

## **Authors' Response to CPDAC Comments on Public Review Draft version CCSP SAP 3.1**

Attached are two documents with comments from CPDAC committee members. The first is an email from Dr. Brian Flannery received for the August 2007 CPDAC committee meeting. The second is a marked-up PDF of the Public Review Draft created by Prof. Richard Lindzen.

Response to Flannery email.

Flannery : Empirical Statement: Average of models better agrees with metric of comparison than any single model.

**Response:** As stated in section 2.7, the reason for the superiority of the mean model is still an active research question, but is most likely the simple result of averaging limiting the influence of values far from the mean.

Flannery: Key Question: To what extent can we rely on climate models to project future change?

**Response:** Comments regarding the skill of the models in climate projections are beyond the scope of CCSP SAP 3.1. Sub-questions regarding climate sensitivity and natural variability were addressed in the rewrite of Chapters 4 and 5, as described below.

Response to Lindzen mark-up of Public Review Draft.

### **Response:**

Most of Prof. Lindzen's comments were minor and were addressed in the major rewrite of the document that occurred in the fall of 2007, as follows:

1. Each chapter was completely reedited by multiple members of the author team.
2. Reedited versions were edited by professional science technical editors.
3. Author team subsequently reviewed and performed final editing for January 14 version for CPDAC Review.

Prof. Lindzen had major criticisms of Chapter 4, which was rewritten with his involvement

**Summary of major changes to Public Review Draft to produce CPDAC Review (January 14, 2008) version:**

*Executive Summary*

The Executive Summary was rewritten to follow the structure of the Prospectus. Although the content is largely the same, there were some additions and deletions that reflect changes in the full document.

*Chapter 1-3, 5 and 6*

These chapters were heavily edited to provide a more consistent level of presentation, and better organization. The content is largely the same, with some detail removed. Public and committee comments were addressed in the edits. Some figures were redrawn or removed.

*Chapter 4*

As a result of criticism by Prof. Lindzen, both during the August meeting and through a series of email exchanges with the authors, a section on observational constraints on climate sensitivity was added, in addition to a major edit.

*Chapter 7*

We added a section on the California climate change assessment activities to provide a more complete chapter and as an example of likely uses of climate model output for regional work over the next several years.

**Authors' Response to CPDAC Comments on CPDAC Review Version of CCSP SAP  
3.1 dated January 14, 2008**

Prof. Richard Lindzen again provided a detailed marked up PDF of the CPDAC Review Draft. A further revision of the document resulted in the March 5, 2008 Draft for Final CPDAC Concurrence. A point by point response to Prof. Lindzen's comments follows.

Page: 3

Line 2 to examine NOT test

**Response:** Sentence changed

Line 5 a NOT the

**Response:** Sentence changed

Page: 4

Line 6 The reason for this is far from evident, and warrants some attempt at explanation. Is it simply that some models give extreme answers that are tempered in the averages?

**Response:** We agree that the literature provides no satisfactory answer. The statement is presented as an objective result based on statistical analysis, which is repeated again in section 2.7 on model metrics.

Line 23 This is not an appropriate use of the word 'robust.'

**Response:** Wording changed

Line 26 This is a delicate number. Hence, model results add little to the likelihood of the number being correct even as a bound.

**Response:** This is an opinion. We believe that the statement was made in the appropriate context, when the sentence following the one referenced is included.

Page: 5

Line 3 Do not confuse uncertainty with model range.

**Response:** Wording changed

Line 8 It will be interesting to see if this version restricts itself to tests that support models.

**Response:** None required

Page: 6

Line 9 This could be true even when major features are misrepresented.

**Response:** The statement represents an objective result

Page: 7

Line 3 Given that the oceans are never in equilibrium with the atmosphere, and that models have made little effort to initialize this, it is not clear why flux corrections are a bad thing.

**Response:** This is an opinion. There has been a dedicated effort to remove flux correction over many years by most modeling groups.

Line 11 At least remark that you are talking about the direct effect of solar variability. If there are indirect effects, modelers have ignored them.

**Response:** We stand by the text in the Executive Summary. In response to the criticism, the following was added in Section 5.2 “Indirect solar effects (e.g. involving cosmic rays and clouds (Svensmark 2007)) are not generally included in AOGCM simulations. These effects have been proposed occasionally as causes of global warming, though over the past 20 years their trends would if anything lead to cooling (Lockwood and Frohlich 2007).”

