review Comments

COMMENT: This title of document is "Climate Models: An Assessment of Strengths and Limitations **For User Applications**." Unfortunately, much of the draft does not address user applications and appears to be directed at climate scientists, not decision makers. In addition, this first version feels very much like a simple collation of facts, rather than a true 'Synthesis and Assessment'. This is understandable given the magnitude of the task, but this product needs substantial work if it is truly to be of use to decision makers. Many of the critical questions posed in the 3.1 Prospectus have not been discussed, let alone answers provided. I am sympathetic to the authors of this document – these are extremely difficult questions to answer -- I would suggest that some have never been answered anywhere – and answering them may require the assistance of experts currently not present on the panel. Nevertheless, these questions are the key part of the document and if they can't be answered such acknowledgement needs to be forthrightly made and some form of guidance for decision making provided.

RESPONSE: We are confused by the reviewer's statement, "Many of the critical questions posed in the 3.1 Prospectus have not been discussed …," because it is largely inconsistent with the explicit mapping of the chapter contents to the questions in his further comments below. Clearly, the reviewer's expectations after reading the prospectus failed to be met by the product. In retrospect, the confusion is more likely caused by the prospectus title, which reasonably may be interpreted as a summary of modeling for specific applications. The writing team believes the product meets the stated objectives. Nevertheless, one lead author did anticipate such confusion after completion of the Reviewers'(first) Draft, a concern which was considered carefully by the rest of the team. Many of the revisions in the Public Review Draft (PRD) were motivated by a concern to make it more broadly understandable.

COMMENT: In order to be useful to decision makers, the document needs several additions. It needs tables that synthesize answers to key questions. It needs an executive summary designed to be read by the heads of water management agencies with a body that can be understood by the technical staff of those agencies. The diagrams and tables in this document should ultimately tell much of the storyline, yet, with the exception of chapter 5, there are very few diagrams and/or tables. It also needs a section on where to get other basic information on climate models and a glossary. All too often, e.g. Chapter 2, the material is presented as a highly technical data dump with no introductory material. Detailed technical information is fine, but the document needs a hierarchy of detail that is navigable by readers with different backgrounds. Finally the document needs to provide less informed readers with a road map of other basic documents on climate modeling including for example, the relevant IPCC chapters. I understand that many of these suggested enhancements are rarely found in first drafts, but I want to make sure that these considerations are incorporated in future versions.

RESPONSE: An executive summary was added to the PRD. Overall, the PRD is much better in tone, style and evenness of presentation than was the Reviewer's Draft. We appreciate that water resource specialists may be disappointed by the lack of hard information in the report. Precipitation simulation is a known weakness in the CMIP3 class of climate models, and that deficiency is reflected in the report. Chapter II was completely rewritten and the current version is much more readable. A glossary was not added, as suggested, because it would be difficult to determine what should, or should not be included. At the time both the Reviewers' Draft and PRD were written, the final IPCC Fourth Assessment Report (AR4) was available, but not citable, since it was not yet publicly released. The next draft will contain the appropriate references.

COMMENT: The Prospectus states: *"The intended use of this CCSP product is to provide information to those who use climate model outputs about the strengths and limitations associated with using models to project the potential effects of human activities on climate and sea-level rise.* ***A discussion of appropriate and inappropriate uses of model output will be included.*** *The product will address scientific issues on a comprehensive, objective, open, and transparent basis. While based on the peer-reviewed scientific literature,* ***it will be written to be accessible and useful to the well-informed general reader and decisionmaker****."* I failed to find a discussion about appropriate and inappropriate uses of model output. As currently written, the material is not accessible to the well-informed general reader and decision-maker.

RESPONSE: The writing team members are all experienced analysts of climate model results and believe the level of depth is necessary for the proper application of climate model output for applications. We share a concern that much of the applications work using model output is ill-informed, with misleading, or even erroneous conclusions. Chapter VII contains examples of appropriate uses of model results. We decided to not includee negative examples of inappropriate uses, as that would make the report unnecessarily provocative, thereby diminishing its value.

