U.S. Environmental Protection Agency
Advisory Council on Clean Air Compliance Analysis
Augmented for Review of Black Carbon
(Black Carbon Review Panel)

Individual Pre-Meeting Comments

Preliminary evaluation of EPA's draft document, *Report to Congress on Black Carbon* (external review draft, March 18, 2011)

Developed in Preparation for the April 18-19, 2011, meeting of the Black Carbon Review Panel.

Table of Contents

Alberto Ayala: Preliminary Comments

General Questions

Q: Does the draft Report accurately interpret and clearly communicate findings based on current lit including important BC uncertainties? What are the Panel's views on the preliminary conclusions as summarized in the Executive Summary and in the key messages of each chapter?

A: First of all, the US EPA is commended for tackling a critically important subject in such a comprehensive manner. It is this reviewer's opinion that the report is an excellent (notwithstanding the "eye tests"), fair wide-angle view of most of the relevant issues related to BC, and is almost error-free. Most of the relevant literature including late braking and contemporaneous work is captured in EPA's document. But there are some areas that are important and are not fully or equally addressed as others in this report. For instance, the EPA report is missing discussion on the influence of BC on world agriculture production. The recent United Nations report, UNEP/GC.26/INF/20, "Summary for decision makers of the integrated assessment of black carbon and tropospheric ozone", Feb. 2011 (http://www.unep.org/dewa/Portals/67/pdf/Black_Carbon.pdf) has clearly defined this problem. In the area of communication, it must be noted that this EPA report is not an easy read. It's a compendium of a massive amount of information. To improve communication of key observations, specially to the non-expert, EPA might consider following the example of the recent United Nations report cited above. It covers BC and other climate pollutants in a more readable and digestible way suitable for a broader audience. In terms of scope, this EPA document answers to the letter each and every one of the key elements of the congressional charge. But I believe it falls short on a couple of critical areas. First, it doesn't conclude for and communicate to the reader what the total weight of evidence suggests concerning the uncertainties associated with BC. The key messages presented at the beginning of each chapter are very useful. And the executive summary is a good summary of key messages of the key messages. But the most fundamental question is this: after everything is said, done, and discussed, does the total weight of evidence (including considering uncertainties and remaining knowledge gaps) suggest that the US should do more, less, or the same to control BC emissions? At the end, I believe this is what the reader is looking for.

Chapter specific questions

Chapter 2

Q: does report accurately identify and characterize light-absorbing carbonaceous particles, including BC and BrC? Does report explain/characterize difference between BC and long-lived GHGs? Does report characterize mechanism of how BC affects climate and full range of climate effects?

A: In general, this is a very good chapter that does a good job covering the basics of BC and how it impacts the climate. There are some areas that this reviewer would like to point out as potential opportunities for improvement. First and foremost, one central is issue has to do with models as tools to assess impacts; as tools to help us understand BC's effects. The report is silent as to how good these existing models are. For example, on pg 2-34, line 16. This is a reference to methods of assessment based on models. However, the report doesn't say where

these tools are sufficiently sophisticated to capture the complexities of the climate system, whether they are "good enough," or very robust and completely reliable and deserving of complete trust. The reader needs EPA's key message as to how useful the model results are. The report introduces, but doesn't define explicitly elemental carbon. Invariably, in practice, BC or EC or soot are operational definitions. So how does this affect (or not) our understanding of BC and its effects?

Pg 2-10, line 7: "this short atmospheric residence time curtails their total contribution to the Earth's energy balance,…." where does the energy go?

Same page, precipitation and deposition does not apply to the smallest particles, which can be BC, obey rules of motion different to larger particles and respond to diffusional processes. So what happens to them?

Similarly, the subject of aerosol transport is not discussed to the level of detail necessary to understand fully the implications of Arctic BC/PM effects and ice melt, as clearly most of the emissions sources are not from the Arctic region. This appears to be important since the same applies to other heavily snow covered regions.

Pg 2-11, line 32: "….BC is linked to a range of …health impacts…….This, too, distinguishes it from long-lived GHGs. Is this saying that long-lived GHGs have no health impact? Is there definitive agreement on this?

Pg 2-13, line 14. this is the first the concept of particle number is introduced, but it is not explained.

Table 2-2. First time the report introduces yellow carbon but it is not explained. Is yellow the same as brown carbon?

The last sentence of the caption for Figure 2-8 seems to be a suitable fist-part for a central conclusion of this report. That is, the net effect of BC, after consideration of all direct and indirect effects and the associated uncertainties for each area, is very likely a "warmer." A negative effect cannot be exclude, so….is this remaining uncertainty so large that because of it action on BC should not be taken beyond what is already done for other reasons (ie., health) or….is the knowledge of the likelihood of BC being a warmer is so large so that no matter the remaining uncertainty about the opposite effect, climate-specific action on BC should be taken now?

