

Compilations of expert reviewers' comments on SAP 2.3

Expert Reviewers:

<u>Name</u>	<u>Affiliation</u>	<u>Email</u>	<u>Comments Received</u>
Sundar Christopher	University of Alabama, Huntsville	sundar@nsstc.uah.edu	8/25/08
Steve Ghan	Pacific Northwest National Laboratory	steve.ghan@pnl.gov	7/21/08
Daniel Jacob	Harvard University	djacob@fas.harvard.edu	7/24/08
John Ogren	NOAA ESRL	john.a.ogren@noaa.gov	8/21/08
Susan Solomon	NOAA ESRL	Susan.solomon@noaa.gov	8/25/08

Our responses are under each comment with text colored.

Over all:

Daniel Jacob:

I found this document to be overall very well informed, objective, comprehensive, accurate, and well written for its intended audience. I think that it is largely acceptable as is but I have a number of comments for the authors' consideration.

All the comments are addressed (see reply for each chapters). All chapters have been significantly revised.

Steve Ghan:

The focus of the report seems to be on results rather than approaches. That makes it difficult to identify specific recommendations for future approaches other than the broad integration of models and measurements theme or appeals for improved understanding. Much more could be said about the present representation of aerosol processes in climate models, to put calls for future improvements in context.

This report (including all chapters) has been significantly revised. Specifically, now Chapter 2 and Chapter 3 review the recent results in observations and modeling, respectively, and discuss the outstanding issues. Chapter 4 now points out the way forward. We have been instructed not to make specific recommendations for future approaches, but to show the future directions.

(Steve Ghan says all yes to the seven questions listed below in John Ogren's review.)

John Ogren:

1. **Are the goals, objectives and intended audience of the product clearly described in the document? Does the product address all questions outlined in the prospectus?**

No to the first question, and yes to the second. The goals and objectives are not described. The prospectus lists two objectives, one for all the CCSP Science and Assessment Products "promote a consensus about the knowledge base for climate change decision support," and one for this particular product "provide a synthesis and integration of the current knowledge of the climate-relevant impacts of anthropogenic aerosols". These objectives are not discussed in the report. The prospectus is quite clear that the intended audience is "policy makers and policy analysts both within and outside the US government and worldwide, interested in these two areas [climate prediction and remote sensing] are the most likely target audience." The report does not describe this intended audience.

John Ogren, NOAA/ESRL

Now the objectives and intended audience have been clearly identified at the beginning of this report (Executive Summary).

- 2. Are any findings and/or recommendations adequately supported by evidence and analysis? In cases where recommendations might be based on expert value judgments or the collective opinions of the authors, is this acknowledged and supported by sound reasoning?**

Yes, and yes.

- 3. Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?**

Yes, and yes.

- 4. Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?**

No, and no. The document does not describe the intended audience, but it appears to be written largely as a scientific review paper where the audience is other scientists. This is not the target audience described in the prospectus.

The document has been very significantly revised with policy makers in mind. The jargons are avoided, the terms are clearly defined and explained.

- 5. Is the document scientifically objective and policy neutral? Is it consistent with the scientific literature?**

Yes, yes, and mostly. Specific comments on inconsistencies with the scientific literature are given in the line-by-line comments below.

- 6. Is there a summary that effectively, concisely and accurately describes the key findings and recommendations? Is it consistent with other sections of the document?**

No, and yes. The Executive Summary is 9 pages long, which I consider excessive. The “looking forward” bullets are an effective way to summarize the key recommendations.

The executive summary has been significantly revised to be more concise and parallel to the chapters. Even though it is still about 8 pages long, the length is appropriated for providing necessary information and summary for the entire report.

- 7. What other significant improvements, if any, might be made in the document?**

I suggest that Chapters 2 and 3 be reorganized. Both chapters discuss models, measurements, and their synthesis, Chapter 2 from the perspective of people making measurements and Chapter 3 from the modellers' perspective. This structure does not

address the information needs of the target audience, where the measurement/model perspective of the authors is irrelevant. A better structure would be to create three chapters, one that discusses measurements and measurement-measurement comparisons, one that addresses models and model-model comparisons, and a third that addresses measurement-model comparisons and synthesis.

We decided to keep Chapter 2 focusing on measurements and Chapter 3 on modeling. Meanwhile, Chapter 2 contains a good fraction about using data to improve/constrain the model and Chapter 3 mentioned the need for data to evaluate the model. Therefore an additional chapter combining data and model is not necessary, but the integrated use of them has been discussed in all chapters.

There have been numerous papers and reports in the past that made recommendations for future directions (e.g., the Seinfeld NRC report, and the PARAGON papers), and many of the past recommendations look very similar to what is being recommended in the current report. I think that it would be useful for the target audience to see a discussion of what has been recommended in the past vs. what is being recommended now, including recommendations for a “way forward” that might lead to a new (not recycled) set of recommendations in 5-10 years.

The previous studies are now summarized, and the significantly revised “The Way Forward” chapter has clearly pointed out the directions that are from this report, not recycled from other reports.

The quality of the figures is uniformly poor, and in many cases the text in the figures is barely readable.

Now all the figures are of high quality.

Sundar Christopher:

This report provides an assessment and review of the effect of aerosols on climate. The report discusses the direct and indirect aerosol effects and how measurements and modeling are contributing to our understanding. The report also provides a brief synopsis on how aerosols may influence precipitation and weather.

The executive summary is broken into several components and in each section it outlines the measurements and modeling frameworks and a ‘looking forward’ section on how to reduce the uncertainties and move the field forward. The goals and objectives are clearly outlined and are compatible with the questions outlined in the prospectus. The entire document relies heavily on various review activities conducted over the past several years and also on the recent IPCC AR4 report. Therefore the current document is consistent with the scientific literature.

The different chapters also provide a good overview of the topic and the important issues that needs to be addressed.

I commend the authors for reviewing, documenting and providing a comprehensive assessment of aerosol impacts on climate while also providing recommendations on how to reduce uncertainties.

While the document relies heavily on CCSP activities of the past and the more recent IPCC report, it will be beneficial to the intended audience if the executive summary included in the form of a table - the significant areas of progress in measurements and modeling. Especially useful will be information on specific improvements as a function of previous IPCC and CCSP efforts.

To keep the ES short and concise, the progresses in measurements and modeling are summarized in short, bulletised sentences and paragraphs, while the tables are contained in Chapters 1-3.

Susan Solomon:

Pages 4-133. I appreciate that many of the authors have worked hard, and on a limited timetable. There is much in the document that is useful and well done, particularly the sections on observations. Thanks.

Pages 4-133. Missing from the document and missing from the summary is a balanced discussion of the fact that the issue of what constitutes an indirect aerosol forcing, and what constitutes a feedback. This is an important matter and is presently unclear. It isn't appropriate to highlight potentially important 'forcings' without making this issue very clear. Please make appropriate additions on page VIII, line 38 and in the main text (see comments on other chapters).

The report has been significantly revised. Now the terms are clearly defined and consistently used throughout the report. We have also added a "Glossary" to reinforce the definitions.