COMMENT: Finally, the document has a substantial emphasis on US models. Given the expertise of the authors this is understandable, but decision makers are not just interested in US models. While these models are high quality and deserve discussion, other models of similar quality exist, and it seems increasingly clear that multi-model results do a better job of constraining the future than do smaller subsets. Some discussion or acknowledgement of other high quality models would substantially enhance this document.

RESPONSE: Chapter II of the PRD contains an expanded discussion of the CMIP3 class of models, including a more complete accounting of model improvement over time and a brief discussion of model metrics. We continue, however, to emphasize the three US models that participated in the CMIP3 coordinated experiments, because the report's audience is primarily based in the US.

Specific Comments on the Six Prospectus Questions

COMMENT: The Prospectus includes six key questions which I provide below in italics along with my analysis of whether these questions have been adequately answered. I will note that as a reviewer, sometimes these questions do not directly map to a single part of the Draft document. While I do not desire to tell the authors how best to structure this document, the existing organization makes it difficult to assess if these questions have been addressed. (And as a minor sidelight, the lack of a Table of Contents in the draft was also an unfortunate barrier to analysis.)

RESPONSE: A table of contents will be added in the final version, as is often done in such reports.

1) What are the major components and processes of the climate system that are included in present state-of-the-science climate models, and how do climate models represent these aspects of the climate system? This section will include descriptions of crucial processes such as tropical convection and major feedbacks in the climate system (e.g., clouds, atmospheric water vapor, surface albedo, and soil moisture). This section will evaluate the ability of the current generation of models to simulate key processes, and identify gaps in understanding. It will also include brief discussion of crucial processes that are likely to play an important role in climate that are not yet incorporated in the models.

COMMENT: This question maps to Chapter II, Description of Global Climate System Models. This chapter deserves a less technical introduction, and the section in the Introduction entitled 'A Brief History of Climate Model Development' belongs in this chapter. (The Introduction also needs significant work.) Simple concepts like "parameterization" need to be introduced. A simple table or two that answer the questions posed above would be extremely useful.

RESPONSE: We have substantially rewritten Chapter II to make the descriptions more readable, useful and at an even level of detail.

2) How are changes in the Earth's energy balance incorporated into climate models? How sensitive is the Earth's (modeled) climate to changes in the factors that affect the energy balance? This section will explain current approaches for incorporating changes in radiative forcing from both natural and human factors since the pre-industrial era. These include changes resulting from greenhouse gas and trace constituent emissions into the atmosphere, volcanic eruptions, and variations in the sun's intensity. This section will present a brief overview of the response of the global climate system, as derived from climate model results, for the various forcings (e.g., solar, volcanic, aerosols, anthropogenically derived greenhouse gases). The relative contributions of natural variability and human-caused factors for the period under consideration will be examined.

COMMENT: These questions are addressed in Chapter IV, Model Climate Sensitivity. As currently structured, Chapter IV deals with uncertainties due to variability in solar radiative forcing, cloud and water vapor feedbacks, and aerosols as well as the general concepts associated with differing model sensitivities. Like most of the chapters, an introduction with a roadmap and a simple explanation of the magnitude of each of these factors would enhance the understandability for high level decision makers. Alternatively, a chapter summary could provide the same material.

RESPONSE: Chapter IV has been extensively rewritten as well. Additional information important to applications has been added regarding the relationships between global temperature changes and regional changes in sub-continental scale temperature and precipitation. Simplifying the explanations of feedbacks further would cause unacceptable loss of context. We recognize that climate sensitivity is a seemingly simple concept that has many complexities. Nevertheless, a better understanding about uncertainties related to sensitivity yields insights into the uncertainties surrounding model based prediction, and therefore is essential for appropriate uses of climate model results for applications.

3) How uncertain are climate model results? In what ways has uncertainty in model-based simulation and prediction changed with increased knowledge about the climate system? This section will provide a discussion of the major sources of uncertainty in climate model results, as estimated through structured intercomparisons to observations, including the identification of the major sources of uncertainty in model assumptions and the characterization of radiative forcing. A description (or acknowledgement) of how increased knowledge can lead to greater uncertainty by increasing the number and complexity of processes included in climate models will be included.

COMMENT: Answers to these questions are found in Change V, Model Simulation of Major Climate Features. This is an enormous question and the Prospectus doesn't give much guidance on exactly what results this question should apply to. From the perspective of Rocky Mountain water users, temperature, precipitation and runoff are three key variables of interest but other decision makers are likely interested in the complete suite of potential model results including sea level rise. A summary table is critical.