Pg 2-17, Figure 2-7, Incorporate uncertainty of each estimated forcing value.

Pg 2-18, Figure 2-8, data shows variation for the each type of forcing, but is not consistent with the average values presented. For example, plotting 0.34 + 0.25 W m^{-2} for direct forcing does not fall within the plotted data

Pg 2-18, Figure 2-8, middle table is not clear whether this is from BC or general observations of PM.

Pg 2-21, Figure 2-9, for comparison, clarify where the IPCC average value for this report falls, 0.34 W m^{-2}.

Pg 2-23, line 13, glaciation indirect effect is introduced but never really within the section

Pg 2-24, line 36, Hansen and Nazarenko (2004) in document, but data not presented in Figure 2-11.

Page 2-24, line 12 – "It is unclear to what extent BC contributes to the overall aerosol indirect effect." This statement and the rest of the discussion in the paragraph where this statement appears seem to be key, definitive statements. From them, the reader concludes that we should be to do nothing on BC for climate because we don't know enough. If this a correct inference from the statements, then the report should clearly say so in a very prominent place up front.

Chapter 3

Q: Does report accurately summarize and interpret body of evidence relating to health effects of BC? Or with regard to potential non-climate (welfare) effects of BC?
A: This is very difficult question because, as stated in bullet 3 of the summary of key messages, the great majority of the available literature on health effects relates to PM, not explicitly BC. And even the rare cases where BC is referenced, it is indirectly done through EC, which is not the same as BC. I struggled with the chapter title for the same reasons. In reality, we are talking about health effects associated with PM, of which BC is a fraction of. And even in the event of association to BC, in reality what we that means is really an association to EC. On the question of health effects and PM exposure, it is appropriate to refer the reader to EPA's recent Integrated Science Assessment for Particulate Matter as the most current and authoritative reference on the subject. Thus, reviewer agrees with authors about the key message. It is necessary to make it clear that we currently do not have enough information to fully access the health effect of BC relative to other constituents of PM2.5.

Pg 3-2, line 20, the comment about particle composition and particle size is important, but it seems out of place. The report does not contain previous, clear treatment of particle mass, particle size, or any other metrics associated with PM emissions. Thus, it is difficult for the reader to gain much out of this reference to the likely inadequacy of particle size as a predictor of health effects.

Pg 3-4, line 1 - what are the limitations or impacts of using EC to mean BC?

Pg 3-4, line 18 – caution should be exercised when using black smoke as a surrogate for BC and, by association, as an indicator of PM, specially in the case of diesel emissions. The vehicle emissions research literature has established well the limitations of black smoke as an indicator of PM.

Pg 3-6, line 25 – it is not good practice to refer the reader to a figure two whole chapters into the document.

Pg 3-3, lines 1-29 – this discussion needs clarification because it is not appropriate to assign the noted effects to BC if the studies actually investigated association to EC. This is a recurring theme that requires appropriate caveats.

Chapter 4

Q: Does report appropriately characterize available information on historical, current, and future emission of BC and related compounds in the US and globally? Does report present this information clearly? Does report summarize and interpret current information on transport of BC emission downwind of sources and relationship between location of emissions sources and geographic region of climate and non-climate impacts?
A: In general this chapter does a good job characterizing BC emissions inventories and trends. In fact, the presentation of current emission inventories for the US and the world are current and excellent. However, most readers will not be able to put into context most numbers presented. For instance the relevance of, say 0.65 million tons (580 Gg) of BC emissions is difficult to grasp unless the reader of provided a more familiar backdrop against which this value can be compared. Thus, consider providing some additional metrics. For instance, use for comparison something like the 200 million cars in the US emit X million tons of PM in a year. Also, the chapter needs some level of discussion and clarification concerning the fact that, as presented, these are all "primary" emissions since "secondary emissions" or precursor emissions are relevant to, among others, PM ambient burdens as discussed in later chapters.

Define contained combustion sources if the term is to be used in the summary of key messages for this chapter.

A comment made previously applies to this chapter on emission inventories and trends. What is the uncertainty of using EC for BC in some source profiles as presented?

Because the chapter includes discussion of top-down and bottom-up inventory approaches, consider corroborating the emissions presented with an alternative approach such as fuel use where the information is available.

Pg 4-4, line 6, are the lines average values or medians?