Comments on Executive Summary:

Daniel Jacob:

1. p. vi: somewhere in this 'looking forward' section (and also in the body of the document) it may be useful to pose the problem of how to assimilate satellite measurements of TOA radiances in models as constraints on aerosol loadings. A difficulty in using satellite AOD products to evaluate models is the inconsistency in assumptions of aerosol properties between the model and the satellite retrieval. The document should at least mention this difficulty, which can be avoided by using TOA radiances.

Sentence added to first paragraph of **ES 3.2. Modeling Atmospheric Aerosols:**

"Inconsistency in assumptions of aerosol properties between models and aerosol retrievals may be avoided by comparing TOA radiances instead of AOD."

2. p. vii: I would argue that the largest uncertainty in modeling of anthropogenic aerosols is in the representation of chemical processes, not emissions.

Largest uncertainty no longer claimed to be emissions. New sentence in ES 3.2: "Multi-model experiments indicate that differences in the models' atmospheric processes play a more important role than differences in emissions in creating the diversity between model results."

3. p. vii, also in text of document: there are tons of field data for evaluating model simulations of wet deposition. What is really needed in my view is an improved level of theory for inclusion in models. Cloud-resolving models could be useful for this purpose.

Wet deposition is no longer addressed specifically in the Executive Summary. Subsequent chapters discuss at length aerosol-cloud processes and how in situ data is used to constrain different aerosol and cloud processes.

4. p. viii, also in text: to say that "Uncertainties will likely increase before they decrease" may seem witty but is a bit silly. New science does not increase uncertainty. It may reveal that prior uncertainty was underestimated, but that's a different statement.

Offending sentence did not make it into the revision.

5. p.x, also in text: mention ice cores, lake and ocean sediments as sources of information on past aerosol trends?

Too detailed for executive summary. Discussion of trends removed from Executive Summary.

John Ogren:

[Note: Many of this reviewer's comments were editorial suggestions that are not applicable in the substantially revised document.]

1. Chapter ES, Page iii, Line 21: There should be a discussion of the distinction between forcings and feedbacks in this section.

Discussion of forcings and feedbacks are discussed in the Introduction, but were felt to be too detailed for the Executive Summary.

2. Chapter ES, Page iv, Line 4-5: Are there alternative explanations? For example, could differences in modeled cloud distributions be as important as differences in modeled aerosol forcings?

No longer applicable for ES

3. Chapter ES, Page iv, Line 6-7: This usage of the words “extensive” and “intensive” is jargon, and needs to be explained.

The terms “extensive” and “intensive” are no longer used in this report.

4. Chapter ES, Page iv, Line 12: To what does "this" refer? The preceding sentence, or the entire paragraph?

NA. ES has been significantly revised based on all reviewers' comments.

5. Chapter ES, Page iv, Line 46: The introductory part of the Executive Summary ends with a paragraph on direct forcing, which for balance should be followed by a paragraph on indirect forcings and feedbacks.

Indirect effects given more weight in ES to better balance presentation.

6. Chapter ES, Page v, Line 1: does "their" refer to measurements or properties?

NA

7. Chapter ES, Page v, Line 9: Within NOAA, the acronym “GMD” can denote the Grants Management Division or the Global Monitoring Division of the Earth System Research Laboratory (ESRL). To avoid confusion, I recommend that all references to “GMD” in this report be changed to “ESRL”.

These acronyms are no longer used in the ES. For other chapters the name ESRL is used.

8. Chapter ES, Page v, Line 42: Change “GMD” to “ESRL”.

NA

9. Chapter ES, Page v, Line 43: Change “mulit” to “multi”.

NA

10. Chapter ES, Page vi, Line 1: Measurements of spectral aerosol absorption are currently being made at most long-term stations. The emphasis should be on upgrading the stations with more accurate instruments.

In Section ES 2 Measurement-based assessment of aerosol radiative forcing, sentence now reads: Development and deployment of new and enhanced instrumentation including aerosol mass spectrometers able to determine size dependent composition on fast timescales, and methods for determining aerosol light absorption coefficients and single scattering albedo with greater accuracy.

11. Chapter ES, Page vi, Line 10: add a sentence (or perhaps a separate bullet?) on the need for systematic integration of satellite and sub-orbital measurements, to quantify uncertainty of satellite retrievals and enhance their interpretation.

The final section of the ES includes the following statements: **“Individual sensors or instruments have both strengths and limitations, and no single strategy is adequate for characterizing the complex aerosol system. The best strategy is to make synergistic use of measurements from multiple platforms, sensors and instruments having complementary capabilities.”** AND also “Most importantly, a working synergy between different types of measurements, between different types of models, and especially an enhanced synergy between measurements and models is critical.”

12. Chapter ES, Page vi, Line 13: add a clause at end of sentence, “and at long-term sites”.

NA

13. Chapter ES, Page vi, Line 24: change “between” to “among”.

NA

14. Chapter ES, Page vi, Line 27-29: where is the verb in this sentence?

NA

15. Chapter ES, Page vi, Line 34-36: The need to deal with aerosol forcings on regional scales, rather than global averages, is a key point that should be included in the “looking forward” bullets.

In ES 3.4 this statement is made, “However, since aerosol forcing is much more pronounced on regional scales than on the global scale because of the highly variable aerosol distributions, it would be insufficient or even misleading to place too much emphasis on the global average.” This statement follows from many statements made in other sections concerning the regional nature of the aerosol forcing.

16. Chapter ES, Page vii, Line 39: It would be useful to reconcile the values in the preceding lines with the TOA DCF in IPCC AR4 of -0.5 (-0.9 to -0.1).

Rewritten

17. Chapter ES, Page viii, Line 39: There should be some discussion of which of the various indirect effects are observed.

This is done in detail in subsequent chapters. In the ES we define the “cloud albedo effect”, but avoid the details and jargon of the other indirect effects. The paragraph in the ES reads, “by modifying amounts and microphysical and radiative properties of clouds (the “indirect effects”). Aerosols influence cloud properties through their role as cloud condensation nuclei (CCN) and/or ice nuclei (IN). Increases in aerosol particle concentrations may increase the ambient concentration of CCN and IN, affecting cloud properties. A CCN increase will lead to more cloud droplets so that, for a fixed cloud liquid water content, the cloud droplet size will decrease. That effect leads to brighter clouds (the “cloud albedo effect”). Aerosols may also affect clouds by absorbing solar energy and altering the

environment in which the cloud develops, thus changing cloud properties without actually entering the cloud droplets as CCN.”

18. Chapter ES, Page viii, Line 43: Please specify which IPCC report.
We now are clear whenever specifying an IPCC report.

19. Chapter ES, Page x, Line 8: In addition to being a moving target, the “global aerosol system” is largely an irrelevant concept. The many regional aerosol systems are a more relevant way to address aerosol-climate interactions.
We mention the “global aerosol system” once in the ES. This term has been used previously within the community, and an entire JGR special issue used that term for its title. We feel that the term is useful in the context used.

20. Chapter ES, Page x, Line 9-10: WRONG. The spatial distributions of sources and sinks of greenhouse gases are also changing on decadal time scales.
Offending statement removed.

21. Chapter ES, Page x, Line 17-18: This wording makes it sound like aerosols are responsible for the observed dimming/brightening, which has not been proven. Suggest alternate wording: "needed to end speculation about the role of aerosol changes in the observed dimming or brightening."
Dimming/brightening no longer addressed in the ES.