RESPONSE: The inclusion of a table was considered, but it was unclear what summary information should be included. We have added the section on metrics in Chapter II, with a summary figure, to summarize the spread among the models for many fields. The prospectus explicitly states that uncertainty will be estimated through structured intercomparisons to observations, which was done extensively in Chapter V.

COMMENT: In chapter V, uncertainties associated with mean and other statistical moments are discussed. The discussion needs some more regional focus, including graphics for the US. Modeled precipitation biases need much greater discussion.

RESPONSE: There are significant discussions in Chapter V of the PRD dealing with extreme events, including floods and droughts, that have a strong regional focus.

COMMENT: I do not believe the second question and the last question above are addressed in the Draft. These are critical questions.

RESPONSE: There are only two questions in the paragraph, and we believe both are addressed adequately in the latest draft.

4) How well do climate models simulate natural variability and how does variability change over time? The ability of climate models to simulate the climatology and interannual variability is crucial for their use by the impacts and applications community. This section will describe efforts to evaluate these aspects of model performance. This section will also discuss the ability of climate models to simulate known patterns of natural variability, such as the Madden-Julian Oscillation, the El Niño Southern Oscillation, the North Atlantic Oscillation, and the Pacific Decadal Oscillation. A section on how these modes of variability have changed over time will be included.

COMMENT: This material is covered in Chapter V, Model Simulation of Major Climate Features, under the Modes of Variability subsection. The section includes some areas not spelled out by the Prospectus including extremes but lacks information on the NAO and PDO. A summary of this information is critical.

RESPONSE: The NAO and PDO modes of variability are covered in the section "Multi-decadal variability," which begins on page 153 of the PRD. We believe the information presented is already in summary form, and further reduction would result in an unacceptable loss of context.

5) How well do climate models simulate regional climate variability and change? This section will discuss how changes in certain regions (e.g., the North Atlantic or Tropical Pacific) can influence global climate change. It will also discuss limitations of "downscaling" methodologies—including regional climate modeling— used to obtain regional information from global simulations.

COMMENT For the most part, answers to these questions appear to be entirely missing from the document. Chapter III discusses The Added Value of Regional Climate Model Simulations, but that discussion is mostly technical in nature and does not discuss the output of such efforts. A discussion about the merits of dynamical vs. statistical downscaling is implied by the last part of this question. Such a discussion would be extremely useful to decision makers.

RESPONSE: We have expanded the discussion of dynamical versus statistical downscaling, showing how they complement each other. Key factors are computational resources and whether or not specific regional-scale circulations and distributions of temperature and humidity need to be resolved. For example, if regional-scale behavior not found in a GCM is necessary, then dynamical downscaling is favored. If instead, information is needed for variables not typically simulated in numerical models, such as zooplankton populations, then statistical approaches may be more effective. We have also identified the added value of regional modeling for resolution-dependent features, such as intense precipitation, and discussed important guidelines to follow when performing regional simulation.

6) What are the tradeoffs to be made in further climate model development (e.g., between increasing spatial/ temporal resolution and representing additional physical/ biological processes)? This section will consider the opportunities and constraints on future model development (e.g., additional computational cycles and lack of process knowledge). It will outline prospects for improvements potentially important to policymaking and decisionmaking.

COMMENT: Presumably this question maps entirely to Chapter VI, Future Model Development. The current version of the chapter discusses a variety of technical modeling issues and provides a general overview associated with Cloud Resolving Models, the Carbon Cycle and other biogeochemical cycles, Land Cover and Land Management Practice Changes and Ocean Biogeochemistry. Unfortunately, it doesn't address any of the questions posed in the Prospectus. These are very important questions and must be addressed in future drafts.

RESPONSE: The revised version of Chapter VI addresses traded-offs between resolution and complexity in the section on cloud resolving models that begins on page 163. The prospects for future models are covered extensively, so we are confused by the comment that it "doesn't address any of the questions posed in the Prospectus."