Line 29 First, given claims for the past thirty years, this is not entirely relevant, but also, observational studies from Tsonis, Ghil and others contradict this claim.

**Response:** We stand by the conclusion. The important phrase is “no models show...”

Page: 8

Line 2 Change ", which show that" to "; in the models". Don't keep forgetting that 'reasonable' simulation does not constitute explanation.

**Response:** The pronoun “which” clearly refers to the preceding phrase “current models.” The suggested change is unnecessary.

Line 15 Don't you mean 'qualitative'?

**Response:** The conclusion was drawn from quantitative comparisons. The word “general” is used correctly.

Line 19 Really? Convergence can and does occur for a number of reasons -- of which physical correctness is probably not the most common.

**Response:** The point of the sentence is that convergence has not been achieved, and that physical correctness will result in convergence. It is not stated, nor implied, that convergence can only result from physical correctness.

Line 30 This suggests a really low standard for success.

**Response:** None required

Page: 9

Line 7 Given this, plus poor performance on PDO and AMO, as well as the strong evidence of other internal multidecadal variations, it is impossible to understand how the

authors can judge the presence or absence of internal variability by its presence or absence in models.

**Response:** The report contains an extensive discussion about variability in section 5.2.2. We believe the discussion is objective and based on assessment of the literature.

Page: 11

Line 25 After working with WRF for a year, I'm not so sure that this optimism can be sustained.

**Response:** None required

Page: 13

Line 5 Define simulation. I don't think this is the word you want. It suggests that you are mimicking preknown observations. This, is certainly one test, but it is only the beginning. Successful prediction is what you want to emphasize.

**Response:** Webster's defines simulation as "the imitative representation of the functioning of one system or process by means of the functioning of another." The execution of mathematical models of the climate system on digital computers meets this definition.

Line 20 Given the relatively short periods being used for attribution studies, it would seem that ability to predict El Ninos becomes important for climate models. Even more important is the ability to deal with so called PDO's and AMO's.

**Response:** The reference to ENSO is in the context of time scale definition in the report. The comment is not relevant to this paragraph.

Line 29 These are almost always approximately in balance even if we change greenhouse gas concentrations.

**Response:** That point is made within the paragraph," The decades-to-century changes in the Earth's energy budget, manifested as climate changes, are just a few percent of the average values of that budget's largest terms."

Page: 14

Line 3 I think radiative forcing is being confused with energy imbalance.

**Response:** We are referring to the energy imbalance, based on model results.

Page: 16

Line 3 Actually, the IPCC does not conduct scientific inquiries. It does assessments based on existing work.

**Response:** One type of inquiry can be an assessment of the scientific literature

Line 11 classic?

**Response:** The word was replaced with “often used.”

Page: 17: No comments

Page: 18

Line 2 Expert Judgement has an established definition. Please give it. It is meant to provide a decision in the absence of specific information.

**Response:** According to the paper “Elicitation and Analysis of Expert Judgment by Booker and Meyer of Los Alamos, who have also authored a book on the subject, “Expert judgments are the expressions of informed opinion, based on knowledge and experience, that experts make in responding to technical problems.” We believe that the definition is implicit in the use of the term in the context of the paragraph. Adding the definition would detract from the presentation.

Line 3 One reason that simulation is not considered a major achievement is that it can sometimes be achieved for the wrong reasons.

**Response:** None required.

Page: 21

Line 3 Hopefully, this will be discussed later in the report.

**Response:** We agree that the literature provides no satisfactory answer. The statement is presented as an objective fact and repeated again in section 2.7 on model metrics.

Page: 23

Line 6 This paragraph makes it sound as though the primitive equations relate to differencing. Of course, they don't.

**Response:** We respectfully disagree. The discussion refers to model formulation. Differencing methods are not discussed anywhere in the paragraph. No changes were made.

Line 17 Clarify what is meant by 'boundary.' To a certain extent, this is an artificial notion since the tropopause is permeable, and allows wave transmission relatively freely.

**Response:** Several definitions consulted use the word “boundary” to describe the tropopause.

Page: 24

Line 13 Some indication of the error in readily interpretable units (such as watts per square meter) should be included.