22. Chapter ES, Page x, Line 27: Shouldn't the way forward include a statement about testing this assumption?
The Way Forward has been completely rewritten.

23. Chapter ES, Page xi, Line 25: “The response to forcing depends on the forcing.” Isn't this so obvious that it's not worth saying?
Offending statement removed.

24. Chapter ES, Page xi, Line 25-28: This isn't much of a way forward. Is this all that your want to recommend be done to improve knowledge of the climate response to aerosol forcing?
The Way Forward has been completely rewritten. The reviewer should note that the committee was tasked specifically not to make specific recommendations. A “summary and assessment” is not a recommendation.

25. Chapter ES, Page xi, Line 32: How is this an improvement? What has been better quantified? How can you demonstrate that this is the most important of all?
NA

26. Chapter ES, Page xi, Line 40-45: These two bullets summarize the way forward for the entire report, not just section 7.
The Way Forward has been completely rewritten.

Sundar Christopher:

1. The executive summary provides a 2 page overview and seven separate sections ranging from measurements of aerosol properties to aerosol interaction with precipitation and weather. However, the 2 page overview merely summarizes the 'accomplishments' in the direct radiative effect (DRE) of aerosols. I recommend that before the start of the individual sections a concise item-by-item overall summary is provided on what is the current state of the art and what needs to be done to reduce the uncertainties. This information is in the individual sections but the intended audience can benefit from a concise itemized summary. It will be very useful if the authors provided a concise assessment of how the aerosol forcing compares with GHG and implications to temperatures rather than citing only Hansen et al (2007) on page X (Line 45).
2. Citing AERONET numbers in a modeling context in section 2.1 may not be appropriate because of the spatial sampling issues.
3. In section 3.1, the reader needs to be informed that the DRE at the TOA and at the surface for clear sky are not entirely from measurements and the standard deviation cited is not an uncertainty estimate but it is the standard deviation among the various methods. New studies have also shown that the definition of aerosol forcing is important to reconcile numbers (Bellouin et al., 2008 - JGR).
4. Section 4.1 needs to start with a short description on how these aerosol indirect effects (AIE) are assessed from an observational perspective. This section also needs to draw from recent work of how meteorology could confound the AIE (e.g. Yuan et al., 2008).

The Executive Summary has been significantly revised with the reviewers' comments incorporated. It now parallels the subsequent chapters, highlighting the most important points. It highlights what we know now without going into detail, and it ends with a Way Forward that shows what the next steps should be, without making specific recommendations.

Susan Solomon:

1. Page VIII, line 38. Please add the following important statement: "Recent studies have raised questions regarding which aerosol indirect terms are appropriate to consider as a forcing, and which are more properly dealt with as a feedback. Current uncertainties in this very fundamental issue make it difficult to quantitatively compare aerosol forcings to forcings due to long-lived greenhouse gases, and also has implications for how these terms are considered and compared in climate models." Please also add this clearly where it is discussed in the main text as well.

The issue of categorizing cloud effects as either forcing or feedback was considered to be too detailed for the Executive Summary. A discussion is included in subsequent chapters. It was decided to define feedback as only resulting from changes to surface temperature.

2. Page III, line 37-39. Here and elsewhere, the issue of regional forcings is highlighted even though it is elsewhere acknowledged that these probably matter very little to climate. Delete this here. Page XI, lines 9-11 have it right and other statements should not contradict this or unbalance its message.

We think the regional forcings are critically important to being able to predict the state of the atmosphere in a changed climate. Spatially variable forcing will initiate or inhibit regional cloud patterns and circulations.

3. Page III, line 45 and page IV, lines 1-14. This is unbalanced and does not belong in an executive summary, particularly not here where it appears you are trying to give overarching conclusions (which ought to be solid ones). What is here is also inaccurate and is presented in the report in a very biased way. Kiehl et al. examined 9 models; there are 23 in the IPCC (2007) report so it is not appropriate to generalize this, and it is also inappropriate to state that there is a 'tuning'. This has not been proven to the level of thoroughness required in an assessment. Delete.

The rewritten Executive Summary is much less detailed. Offending section has been removed.

4. Page IV, line 37. It is inappropriate to recast an IPCC conclusion that clearly stated 'most of the warming of the last 50 years is very likely due to greenhouse gas increases' to instead be about global change, to drop the 'most of' and to change 50 years to 100 years. This is incorrect and must be corrected if a credible report is to be produced. You can say that improved understanding of aerosols has improved our understanding of key aspects of aerosol radiative forcing, particularly the total direct effect. Please then add: 'Aerosol concentrations are believed to have changed very little since about the mid-1970s, the period of which most of the warming of the past 50 years has occurred, and most of the warming of the past 50 years is therefore not very dependent upon aerosol changes but rather upon greenhouse gas increases'

We feel that we are very clear in this matter. The net warming is primarily due to greenhouse gases. However, in order to represent the observed temperature trends, aerosol forcing must be incorporated into the models. However, because of the wide range of how aerosols are represented, predictions remain uncertain. The reviewer's proposed sentence addition is incorrect. We do know that aerosol concentrations have changed significantly on regional bases since 1970, although it is not clear on a global trend.

5. Page VI, line 15, to page XI, line 44. The order of much of this material (and in the main report) does not seem to me to be appropriate. It would be far better to introduce the concept of forcing, then talk about observations, then talk about models. Putting the way things are represented in models first is unbalanced, because it fails to first explain all the many barriers to representing things in models (i.e., observations tell us there are many challenges to doing that) but instead makes it sound as if the models are all bad. Please reorder. Also, please make clear throughout the report when you are talking about aerosol modeling and when you are talking about coupled AOGCMs.

The ES has been completely rewritten with a different structure. We now try to keep clear the differences between CTM and GCMs.

6. Page VIII, lines 37-38. Is this indirect forcing important for local climate responses? Clarify, please.

In the rewritten ES indirect effects are described with greater emphasis with as much detail as is appropriate for the ES.

7. Page IX, line 23. Please add the following statement: ‘Some studies suggest that such high resolution and interactive approaches to calculating feedbacks substantially reduce the aerosol indirect effects compared to less detailed treatments.’

In the rewritten ES indirect effects are described with greater emphasis with as much detail as is appropriate for the ES.

Comments on Chapter 1, Introduction:

This chapter has been almost completely rewritten in response to reviewer comments. Indeed, one of the reviewers (Ralph Kahn) has served as lead author for the revised text. As such, essentially all the points raised have been addressed.

Daniel Jacob:

1. Page 9, last paragraph. "...for the next set of IPCC simulations the prime mode of operation may continue to be off-line simulations..." This will not be good enough for aerosol indirect effects. Online simulations are necessary for at least one simulation per emission scenario. See comments regarding page 132.

The entire section covering the contents of the report has been rewritten.

2. Page 10, inset 1, second paragraph: much attention was paid to this effect of aerosol in the mid-to-late-seventies **eighties** in studying the consequences of a nuclear war.

Inset 1 was entirely eliminated, as the technical details presented are not directly relevant to the material in the report.

John Ogren:

1. Chapter 1, Page 1, Line 30-31: This use of the word "aerosol" is inconsistent with its definition as "a suspension of particles in air". Aerosols contain particles, but they are not particles. Clouds can also be considered to be an aerosol.