Summary

COMMENT: This document has the potential to be very useful to decision makers coping with the various uncertainties surrounding future climate change including how well current climate models work and their applicability for planning purposes. The first draft of this document is a good start but it needs to be written in a way that is truly accessible to decision makers. This means that adequate introductory material needs to be added and simple summaries provided. In addition, the authors need to provide answers to all questions in the Prospectus.

RESPONSE: As is clear from the responses above, the authors and reviewer have a difference of opinion as to whether the Prospectus questions were adequately answered. Further, the reviewer appears to want a product more directly related to specific applications. While we have taken steps in that direction, we expect that the product will still not meet this particular reviewer's expectations.
February 26, 2007

Reviewer: John E. Walsh

COMMENT: At first glance, I wondered about the need for this 140-page report, given that (1) the new Fourth Assessment Report of the IPCC's WG1 contains a chapter assessing current climate models, and (2) Parkinson and Washington's climate modeling textbook -- 2nd edition, 2005 -- and Trenberth's *Earth System Modeling* volume cover the subject of climate modeling rather comprehensively. However, after a careful reading, I concur that the report serves a useful purpose. The focus on the climate models of the U.S. gives the report its niche. The most effective portions of the present draft adhere to this focus; the least effective do not.

It is also commendable that the report largely restricts itself to simulations of recent (20th century) and present climate. The inclusion of projected changes and/or paleoclimate simulations would have opened cans of worms and made the report unwieldy.

An inevitable limitation of the report is that its shelf life will be rather short. As the next generation of models comes on the scene over the next few years, progress in climate modeling will likely make much of this report outdated. Periodic updates, *a la* IPCC, would be a remedy.

As noted in the instructions to reviewers, this report is clearly a first draft. It is missing some sections (e.g., Executive Summary; p. 30); not all figures are included; and the need for further input is mentioned parenthetically on many occasions. Although a reviewer can contribute more effectively when a first draft is polished, I will nevertheless provide detailed comments on more substantive points, while ignoring many editorial details and obvious typos.

RESPONSE: We agree that the Reviewers' Draft was very rough and have made significant improvements in both content and completeness. This version did not have page or line numbers. Dr. Walsh's page references are based on a MS Word version of the document, which has slightly altered formatting, and therefore page numbers, from the PDF version that was created from that MS Word document. We appreciate the reviewer taking the time to review the document in the form it was received.

COMMENT: On many occasions the text states that models "have improved" in their simulations of (fill in the blank). Those statements should always indicate what the improvements are *relative to* -- and ideally should provide specific references to substantiate the statements.

RESPONSE: We have eliminated these ambiguities. In the model development sections describing the three US modeling efforts, it is implied that the word "improved" is relative to the groups' prior model versions. Elsewhere, it is clear that improvements are relative to models reported in the IPCC Third Assessment Report released in 2001.

COMMENT: Page 1, middle: The text states that the report will focus primarily on the CMIP models. Given the immense coordination and archival of models for the IPCC's AR4 (2007) and the many journal papers now reporting diagnostic assessments of those models, an emphasis on the AR4 models would certainly be more timely. Some sections (e.g., the assessment of model-simulated ENSO variability on p. 112-121) do utilize the AR4 models.

RESPONSE: With the publication of the AR4, the model experiments that were used for the AR4 are now referred to as the CMIP3 simulations, to give proper acknowledgment to the WCRP's Working Group on Coupled Modeling, which coordinated the experiments for the IPCC.

COMMENT: Page 57, bottom: The sentence "The models are also adjusted in different ways…so as to optimize so as to optimize the fit to observations deemed to be of particular importance" seems extremely significant and deserving of elaboration -- perhaps with a few examples. This issue is fundamental to an understanding of the climate modeling enterprise.

RESPONSE: The section titled "Component coupling and coupled model evaluation" that began on page 27 and continues through page 28 of the Reviewer's Draft has been shortened and rewritten in the Public Review Draft (PRD). The new text begins on page 45. An improved discussion of model tuning and evaluation gives significant insight into the model development process, which we believe was the point the reviewer was making. The subject of tuning to sensitivity is mentioned on line 24, page 73 of the PRD, which states that none of the models were tuned to produce a desired sensitivity.