**Response:** The discussion of error magnitudes would require a thorough discussion of the papers, as there is not a simple range that can be extracted and quoted. We believe

the references are complete and readers are invited to refer to the papers should they require more depth.

Line 15 Note that, to a certain extent, parameterizations represent wishful thinking since it is rarely established that parameterization is even possible. In most cases, they represent ad hoc assumptions as to how one supposes the unresolved process ought to operate.

**Response:** We disagree with the term “wishful thinking,” as well as the premise of the statement. The reviewer is expressing an opinion to which we believe a response is not required.

Page: 25

Line 24 What exactly is one 'simulating' here.

**Response:** Wording changed to “kinetic energy dissipation through turbulence”

Page: 26

Line 18 Include a simple definition.

**Response:** A reference to the primitive equation formulation (Haltiner and Williams) was provided earlier.

Line 24 There is a need to include, somewhere, a sentence about the small Rossby radius of internal motions.

**Response:** The text from the bottom of page 27 through the top of page 29 describes the scales that dominate ocean dynamics with appropriate references. The term Rossby radius of deformation is not used, because it would require an understanding of geophysical fluid dynamics beyond the scope of the intended audience.

Page: 27

Line 21 Do atmospheric models still use Shapiro filtering? If so, this should be described.

**Response:** The parenthetical statement is a distraction to the discussion and was removed.

Page: 29

Line 9 It's not a big deal, but I have the impression that self-congratulatory adjectives like 'major', 'pioneering', etc. are overused. Perhaps one could just use important or potentially important.

**Response:** None required.

Page: 30

Line 8 It should be noted that initialization could be important given the long time scales associated with some oceanic systems.

**Response:** While true, the discussion of initialization for long climate runs is not within the scope of the section, which describes the construction and evaluation of ocean GCMs.

Line 27 As one discusses more and more detailed processes, there should be some discussion of how these might be prioritized. If there is no way to prioritize, then this is a very strong implicit acknowledgement of ignorance.

**Response:** The discussion describes a retrospective view of land surface model development in the literature. Priorities for future development are beyond the scope of the chapter.

Page: 34

Line 12 How does this compare with the impact of arctic clouds on albedo?

**Response:** An interesting question, but one that does not need revision of the text in the current context. The issue of albedo masking extends outside the Arctic, and may be especially relevant where the solar flux is stronger during winter and spring.

Line 22 One would never know this from the media where predictions of ice sheet behavior are treated as routine.

**Response:** None required

Page: 36

Line 10 Why not use 'testing' instead of 'validation?'

**Response:** "Validation" means testing on data independent from that used to develop (and possibly calibrate) the model. "Testing" can include comparison of the model with the data used for development. Thus, hydrologists in particular often talk about calibration and validation of models to make this distinction clear.

Line 16 Is model convergence necessarily a sign of improvement?

**Response:** The text has been changed. The intent is to point out convergence toward observation-based estimates, though the elusiveness of good observations remains a problem.

Page: 40

Line 21 Explain what this is and why one should or should not get rid of it. As Ed Schneider noted some years ago, since the ocean is not in equilibrium with the surface, some adjustment should be applied.

**Response:** This was covered in the response to the comment on page 7, line 3.

Page: 41



Line 8 It is interesting to note that both this (Schneider and Lindzen, 1977) and the gravity wave drag were proposed about thirty years ago.

**Response:** None Required

Line 25 The use of the feedback analysis would show that this change does not really demand such a large change in feedback factors.

**Response:** The feedback analysis was performed in chapter 4, as referenced in the sentence.

Page: 42

Line 5 It should be noted that observational estimates vary by a factor of three or so. Estimates of solar forcing are even more uncertain.

**Response** A reference was added to change the sentence to, "In 20th Century simulations, solar variations followed the prescription of Lean et al. (1995), while volcanic forcing was based on Sato, et al (1993)."

Page: 43

Line 4 Was there ever really balance to this extent?

**Response:** The question is out of context. The box describes the procedure used to initialize models for radiative forcing experiments, which requires an initial state in near balance.

Line 23 This is a simple example of prioritization. There should be more of it.

**Response:** None required.

Page: 48

Line 8 It should be mentioned that this is not supposed to be the case with GCMs.