Aerosols are now defined as suspended particles within a certain size range, distinguishing them from cloud droplets.

2. Chapter 1, Page 1, Line 42: "thought to be" - why such weak wording? IPCC AR4 states "it is *virtually certain* that anthropogenic aerosols produce a net negative radiative forcing."

The conclusions of IPCC AR4 are now described accurately, and their own terminology is used and explained as part of the discussion of Figure 1.3.

3. Chapter 1, Page 3, Line 8: Misleading. 63% is removed within 3000 km, 37% is transported further.

This has been rewritten.

4. Chapter 1, Page 3, Line 22: Weak wording again, where IPCC AR4 uses "virtually certain."

The relevant IPCC terminology is now used explicitly and explained.

5. Chapter 1, Page 3, Line 24: Comparable on a global scale, but aerosol forcing can be much larger on regional scales.

Right. The regional nature of aerosol impacts is now a major theme of this chapter.

6. Chapter 1, Page 3, Line 39: WRONG. Greenhouse gases do not have uniform distributions. Tropospheric ozone distributions are far from uniform, and CO₂ and methane distributions show hemispheric differences.

Yes. Long-lived GHGs are now specified explicitly.

7. Chapter 1, Page 4, Line 41: "first identified" - The IPCC reports were not the first. This has been completely rewritten and the history corrected.

8. Chapter 1, Page 5, Line 34: Not "far more of an offset", rather "can exceed the magnitude of the GHG forcing, leading to a net negative TOA forcing for the region". Probably should caution again about uncertainty of adding forcings.

See answer to Point 5 above.

9. Chapter 1, Page 6, Line 19: Should include a reference to the Seinfeld NRC (1996) report "Aerosol Radiative Forcing and Climate Change".

Done.

Points 10-15. This text has been entirely rewritten, so the suggested copy edits no longer apply.

10. Chapter 1, Page 6, Line 20: Change "to" to "for".
11. Chapter 1, Page 7, Line 18: delete comma.
12. Chapter 1, Page 7, Line 19: change "develop" to "estimate" or "evaluate"
13. Chapter 1, Page 7, Line 20: change "are" to "is"
14. Chapter 1, Page 8, Line 18: Confusing references to IPCC reports, sometime the year is used and sometimes the report name ("AR4") is used. Pick a single style and stick with it.
15. Chapter 1, Page 9, Line 23: fix spelling of "distributions".

Points 16-24. The three Inserts, formerly on pages 10-12 of the chapter, were eliminated in the revised version (see response to other reviewers).

16. Chapter 1, Page 10-11, Line 1-44: It's not clear why these insets are even included. Who is the target audience, and is this information that they need and don't already know?
17. Chapter 1, Page 12, Line 3: It is appalling to see an incorrect definition given for the aerosol extinction coefficient. It is a dimensioned quantity, not a fraction!
18. Chapter 1, Page 12, Line 9: It is equally appalling to see an incorrect definition given for the aerosol optical depth. It is a dimensionless quantity, not a length!
19. Chapter 1, Page 12, Line 11: Be consistent throughout the report of the spelling of Ångström. It is a proper noun, and the diacritical marks over the "A" and "o" are both required. Check the entire report.
20. Chapter 1, Page 12, Line 27-28: An individual particle does not have a size distribution
21. Chapter 1, Page 12, Line 29: add qualifier "all else being equal (composition and size distribution)"
22. Chapter 1, Page 12, Line 33: what does "aerosols become .. more scattered" mean?

23. Chapter 1, Page 12, Line 33: change “more cloud droplet number” to “higher cloud droplet number concentrations”
24. Chapter 1, Page 12, Line 44: should define TOA in the inset

Sundar Christopher:

1. Chapter 1 – Introduction is concise but it lacks some relevant details that will benefit the intended audience. For example, the chapter should state clearly the current understanding. It also requires a Table of aerosols, relevant processes that generates these aerosols and the average life times. Section 1.2.2 will benefit if a discussion on how our understanding on aerosols has improved from the previous to the current IPCC reports is included. It will be useful to include quantitative numbers throughout this section (e.g. Line 28, Page 5, how much is ‘much greater’). Page 7, Line 4, how much is quite well known. Page 7, Line 14-17 needs references. Page 7, Line 25/26, how much is ‘short time’? Page 9, Line 45, Reword ‘saving of aerosol distributions’.

Current understanding in now more completely covered in this chapter, the requested table has been added, aerosol lifecycle processes are now described, a summary of relevant IPCC report results is given, and numbers are provided in Section 1.2 for radiative forcing. The specific edits required are no longer relevant, as the entire section has been rewritten.

2. Inset 1 on page 10 is difficult to read since there are numerous parentheses. Please restructure sentences.

Inset 1 was entirely eliminated, as the technical details presented are not directly relevant to the material in the report.

3. Page 12, Line 30. Dust could ‘become’ hydrophilic if mixed with other aerosol types. Inset 3 was replaced with a Glossary at the end of the overall document, and the contents entirely rewritten.

Comments on Chapter 2, Measurements:

Daniel Jacob:

1. p.18: the text about CTMs being used to “interpolate and supplement” field observations seems way too optimistic. We are not at that level in terms of observational quality or coverage.

We have reworded the sentence.

2. p. 20: IMPROVE should not be included in Table. Extinction is not measured, just chemical composition. If you have IMPROVE then you should also include EMEP and other air quality networks.

According to their web site, IMPROVE measures light scattering coefficients with nephelometers at a number of sites (<http://www.nature.nps.gov/air/monitoring/MonHist/parameter.cfm>). In addition, we now mention the EMEP and WMO GAW networks in the text.

3. p.27: some mention of LITE in box 2.4?

Yes, it has been spelled out and referenced.

4. p.27, caption of Fig 2,2: A-train is in present, not future.

Fixed.

5. p.29: the comment about 30% increase in DRE and DCF relative to a priori obviously depends on what a priori is used. Not very helpful.

Deleted.

6. p. 30-35, also p. 53: section 2.3.1 is just a lengthy summary of the Bates et al. paper. Such parochialism doesn't seem appropriate for a review document.

This section has been changed substantially. The detailed description of the Bates et al. paper has been removed with only fundamental results described. Major results of aerosol characterization from NEAQS, MILAGRO, and ICARTT have been included. References to other work (e.g., Koch et al., 2007; Shindell et al., 2008) have been added. The section now reports on three ways in situ measurements can be used to improve models: validation of CTMs with measured in situ aerosol properties, use of measured optical properties in RTMs, and development of simplifying parameterizations for use by RTMs based on field observations.

7. p.37: in figure 2.12, define alpha. Reflectivity?

Alpha is broadband surface albedo. To avoid confusion with “alpha” for aerosol mass extinction efficiency, we are now using A for the surface albedo. Also this figure has been moved to Chapter 1 introduction (i.e., Figure 1.4) to discuss sensitivity of aerosol direct forcing to AOD and the measurement requirement.

8. p.40: figure 2.14 is messy and figure 2.15 is not helpful.
Both figures have been deleted.

9. p.43: MODIS data over land have bigger problems than 10-15%.
Reworded.