COMMENT: The text should address the issue of flux adjustment (or other restoring methodologies) by defining these procedures and saying where they are -- or are not -- used in the models. The only such comment I could find is on p. 28 and pertains specifically to the GFDL model. If flux adjustment/restoring is not used anywhere in any U.S. model, then say so – that's a major advance over the coupled models of ten years ago.

RESPONSE: Line 5, page 91 of the PRD makes such a statement. A similar statement can be found in the Reviewers' Draft in the first paragraph of Chapter V (page 77 in the PDF version).

COMMENT: Pages 101-106: The "Polar climate" section does not focus on U.S. models, creating some inconsistency with other sections. The subsequent section on sea ice (p. 107-108) does emphasize the U.S. models.

RESPONSE: U.S. global and regional models are included in the multi-model analyses cited in the "Polar Climates" section. We have added discussion of the specific performance by U.S. models. They tend to span the same range of accuracy as the full set of models from centers around the world and thus perform roughly the same as their non-U.S. counterparts (line 20, page 134 of the PRD).

COMMENT: Pages 101-109: Missing from the presentation are assessments of the models' simulations of permafrost and snow cover (including snow cover over the larger non-permafrost areas). Even generic background information is missing -- for example, do any of the models include permafrost thaw or, more generally, the effects of soil freeze/thaw on hydrology?

RESPONSE: The section on Land Surface Models already discusses generic issues, like advances in the sophistication of snow simulations and attempts to model permafrost and seasonally frozen soil. Reference to this section has been added to the Polar Climates section at line 20, page 135 of the PRD.

COMMENT: Related to snow cover: Somewhere in the text (Under "Land surface models"?), there should be some indication of how snow is masked by vegetation in the models. Snow/vegetation interaction is important for the albedo-temperature feedback seasonally, interannually and over longer timescales.

RESPONSE: This information has been added to the Snow and Ice section under "Land Surface Models" (Line 7, page 37 of the PRD).

COMMENT: How is glacial runoff (seasonal melt) included or not included? Page 82, middle, implies that CCSM3 does not include this process. Presumably none of the models include ice sheet dynamics. Given this issue's high visibility during the recent release of the IPCC's SPM (WG1), the text should include more explicit information on the treatment of ice sheets and glaciers in climate models.

RESPONSE: On page 8 of the PRD we explicitly state that glacier runoff is not included CMIP3 models. We have added a section on ice sheet modeling on pages 37-39 of the PRD.

COMMENT: The section on sea ice models (p. 23-26) fails to do justice to the role of high-frequency ice deformation, which controls the fractions of open water and thin ice where the vast majority of surface/atmosphere exchanges occur over polar oceans.

RESPONSE: Included in an early revision of the PRD was a comment on the importance of resolution/open water and exchange of heat between ocean and atmosphere. "The incorporation of a sea ice component into the climate model system allows for the capture of the processes related to the exchange of heat between the ocean and the atmosphere. The flux of heat is affected by the relative amount and extent of the ice as well as the amount of open water between areas of ice. Sea ice models incorporated into climate models capture some of the exchange, but currently are at resolutions that do not allow for the simulation of the smaller scales seen in nature." This text was inadvertently deleted in a subsequent edit, and will be restored in the next version.

COMMENT: Pages 28-30: The sections on "Recent development paths" at the U.S. modeling centers will have an especially short shelf life. When the missing sections are provided, they should be made as brief as possible -- and the GFDL section could be shortened.

RESPONSE: We believe that the emphasis on US models is one unique aspect of this report. Nevertheless, Chapter II was completely rewritten in the PRD and is much more readable and informative.

COMMENT: Pages 6-16: The description of atmosphere and ocean GCMs seems way more technical than necessary. This is material for textbooks, not for this report. I suggest trimming by 50% or more.

RESPONSE: As stated above, we have substantially rewritten Chapter 2 to make the descriptions more readable, useful and at an even level of detail.

COMMENT: Pages 32-40 are confusing and difficult to review (e.g., with figures missing). More specifically, the presentation of results from one ensemble member "that agrees best with the observed…time series" seems like a very dubious practice -- planners and policymakers can easily misinterpret and misuse such selective results.

RESPONSE: This section was completely rewritten and integrated into the model evaluation section (page 53 of the PRD). It illuminates some of the tradeoffs required in climate model development that are mostly unknown outside of the modeling community. We believe that this information is important to those who may apply climate models or use their results.