**Response:** We believe that point is apparent in the following statement that begins with, "In contrast..."

Page: 50

Line 11 'holistic' may not be the word you want. It has a favorable 'new agey' image that you are not intending.

**Response:** The term was chosen carefully. One definition of holistic is "emphasizing the functional relation between part and wholes," which is our intent.

Page: 51

Line 1 As Kiehl explicitly notes (and as is shown in this report), this is achieved by tuning aerosols. Thus the significance of the achievement is limited.

**Response:** The reviewer's comment is misleading. The inclusion of aerosols was the final piece that brought model simulations in agreement with the 20th century global temperature record. It has been shown repeatedly that the combination of natural and anthropogenic forcing changes is responsible for the observed record.

Page: 53

Figure 2.5 caption It might help to identify the fields.

**Response:** To include all of the fields would be overkill, because the point is that model performance varies depending on the metric chosen, and that there are many metrics. Surface temperature is cited as an example on page 51 where the reference is made to the figure.

Page: 54

Line 10 What about multi-decadal oscillations?

**Response:** These are included in the phrase "...internal "oscillations" such as ENSO. " It is clear in later sections that multi-decade oscillations are considered part of natural variability.

Page: 55

Line 4 This suggests the differences are small. I don't think this is the case.

**Response:** The word "somewhat" was deleted.

Line 13 paleorationalize might be a more accurate description.

**Response:** None required.

Page: 61

Line 30 I'll try to look up some of the problems that even WRF has with clouds. This suggests that fine grids may not be enough.

**Response:** That point is made within the same paragraph.

Page: 63

Line 24 Shouldn't 'required' be replaced by 'allowed?'

**Response:** Wording changed.

Page: 64

Line 2 Please don't confuse the range of model results with the uncertainty.

**Response:** The full phrase is "uncertainty in climate simulation "

Page: 66

Line 6 How do models deal with diurnal boundary layers over land?

**Response:** That is a broader question that applies to all numerical climate simulation. Model parameterizations typically recognize differences between stable and unstable boundary layers (with varying degrees of accuracy), and they tend to have higher vertical resolution near the surface to represent varying boundary-layer stability and depth. Including the full background on boundary-layer representations is beyond the scope of the report.

Line 25 How do differences in tuning impact ensembles?

**Response:** We are not sure what "tuning" the reviewer is referring to - AOGCM tuning or the tuning of the RCMs. Either way, differences in tuning are part of the uncertainty in modeling. Text slightly modified to address this point.

Page: 68

Line 23 Why lower? The warming should first occur near  $\tau=1$  or around 6-10 km depending on latitude.

**Response:** This generally accepted explanation is qualitative and physically correct. No change was made to the text.

Footnote Why not isolate and quantify this component?

**Response:** The footnote was added for accuracy. We are reporting on methods used in the literature. A justification is beyond the scope of the report and the intended audience.

Page: 77

The notion that climate is simply proportional to mean radiative forcing is almost certainly wrong. The success of Milankovich (Roe, 2007) illustrates this.

**Response:** A sentence was added to the last paragraph on page 76. The glacial-interglacial fluctuations of the Pleistocene (the "Ice Ages") are thought to be forced by changes in the Earth's orbit on time scales of 20,000 years and longer—the astronomical theory of the Ice Ages. Since this theory assumes that the mean temperature of the Earth can be altered by changing the distribution of the incoming solar flux, without changing its global mean, it suggests important limitations to simple models based solely on global mean radiative forcing. For the limited purpose of constraining climate sensitivity, we need not understand how glacial-interglacial variations of ice sheets and of carbon dioxide are forced by changes in the Earth's orbit

Page: 79

Line 3 Don't confuse uncertainty with variation.

**Response:** Wording changed

Line 18 In Chou and Lindzen (J.Clim., 15, 2566-2570. 2002) we showed that the data underlying FAT were incorrectly interpreted by Hartmann. I suspect, but I'm not altogether certain, that this is relevant to the FAT hypothesis.

**Response:** We are not claiming that the FAT hypothesis is correct here, simply listing it along with IRIS as having been proposed in the literature

Line 27 This was the whole point of normalization in Lindzen et al, 2001.

**Response:** We agree. No change required

Page: 82

Line 7 As I recall, Manabe and Wetherald assumed fixed relative humidity.