10. p.48: section 2.4.1 is weak. It doesn't tell me why resolving vertical distribution is critical.

The section is re-structured and importance of resolving aerosol vertical distribution is now discussed in section 2.4.3.

Steve Ghan:

1. Page 29, second paragraph. **Constraining models with in-situ measurements.** Should also mention that measurements of extensive properties of aerosol can be used effectively to evaluate CTMs on a regional basis, which can suggest improvements in such uncertain model parameters as emissions factors and scavenging coefficients, thus increasing confidence in regions and periods where and when observations are not available.

Added a paragraph that reads "Short-term, focused measurements of extensive aerosol properties (e.g., aerosol concentration and AOD) also can be used to evaluate CTM parameterizations on a regional basis in order to suggest improvements in such uncertain model parameters as emission factors and scavenging coefficients (e.g., Koch et al., 2007). Improvements in these parameterizations using observations yields increasing confidence in regions and periods where and when measurements are not available. To evaluate extensive properties generated by CTMs on broader scales in space and time, satellite observations and long-term in situ measurements are required."

2. Page 29, third paragraph, **Integration of satellite measurements into model simulations.** Should note that GCMs used to simulate the pre- and post-satellite eras cannot assimilate satellite measurements. Thus, a valuable role for satellite retrievals is to evaluate the GCM simulation during the satellite era. Model refinements guided by such evaluations can then be used to improve simulations of the pre- and post-satellite eras.

We agree. The paragraph now reads: "Global measurements of aerosols from satellites (mainly AOD) with well-defined accuracies offer an opportunity to evaluate model simulations at large spatial and temporal scales. The satellite measurements can also be used to constrain aerosol model simulations and hence the assessment of aerosol direct radiative effect through data assimilation or objective analysis process Recent efforts have also focused on retrieving global sources of aerosol from satellite observations using inverse modeling, which may be potentially valuable for reducing large aerosol simulation

uncertainties (Dubovik et al., 2007). Model refinements guided by the model evaluation and integration practices with satellite retrievals can then be used to improve aerosol simulations of the pre- and post-satellite eras”.

3. Page 53, last sentence. If Bates et al showed that the estimated DRE using measured optical properties and simulated aerosol mass is greater by 30% and that estimated using a priori optical properties, then that bias could partially explain the difference between the AOD retrieved by satellite and estimated by models.

As pointed out by other reviewer, the 30% increase in DRE and DCF relative to a priori obviously depends on what a priori is used. Note that the satellite-model differences were examined using models that are not the same as those used in Bates et al. (2006). So we are hesitant to make the suggested claim.

John Ogren:

1. Chapter 2, Page 20, Line 1-27: It would be helpful to provide a citation and/or URL for each network

Done.

2. Chapter 2, Page 20, Line 7: ARM only has one mobile facility. It has no sites in Europe or Asia, although the mobile facility has been deployed on those continents.

Fixed.

3. Chapter 2, Page 20, Line 10: Change “GMD” to “ESRL”.

Done.

4. Chapter 2, Page 20, Line 11-15: The NOAA/ESRL network measures the hemispheric backscatter fraction, not the upscatter fraction. There are five baseline stations in the NOAA/ESRL network.

Fixed.

5. Chapter 2, Page 21, Line 1: The ARM Aerosol IOP is not listed in Table 2.1.

Now listed.

6. Chapter 2, Page 21, Line 2: Bad grammar, “that targeted at characterizing”

Fixed.

7. Chapter 2, Page 21, Line 40: Be consistent with notation, chapter 3 uses a different symbol to denote single-scattering albedo

Fixed.

8. Chapter 2, Page 22, Line 22-23: Change “GMD” to “ESRL”. The citations refer to the in-situ measurements by this network, which don’t belong in a section on remote sensing.

Fixed.

9. Chapter 2, Page 22, Line 37-43: The WMO Global Atmospheric Watch should be mentioned in this section. The paragraph is written as if the role of the ground-based networks is to support intensive field campaigns. They have a greater role than that, for example, determining trends, providing climatologies of aerosol radiative properties used by the models, and providing long-term data sets for testing models.

We now mention the WMO GAW network as well as other national and international networks in Section 2.2.2. We also have added text describing more fully the information obtained from long-term surface measurements as suggested by the reviewer.

10. Chapter 2, Page 22, Line 39: Change “GMD” to “ESRL”.

Done.

11. Chapter 2, Page 22, Line 46: Later on in this report, the need for characterization of aerosol properties in the column is discussed. Chapter 2.2 would be a good place to discuss in-situ aerosol profiling programs (DOE/ARM, NOAA/ESRL, UCSD/Scripps).

We have added a section (2.2.3) describing the regular flights over Bondville and Southern Great Plains.

12. Chapter 2, Page 28, Line 21-22: There are plenty of other reasons for the uncertainty of model simulations, for example, the difficulty of properly representing cloud processes.

We agree and the point has been added.

13. Chapter 2, Page 31, Line 11: The text discusses “BC”, while the figures use “EC”. It appears that these two terms are being used as synonyms, which is not correct.

This section has been significantly modified. The referred figure and text have been removed.

14. Chapter 2, Page 41, Line 21,26: The standard error used here is the standard error of the mean, and so it is appropriate to report it along with the mean, not the median.

Fixed.

15. Chapter 2, Page 46, Line 4-5: I agree completely with this statement. It should be featured prominently elsewhere, like in the executive summary and as an integral part of the “way forward” chapter. This role of uncertainty analysis was a central feature of the recommendations of the Seinfeld NRC (1996) report.

We agree. Executive summary & Way Forward????

16. Chapter 2, Page 48, Line 42: How can lidars “well constrain the aerosol-induced atmospheric heating rate” when most are not sensitive to aerosol light absorption?

We have reworded the sentences.

17. Chapter 2, Page 50, Line 34: change “inversed” to “retrieved”

Done.

18. Chapter 2, Page 52, Line 3: Suggest re-wording the beginning “To quantify regional-scale trends...”. Global scale trends are not particularly relevant, and local trends can be measured with ground-based instruments.

We agree. Done.

19. Chapter 2, Page 53, Line 38: This section is too long, and there is a lot of repetition of material already presented.

Reworded to avoid repetition.

20. Chapter 2, Page 71, Line 22: Change to “ESRL Earth System Research Laboratory (NOAA)”.

Done.

Sundar Christopher:

1. Much of Chapter 2 comes from a recent review of Yu et al (2006) and it may require some modifications and additions to include papers since 2006. Table 2.1, - include DABEX to table (Haywood et al., 2008, JGR). Page 23, Line 18, Reference for OMI. Newer algorithms also exist for OMI other than Torres et al. Example – Veihelmann et al (2007-ACP) and others.

Yes, updated. A general note: this team have taken a serious look at those potential references and chosen most important studies to cite. It is clear that we have added a large amount of major results in this revision. As one can understand, not all published papers should be included in this report.

2. Page 25, Line 8 needs accuracy estimates for Deep Blue since MODIS operational algorithm estimates are provided.

Added.

3. Page 28, Lines 39-45 and Page 29, Lines 1-3 are an abrupt transition in the closure studies section. Rather than discuss a specific study it will be useful to generalize the main points.

Done.