COMMENT: Page 80: Is the mid-century cooling, as simulated by the models, due to the prescribed aerosols? to prescribed solar variability? or both? Given the recent paper by M. Wang et al. (2007, *J. Climate*) arguing that the mid-century warming/cooling is consistent with natural variability in some IPCC AR4 models, elaboration seems to be needed here.

RESPONSE: We leave it to the individual modeling groups to argue the points in the cited paper related to the effects of individual forcing elements, which they have not yet published. In the PRD, we have rewritten the twentieth century section to show the global surface temperature time series from the individual US models, and make the point that the time series can only be matched if anthropogenic effects are included.

COMMENT: Pages 112-121: This ENSO section is way too long. Simulation of ENSO was already covered on p. 86-87, where the models' underestimation of equatorial Pacific SST variability is reported.

RESPONSE The first discussion was limited to trends in SST within various regions. whereas the second section focused on ENSO *per se* and to what extent the model predictions are consistent and reliable. The ENSO information was consolidated and begins on line 24, page 143 of the PRD. ENSO is the leading mode of recent tropical variability, and has teleconnections that produce effects in mid-latitude North America, including the marginal rainfall climate of the American southwest. We believe it is worth some level of detail (especially since the conflicting results among the models defy any pithy summary), and the intended audience of policy makers and impacts scientists may appreciate a full discussion even if it is unnecessary for climate experts like the reviewer. We are open to shortening this section, but prefer to wait for additional advice from the public comments.

Minor points:

COMMENT: Page 79, first sentence of "B. 20th century trends": Why not identify the model? Models are identified everywhere else in the report.

RESPONSE: This figure was replaced with labeled versions of individual model results. This section begins on page 99 of the PRD.

COMMENT: Page 82: A reference is needed for the stratosphere-troposphere coupling of volcanic effects.
RESPONSE: The reference has been added in this section, which is now found on page108.

COMMENT: Page 84: The statement that the Gulf Stream warms Europe needs to be placed in the context provided by R. Seager et al. (2006, "The source of Europe's warm climate", *American Scientist, 94*, 334-341).

RESPONSE: We have modified the statement to read "It has been suggested that this pattern of circulation if it becomes weaker (i.e. less warmer water flowing towards Europe) will impact the climate." which is consistent with the Seager paper. (Page 113 of the PRD).

COMMENT: Page 84: What is the uncertainty in estimates of the integrated transport by the AMOC? The text shows uncertainties only in transports at specific latitudes (p. 93-94, figure from Schmidt et al., 2006).

RESPONSE: In both the Reviewers' Draft and the PRD there is a statement referring to the uncertainty presented in Table V 2, which now appears on page 114. The text preceding the table on page 113 reads, "The authors do not attempt to explain why the models are different from each other and from observations, rather, that there is a broad range in the value of these metrics for a set of climate models."

COMMENT: Page 99, lines 10-11: Where is the circulation strength in both winter and summer…expected to weaken?"

RESPONSE: The text now appears on page 129 of the PRD, the preceding paragraphs refer to the Asian monsoon.

COMMENT: Page123: Reference to Burke et al. (2006) is missing.

RESPONSE: The reference now appears on page 180 of the PRD.

COMMENT: Page 124: How is the "4^{th} largest precipitation event" equivalent to the 99^{th} percentile?

RESPONSE: From the context of Emori et al. paper, the percentile ranking is based on all 365 daily values, both zero and non-zero. We have added the phrase "of the 365 daily values" after "99th percentile" to clarify (line 12, page 158 of the PRD.)

COMMENT: Page 131: I suggest omitting the second paragraph on this page ("Research with CRM falls into two categories…"). The present report does not need this paragraph.

RESPONSE: Since this chapter deals with future development trends, we believed it was important to retain the paragraph.

COMMENT: Page 138: The source for Figure VI-1 needs to be provided.

RESPONSE: On page 167 of the PRD, the text states that the values in the figure," are consistent with estimates from a variety of sources, but substantial uncertainties attach to the numbers (e.g. often a factor > 2 uncertainty for fluxes; (see Prentice et al. 2001)." The values were not taken exclusively from that source, however.