**Response:** The reviewer is mistaken. This is a reference to Manabe and Wetherald's first GCM simulation, in which relative humidity is free to evolve, not to the earlier radiative-convective paper in which they assume fixed relative humidity

Line 9 Given the striking horizontal heterogeneity of water vapor, it seems hard to argue for a simple water vapor feedback without considering variations in the areas of moist and dry regions.

**Response:** Sentence altered to, "Despite the fact that the distribution of water vapor in the atmosphere is complex, we are aware of no observational or modeling evidence that casts doubt of any significance on this basic result, and we consider the increase in equilibrium sensitivity to roughly 2°C from this feedback to be a solid starting point from which the more uncertain cloud feedbacks then operate."

Page: 88

Line 24 This can be misleading. Much of this correlation comes, I suspect, from the gross nature of seasonality rather than from the details that are crucial for climate simulation.

**Response:** We stand by the statement, which is not disputed by the reviewer.

Page: 89

Line 11 Precipitation, it should be noted is crucial to such matters as cumulus detrainment and cirrus coverage. Errors in precipitation cannot be decoupled from other processes involving moisture.

**Response:** The discussion is of patterns of cloudiness and precipitation, not of precipitation and cloud physics. Inclusion of the suggested addition would detract from the discussion.

Page: 94

Line 13 This would appear to contradict earlier statements about internal variability.

**Response:** The sentence was confusing, and thus deleted.

Line 21 This appears inconsistent with the previous comment. It also seems inconsistent with the recent paper by Smith et al dealing with the recent lull in warming.  
**Response:** The statement elaborates on the previous comment by posing possible, albeit unlikely explanations, and is consistent with it.

Page: 100

Line 11 This sounds like special pleading.  
**Response:** None required

Page: 103

Line 28 Given that North et al conclude that we can't speak of anything prior to the Little Ice Age, I find it peculiar that one is still speaking of '1000 years or more.'  
**Response:** The North, et al study found "it plausible that the Northern Hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium" (pages 3-4).

Page: 104

Line 13 As pointed out earlier, these models build in climate sensitivity. Thus, a large measure of simulation is built in.  
**Response:** We agree. No change required to text.

Line 23 There are significant planetary scale transients (Straus and Lindzen, JAS, 2000) and these have periods on the order of 50 days.  
**Response:** That may be true, but we are describing common practice for analyzing mid-latitude storm statistics. In any case, the next sentence states that the results are relatively insensitive to the filter scale chosen.

Page: 108

Line 30 A significant part of the moisture is recycled via evapotranspiration.  
**Response:** We acknowledge the role of recycled evapotranspiration as a second order effect in monsoon dynamics. Nevertheless, the original moisture source is the ocean and the discussion is at the right level for the intended audience.

Page: 110

Line 16 How did the 'successful models' do with the ending of the Sahel drought?  
**Response:** We are referring here to the coupled models, for which it is not meaningful to compare the evolution of Sahel rainfall with observations in detail, since it is known that the Sahel is sensitive to internal variability in the Atlantic. One could refer to AMIP runs, in which several models show an amelioration of the Sahel drought, but that would be a distraction in this context.

Page: 118

Line 15 How was this distinguished from anthropogenic warming?

**Response:** All 20th century simulations (which included anthropogenic forcing) replicated to some extent the warming observed since the late 1970s; both unforced control runs and 20th century simulations obtained occasional periods of warming similar to the observed 1920-1950 episode.

Line 26 How did magnitudes compare?

**Response:** Sentence changed to, "By isolating the multidecadal period of several regions in the ensemble SST series through statistical methods they found the models obtain the observed magnitude of the AMO .

Page: 121

Line 26 I suspect that the isolation is largely confined to the near surface region and is intimately tied to boundary layer physics.

**Response:** We disagree. Citations added to support the statement.

Page: 130

Line 10 'toward' is sort of vague. I don't think the current actually reaches Europe.

**Response:** The parenthetical phrase containing the word was removed.

Line 20 I'm not sure what this sentence actually means.

**Response:** The confusing clause was removed.

Page: 131

Line 11 How do simulations compensate for this?

**Response:** The following sentence was added, "In coupled ocean-atmosphere simulations, erroneous ocean heat transport is compensated by changes in atmospheric heat transport that give a more realistic total heat transport (Covey and Thompson 1989)."