4. Section 2.3.1 does not appear to fit in this report for various reasons. Although I agree that there needs to be a discussion on how insitu measurements can improve modeling estimates of DRE, this entire section is taken from one paper – Bates et al (2006). The report can simply provide the necessary relevant information and reference the Bates et al (2006) paper for further reading.

This section has been changed substantially. The detailed description of the Bates et al. paper has been removed with only fundamental results described. Major results of aerosol characterization from NEAQS, MILAGRO, and ICARTT have been included. References to other work (e.g., Koch et al., 2007; Shindell et al., 2008) have been added. The section now reports on three ways in situ measurements can be used to improve models: validation of CTMs with measured in situ aerosol properties, use of measured optical properties in RTMs,

and development of simplifying parameterizations for use by RTMs based on field observations.

5. Page 36, Lines 21 and 22 requires some discussion. By how much have these differences reduced? I agree that there are differences among satellite-derived products but this does not necessarily mean that they can be attributed to cloud contamination only and therefore the discussion on Page 26, Lines 30-33 needs to be expanded.

The reduction of MODIS-MISR AOD difference has been specified based on Levy et al. (2007b) and Kahn et al. (2005b). A number of reasons have been identified for the MODIS-MISR difference, as discussed in Kahn et al. (2007). We never believe that cloud contamination is the only factor.

6. Page 39, Line 43-44, reference Zhang et al (2005, JGR) who addresses cloud contamination issues and how to correct for these.

Added.

7. Most, if not all of section 2.3.3. is from Yu et al (2006) and it is appropriate for this report. However, it is important to either have a separate section or some write-up on what has been done since that time. For example, Patadia et al (2008) GRL now show that with improved angular models for land areas from CERES a land DRE is possible from MODIS/MISR/CERES. – Page 44, Line 11. Chand et al (2008) address issues related to vertical distribution of aerosols from CALIPSO. Bellouin et al (2008) discuss the importance of using the correct definitions for forcing, etc.

We have included these references in this revision.

8. At the end of this section and before the start of 2.3.5 it will be useful to provide a concise itemized summary of what the state of the art is and what are the next steps forward. This is sprinkled throughout the text (example, page 45, Lines 25-29) and it will be useful to have this all in one place.

We have re-organized this section to address the comment. But we have left “the next steps forward” for the report’s chapter 4 (The way forward). In chapter 2, we focus on the summary of current understanding (i.e., progresses and issues).

9. Section 2.3.5 on aerosol-cloud interaction is definitely weaker when compared to the DRE section. This section could be separated into ice and water cloud effects. Then the water cloud impacts should be discussed in terms of the various indirect effects. There have been various review papers regarding the AIE and synthesizing this could be extremely useful.

This section has been substantially enhanced. After a brief introduction, we now have two sub-sections: 2.3.5.1: Remote sensing of aerosol-cloud interactions and indirect effects (both satellite and ground-based); and 2.3.5.2: In-situ studies of aerosol-cloud interactions. These two sections give a comprehensive summary of what we have learned from different approaches, what are major reasons for the differences in the aerosol-cloud interactions derived from different approaches/studies, and what are

current measurement accuracy for key variables. A number of important studies have been referenced.

10. A few items seem out of place in Section 2.4. Section 2.4.1 starts with a discussion of aerosol vertical distributions but then it quickly moves to dust thermal effects. The vertical distributions of clouds and aerosols are important for both the solar and thermal effects. Section 2.4.2 should include recent papers for over land studies.

Re-organized.

11. Section 2.4. also require newer references – example, Yuan et al , Jones and Christopher, Qaas et al and some discussion on how meteorology could confound the cause-effect relationships that are usually explored.

See our response to your comment #1.

12. The concluding remarks in section 5 in pages 54 and 55 could be broken into a concise format that will allow the reader to gauge what the next steps might be.

Thanks for the suggestion and the section is streamlined.

Comments on Chapter 3, modeling:

Daniel Jacob:

1. p.83 : “required” -1.2 W m⁻² – explain
ACCEPTED
2. p.84-: section 3.2 on comparing aerosol observations and models is just an extended summary of the Liu et al. 2006 and Ginoux et al. 2006 papers as “examples”. Also a lot of reliance on Lacis, personal communication. That seems too parochial for an assessment paper. The beginning of the section repeats material from the Intro.
ACCEPTED; ORDER AND EMPHASIS SOMEWHAT CHANGED
3. p.105: not clear why scavenging depends on composition. Even hydrophobic particles would be scavenged by washout. Not clear why the “sulfate plus insoluble paradigm” may become less applicable.
Different aerosol components have different solubility when they are freshly emitted, so they have scavenging efficiencies. When they are aged, the difference appears very small. We change the wording in the text. The other sentence is also modified to make it more clear.
4. p.106: I’m not convinced that ‘extensive observations, under difficult conditions’ (what are we talking about here?) are what’s needed to improve the treatment of updrafts and associated droplet activation in models. Theoretical work leading to improved model parameterizations seems like a more productive direction.
ACCEPTED; CHANGED EMPHASIS
5. p.118: Heald et al. 2005 reference missing
IN APPENDIX A2 NOW REMOVED; IT WAS USED IN CHAPTER 2 NOW
6. p.119: I don’t think that the state of knowledge of SOA warrants a statement about isoprene being possibly the principal precursor.
APPENDIX REMOVED. SOA IS NOW DISCUSSED IN CHAPTER 2

Steve Ghan:

1. Page 90, third paragraph. Excessive black carbon absorption would decrease, not increase the single-scattering albedo.
ACCEPTED
2. Page 96, line 2. Replace donates with conditions
ACCEPTED
3. Page 98, figure 3.16 is same image as figure 3.14.
ACCEPTED; RIGHT FIGURE SUBSTITUTED

John Ogren:

1. Chapter 3, Page 83, Line 27: where is the “following figure”? Please specify the figure number.

ACCEPTED, but figure removed

2. Chapter 3, Page 84, Line 21: Aerosol scattering is never “primarily backward”. Rayleigh scattering is equal in forward and backward hemispheres, while forward scattering is greater than backwards scattering for Mie and geometric scattering.

ACCEPTED

3. Chapter 3, Page88 , Line7 : “Qualitative agreement” isn’t particularly useful here, we need to be developing and using quantitative measures of agreement. Otherwise, we just continue to wave our hands, and are unable to demonstrate the extent to which model or measurement improvements are reducing uncertainties.

ACCEPTED

4. Chapter 3, Page 91, Line 27-29: Why bother with this comparison, then?

ACCEPTED; PHRASE REMOVED AND COMPARISON EXPLAINED BELOW

5. Chapter 3, Page 96, Line 2: replace “donations” with “emissions”

ACCEPTED, EXCEPT USED “CONDITIONS”.

6. Chapter 3, Page 97, Line 33-34: Even if the full scope is beyond the framework of this document, it would be useful to provide a summary of the most important issues.

SOME DETAILS MENTIONED IN CHAPTER 4

7. Chapter 3, Page 100, Line 44-45: *The distinction between meteorological feedback and aerosol forcing can become quite opaque.* I agree. But do you really want to leave this statement hanging out there, and force the intended audience to figure out how to interpret it? This report could make a valuable contribution if it were to explain the importance of the distinction between forcing and feedback to a policy-maker who has to make policy decisions based on this scientific discussion.

ACCEPTED; DEFINITION PROVIDED

8. Chapter 3, Page 101, Line 10: “constants tuned” This is a very troubling clause. To what extent are any of the models tuned go give a particular response? What are the consequences of such tuning for the credibility of model-derived forcings?

ACCEPTED; OBSERVATIONS USED FOR CALIBRATION NOW REFERRED TO

9. Chapter 3, Page 106, Line 18: From the earlier discussions, I thought that there was clear observational support for the first part of the chain:
increased aerosol -> increased cloud drop concentration -> smaller drops

PHRASE REMOVED AND PARAGRAPH PROVIDES EXPLANATION

10. Chapter 3, Page 106, Line 27-30: Didn't Baker and Charlson (Nature, 1990) make a similar point? If so, you should cite them here.

ACCEPTED; ADDITIONAL POINT IS NOT MADE

11. Chapter 3, Page 108, Line 10-12: This point is worth emphasizing (“shout it from the rooftops”) in the executive summary... we need much better constraints on the aerosol forcing in order to adequately constrain the climate sensitivity factor. This report will be a failure if the target audience doesn't get this take-home message.

ACCEPTED

12. Chapter 3, Page 108, Line 23: The word “effect” can include both forcings and feedbacks. You're just talking about forcings here, so say “forcing”.

ACCEPTED

13. Chapter 3, Page 109, Line 43-45: There's not much of a constraint if the vertical profile of aerosol absorption isn't known. That reinforces the need for measuring aerosol light absorption aloft.

ACCEPTED; PHRASE MODIFIED

14. Chapter 3, Page 110, Line 21: The objective isn't to quantify speculation, it is to eliminate speculation and replace it with quantitative results.

ACCEPTED

15. Chapter 3, Page 115, Line 18: Does the target audience really need this appendix?

ACCEPTED; Appendix removed

16. Chapter 3, Page 115, Line 25: missing $n(r)$ after “where”.

Appendix removed

17. Chapter 3, Page 116, Line 23-24: replace “scattering” with “extinction”

Appendix removed

18. Chapter 3, Page 119, Line 18: Very interesting appendix, but what's the bottom line? That target audience needs to know the uncertainty of aerosol forcing due to uncertainty of SOA formation.

ACCEPTED; Appendix removed

19. Chapter 3, Page 120, Line 1: This is a very interesting appendix. It raises the question of which discussions should be relegated to appendices and which belong in the body of the report. As it stands now, the body of the report is so weighted down with details that it's hard to see the big picture. Reorganization into a more rational structure should help, but I suggest considering techniques that will make it easier for the target audience to see the forest instead of a collection of trees. Sidebars and appendices should both be considered.

ACCEPTED IN TERMS OF RORGANIZATION BUT APPENDIX REMOVED

Sundar Christopher:

1. Chapter III draws heavily upon one published study (Liu et al, 2006) and my suggestion is to reduce the number of figures from Liu et al by simply providing an overview and referring the reader for more details to this paper.

ACCEPTED; FIGURES REMOVED AND SECTION SHORTENED

2. On page 91, the Penner et al (2002) reference can be updated or a better discussion can be provided. The ERBE provides clear sky fluxes – not reflectances (Page 32) and the CERES data has much improved calibration and newer algorithms with better flux estimations. See papers by Loeb et al (2005). The CERES has much better cloud clearing techniques when compared to the ERBE.

TAKEN INTO ACCOUNT

Susan Solomon:

1. Page 75, lines 41-46. Same issue as my comment above. You cannot recast an IPCC conclusion that clearly stated ‘most of the warming of the last 50 years is very likely due to greenhouse gas increases’ to drop the ‘most of’ and to change 50 years to 100 years. This is incorrect and must be corrected. You can say that improved understanding of aerosols has improved our understanding of radiative forcing. Please then add: ‘Aerosol concentrations are believed to have changed very little since about the mid-1970s, the period of which most of the warming of the past 50 years has occurred, and most of the warming of the past 50 years is therefore not very dependent upon aerosol changes but rather upon greenhouse gas increases’

ACCEPTED IN PRINCIPLE; DIFFERENTIATION BETWEEN THE REALITY OF GREENHOUSE WARMING, WHICH THIS DOCUMENT DOES NOT DISPUTE, AND CALCULATION OF CLIMATE SENSITIVITY IS NOW MADE MORE CLEAR

2. Page 76, lines 1-13. The obviously sarcastic use of the phrase ‘inconvenient truth’ is unprofessional and inappropriate in a scientific assessment. Further, the rest of the paragraph is biased and overgeneralizes, as noted in my comments above regarding the Kiehl et al. paper. Kiehl et al. examined 9 models; there are 23 in the IPCC (2007) report so it is not appropriate to overgeneralize this, and it is also inappropriate to state that there is a ‘tuning’. This has not been proven to the level of thoroughness across the full set of models required in an assessment. Also, Kiehl et al. do not reach the conclusion that this implies a tuning. Finally, it is also unbalanced and inappropriate in an assessment to refer to the Schwart et al. paper, and a news article by a journalist (Kerr), and omit the scientific rebuttal provided by Knutti et al., (2008). The Knutti et al. paper, among other matters, made clear that the last 50 years are what is key for the attribution debate, and it also showed why overgeneralizations about climate sensitivity should be avoided given the important role of ocean heat content and climate inertia. This paragraph has many problems and should be deleted in its entirety.

AGAIN, ACCEPTED IN PRINCIPLE; SEE COMMENT ABOVE. 'OFFENDING' PHRASE REMOVED.

3. Page 76, line 40. Clarify what is meant by range. Please ensure that all comparisons are quantitative, with appropriate discussions of uncertainties. You cannot just talk about a 'range' because that has many different meanings.

ACCEPTED; RANGE NOW GIVEN

4. Page 103, lines 33-42. Needs a better discussion of why indirect effects may be difficult to constrain.

ACCEPTED; more discussion added

5. Page 104, line 35. Please add: "In this case, the effect is a feedback and not a forcing."

ACCEPTED

6. Page 105, line 42. Please do not use colloquial and unclear phrases like 'achilles heel' in an assessment. The nonspecialist will not know if this mean the models are dead, dying, unfixable, or what.

ACCEPTED

7. Page 107 lines 3-23. This discussion needs error bars. Assertions about inconsistencies can only be made if it is clear what the error bars are on things being compared.

ACCEPTED; ERROR BARS NOW PROVIDED

8. Page 108, lines 1-12. This is oversimplified. Among other reasons for particular choices among model studies are other forcings –e.g., a model that does not include tropospheric ozone nor black carbon because of computational limitations might choose to reduce uncertain aerosol terms to compensate. This doesn't mean 'tuning' is going on but rather that reasonable choices are being made for practical reasons. Please add this.

ACCEPTED IN PRINCIPLE; PARAGRAPH HAS BEEN ALTERED AND 'TUNING' DROPPED

9. Page 109, line 34. Please add a reference to chapter 9 of IPCC (2007), which also made clear that a detailed analysis of patterns in space including SH/NH gradients and height gradients can be used to place important constraints on aerosols.