Line 15 The seasonal cycle hardly penetrates the mixed layer.

**Response** The sentence was changed to, "The seasonal cycle and longer-term trends of heat content, provides useful model metrics, although the seasonal cycle does not affect the deep ocean

Line 23 Remember that longer periods involve greater depths.

**Response:** Sentence changed to, "Although the annual cycle involving near-surface layers and global trend are reproduced, model analyses (e.g., Hansen et al. 2005) show they do not simulate decadal changes in estimates made from observations (Levitus et al. 2001)

Page: 136

Line 5 It seems to me that one should stick to meteorological extremes per se without the economic and demographic gloss. After all, more lives will be saved by fewer cold waves.

**Response:** The scope of the report includes examples of model information used for applications. In this introductory paragraph, we are illustrating why extreme events are important.

Page: 137

Line 15 Is this really a robust long term trend?

**Response:** Text clarified and reference added to read, "Perhaps the most prominent observed global trend is an increase in the frequency of heavy precipitation, particularly during the last 20 to 30 years of the 20th century. This trend is significant at the 95% confidence level for the period 1979-2003 and at the 99% confidence level for the period 1951-2003 (Trenberth et al. 2007).

Line 30 You're comparing an observed trend with model variability.

**Response:** We disagree. The referenced paper is a detection and attribution study and makes a signal to noise comparison.

Page: 140

Line 5 This would seem to suggest that eddy intensities are too low.

**Response:** We believe that the conclusion is implied in the remainder of the paragraph.

Line 11 Is this consistent with earlier claims?

**Response:** There is no earlier claim of an observed trend in heat waves. (See discussion on page 136)

Line 24 I'm not sure it was that slight. It apparently was enough to end citrus agriculture in north Florida.

**Response:** None required. The point was that the frost day trends in models and observations were of different sign.

Page: 142

Line 18 This may be wishful thinking. Resolving a cumulus tower with one or two points is unlikely to produce reasonable results.

**Response:** We stand by the statement. A dynamical regime transition occurs at higher resolutions, which require non-hydrostatic formulations that permit explicit simulation of buoyancy driven convection.

Page: 145

Line 15 Especially given the earlier claim that models are replicating internal variability adequately.

**Response:** None required

Page: 146

Line 10 So far, these are unmet challenges for WRF.

**Response:** We acknowledge that this is an active area of research, which is why the discussion in this chapter on future model directions.

Page: 156

Line 15 In the light of earlier statements, it seems inconsistent to maintain that such a procedure would actually span the range of possible future climate states.

**Response:** There was never such a claim made. It is an explicitly stated assumption in the referenced report.

Page: 158

Line 7 Such claims seem to be based on the 'dumb farmer' fallacy.

**Response:** We are confused by the comment. The statement discusses the consequences of a possible erroneous assumption in the methodology.

Page: 159

Line 19 Mortality v. temperature varies markedly with location.

**Response:** The sentence explicitly states, “existing models (Kalkstein and Green 1997) used location-specific absolute magnitudes of temperature to estimate mortality (e.g., Davis et al. 2002).”

Page: 160

Line 20 The language occasionally confuses highly uncertain projections with reality.

**Response:** The paragraph explicitly states that the studies are based on model projections.



**Authors' Response to CPDAC Comments on CPDAC Final Concurrence Version of  
CCSP SAP 3.1 dated March 5, 2008**

After reviewing the response to his review, and the March 5, 2008 version of the report, Prof. Lindzen had further objections, which resulted in the following two changes:

1. The following sentence was removed from the last paragraph of section 5.2.2 (Model Simulation of Observed Climate Variability):  
“Regarding climate variability in the longest time periods, a number of groups have analyzed essentially the same paleodata with differing results, although global warming observed over the past 50 to 100 years consistently stands out as unusual in the context of the past 1000 or more years (North et al. 2006; Solomon et al. 2007).”
2. The following two sentences were added at the end of the first paragraph of Section 5.2 (Twentieth Century Trends):  
“Precise initial conditions, especially deep-ocean temperature and salinity, are not known for 1860. The spread among individual simulations from the same model (the dotted-line curves) thus indicates uncertainty in model simulated temperature arising from lack of knowledge about initial conditions.”