ACCEPTED

Comments on Chapter 4, Way Forward:

Daniel Jacob:

1. p.128: paragraph on aerosol trends is begging for a few references.
ACCEPTED
2. p.129: in terms of future model improvements, how about being able to simulate TOA reflectances for comparison to satellite data as constraints on aerosol loadings?
ACCEPTED AND PUT INTO THE TEXT IN SEVERAL PLACES
3. p. 129-130: again the draft plays up uncertainties in emissions, but I think uncertainty in chemical processes is more severe.
ACCEPTED AND THE PROPER BALANCE BETWEEN THESE TWO UNKNOWNNS IS RESTORED
4. p. 130: it might be stated that progress in aerosol simulation in GCMs is tied to progress in representation of the hydrological cycle.
ACCEPTED

Steve Ghan:

1. Page 132, first paragraph. It is time to move beyond the externally-mixed treatment of past climate models. Treatments of internal mixing are available and have been applied to the GISS and NCAR models. Treatments of the droplet nucleating and optical properties of internal mixtures are available and have been applied to at least the NCAR model. Whether those schemes will be used in the next IPCC assessment will be decided in the next few months. But certainly within the next decade (the apparent timeframe of the recommendations) such treatments will be incorporated in climate models.
ACCEPTED – COMMENT ADDED
2. Page 132, third paragraph. This critical paragraph really leaves the reader hanging, concluding that “This approach, however, would fail to provide much of the necessary information”. I suggest a way out, which may be what was intended, but is not explained clearly. Incorporate all of the advanced aerosol processes in the GCMs, simulate them once for each emissions scenario, and then, if the aerosol processes are too expensive to run in all other realizations of the scenarios, in the subsequent simulations read in the monthly mean aerosol from the history of the online simulations. It is possible that aerosol variations on submonthly time scales are important, but preliminary experiments by me suggest that estimates of aerosol direct and indirect effects using online and offline aerosols are comparable. I suggest the following changes. First, in the first line of the second paragraph on this page, insert “climate” before “models”. Then replace the third paragraph with the following: “All these processes will require observations to understand them, and extensive computer time to simulate them. If the added complexity is too

computationally expensive for all of the ensembles of climate simulations expected for IPCC, a solution would be to treat the aerosol processes online in the climate model for only the first member of the ensemble for each aerosol emissions scenario. For the other members, the processes that change the aerosol could be replaced with temporal interpolation of the monthly mean aerosols from the first member of the ensemble. Preliminary experiments with such a configuration (Ghan, personal communication 2008) suggest that estimates of aerosol direct and indirect effects using offline aerosol are comparable to estimates using online aerosols. The interaction between clouds, relative humidity and aerosol on sub-monthly timescales is evidently unimportant provided the monthly means are constrained to be the same". This might be bit long. The last sentence could be omitted.

ACCEPTED IN PRINCIPLE. THE PARAGRAPH HAS BEEN REWRITTEN, WITH A MORE EXPLICIT DISCUSSION OF 'THE NEXT STEPS'.

John Ogren:

1. Chapter 4, Page 123, Line 1: 4.2 reads like requirements from the measurers point of view. 4.3 reads like the requirements from the modellers point of view. What's needed is the synthesis of those two sections, and elimination of duplication, as the reader cares about the way forward, not the writer's perspective.

ACCEPTED IN PRINCIPLE. SOME DEGREE OF SYNTHESIS WAS DONE BY INCORPORATING THE OBSERVATIONS NEEDED TO IMPROVE MODELS TOGETHER WITH THE MODELING.

2. Chapter 4, Page 123, Line 43: What are the spatial and temporal averaging scales that correspond to the target uncertainty?

ACCEPTED; SCALES ADDED

3. Chapter 4, Page 124, Line 2: Need to specify that the models being assessed are global, as many more parameters can be evaluated with regional-scale models.

ACCEPTED

4. Chapter 4, Page 124, Line 5: It's a current goal as well as a future goal.

ACCEPTED

5. Chapter 4, Page 124, Line 7: Shouldn't the future goal be for the model to calculate all the required aerosol properties, rather than specify them?

ACCEPTED

6. Chapter 4, Page 124, Line 26-28: Isn't figuring off the optimal tradeoff a research topic in itself? Is this research part of the way forward?

ACCEPTED

7. Chapter 4, Page 124, Line 36: We're all a bit tired by this chapter, but isn't the phrase "observations are needed to observe" just a wee bit meaningless?

ACCEPTED

8. Chapter 4, Page 125, Line 9-11: Suggest adding a sentence that we have the foundation for such a network (WMO/GAW, with NOAA/ESRL providing the US contribution), but that additional measurements and locations are needed?

ACCEPTED

9. Chapter 4, Page 125, Line 12: replace “do” with “to”, and “relation” with “relationships”

ACCEPTED

10. Chapter 4, Page 126, Line 7: Change “GMD” to “ESRL”.

ACCEPTED

11. Chapter 4, Page 126, Line 7: The NOAA network began operation over three decades ago.

ACCEPTED IN THE SENSE THAT WE HAVE NOT LIMITED THE DISCUSSION (WHICH INCLUDES AERONET) TO ONLY ONE DECADE.

12. Chapter 4, Page 126, Line 11: replace “systems” with “systematic”

ACCEPTED

13. Chapter 4, Page 126, Line 14: replace “remote sensing” with “observations”

ACCEPTED

14. Chapter 4, Page 126, Line 16: replace “rind” with “ring”

ACCEPTED

15. Chapter 4, Page 126, Line 35-38: Too satellite-centric. Need to make it clear that the synergy includes suborbital platforms. The way forward for future satellite missions needs to have integrated suborbital measurements as part of the strategy. That point is made later in the paragraph, but by then it seems like an afterthought.

ACCEPTED

16. Chapter 4, Page 128, Line 3-4: This is where the need for in-situ measurements of aerosol vertical profiles is apparent, but not explicitly discussed.

ACCEPTED

17. Chapter 4, Page 128, Line 22-25: The attribution also requires determination of trends in cloudiness and cloud albedo.

ACCEPTED

18. Chapter 4, Page 128, Line 31: “Cross-pollination” sounds so casual and random, it occurs when a bee happens to buzz past, whereas what is needed is a very systematic and deliberate integration of observations and models.

ACCEPTED

19. Chapter 4, Page 129, Line 17-19: This is another one of those key take-home messages that need to be highlighted in the executive summary.

ACCEPTED

20. Chapter 4, Page 131, Line 23: replace “would be valuable” with “is required”
TAKEN INTO ACCOUNT

21. Chapter 4, Page 132, Line 28-29: Not a very optimistic note to end the “way forward” chapter on. It leaves the reader thinking that we can't currently define an adequate way forward.

ACCEPTED – ENDING HAS BEEN CHANGED.

Sundar Christopher:

1. Page 124 (lines 1 to 4) are not consistent with other sections and needs to be reworded. There are several satellite sensors that have improved algorithms for over land such as MODIS, MISR and OMI and these improvements are indeed reducing uncertainties and allowing for newer research studies.

ACCEPTED

2. While section 4 is useful it may benefit the reader if there was a summary table that provided a synthesis of the way forward in all measurement and modeling areas.

ACCEPTED – THE WAY FORWARD IS NOW WRITTEN IN A CONCISE WAY WITH POINTS CLEARLY LAID OUT