Same page, Figure 4-1. I disagree with the statement that only a single value is known for the individual profile of HDDV exhaust as illustrated in the plot of BC and OC. It may be that SPECIATE contains a single value for HDDV exhaust. If that is the case, then the shortcoming is associated with the database, not with the implied lack of information. If there is an exhaust profile that has been studied extensively, that is the diesel profile.

Pg 4-7, line 3, the title of Figure 4-3 uses EC instead of BC.

Pg 4-11, line 20 – consider making a distinction between "conventional" diesel as referenced here and clean diesel as discussed later. This is a critically important point because since 2007

most new diesel are "clean" diesel, meaning they reflect the use of advance design, including aftertreament such as the diesel particle filter. This filter eliminates soot or BC.

In order to avoid the eye tests, consider alternative presentation of data such as that in Table 4-3.

Pg 4-15, line 15 – California and other states are the exception to the statement that DPF controls do not apply to existing engines in the US. Please clarify. For instance, New Jersey (see Ch 7, pg 7-25) also has a mandatory retrofit program. Thus, please note other locales in the US do have in-use requirements. It's only EPA's program that is voluntary.

Pg 4-26, line 13, 4th IPCC assessment (IPCC, 2001) is not listed in bibliography. 2001, should be the 3rd instead of 4th?

Pg 4-35, line 20 – there is reference to "fine" particle regulations. Yet I do not think fine particles have been defined or correlated to PM2.5 or any other discussion of PM.

Pg 4-39, line 11. Again, there is reference to EC and BC interchangeably and no clear discussion of the magnitude that this assumption introduces.

Chapter 5

Q: Does report accurately characterized information on BC from the observational record?
A: This is a great chapter that lays out very nicely the extent of what we know about BC from the most current observational record. The results presented is very appropriate and complete. The inclusion of satellite observational data is right on target as this presents an emerging and promising field with potential, when uncertainties are resolved, to address many of the key data gaps associated with the current approach for monitoring ambient concentrations of BC and, for that matter, other pollutants. This chapter is also the beginning of discussion concerning the practical aspects associated with BC, principally its measurement and definition based on the operational method to determine it. The discussion is difficult because of the complexities, intricacies, and nuisances of the available methods. However, Figure A1-1 in the first appendix is a great graphical representation of methods and definition. I suggest you consider including this picture in the main body of the document to aid in the explanations concerning definitions and methods.

Pg 5-1, lines 7-9, this sentence should be its own bullet or the beginning of the bullet because it stresses the need for further research based on limited data.

Footnotes in Table 5-1 are missing

Check alignment of labels and values of row of "global background" information in Table 5-2.

Pg 5-11, line 10 – It is interesting to note that the reference to "large gradients" in BC concentrations as discussed and presented in Figure 5-3 really depends largely on the scale of this particular set of results. That is, to mind come the widely noted freeway studies and

investigations of near-roadway pollutant concentrations of the UCLA/USC group. In those investigations at scales of hundres of meters from the roadway, the large gradients are observed for ultrafine particle concentrations. In contrast, PM and BC concentrations in that micro-environment remain relatively flat.

Fig 5-4, what year is this information for?

Pg 5-12, footnote 10 – have you defined LAC?

Fig 5-5a – scales on axes are not legible

Pg 5-18, lines 15-18, does this imply that the reductions are occurring because of controls in Eurasia or a different reason? The reason should be made explicit since it is an obvious question by the reader of this sentence.

Pg 5-23, line 25 – why the use of bold font? Is it supposed to mean emphasis? Why? This approach is not used elsewhere.

Pg 5-28, Fig 5-17 – Not clear what the blue bars at top of figure represent relative to black ones or how to interpret

Pg 5-20, line 6, Units for wavelengths is not displayed (nm).

Pg 5-31, lines 4-5, there is no such Figure 5-22. The text refers to Fig 5-20.

Chapters 6 -10

Q: Does report accurately reflect and communicate information on available technologies, control strategies, and costs of BC emission reductions in various sectors? Are there additional control technologies or mitigation strategies that should be included? Can Panel suggest other sources of information on costs of BC emission reductions, particularly for international sources? Does report characterize range and magnitude of potential climate and public health benefits from BC emission reductions?

A: The chapters on mitigation options are of particular importance and are the very core of the congressional charge for this work. The diversity of sources and the mitigation options pertaining to each specific source makes it a challenging endeavor to try to present them in a logical and useful way for the lay reader. The analysis shows that there is a range of cost effectiveness from sector to sector. These differences should be highlighted and exploited as a means of potential prioritization of approaches across sectors. But congratulations to the EPA authors because you have largely accomplished this charge and you have almost done it a manner free from errors.

Fig 6-1, top chart, the labels of the various sectors are not legible

Pg 6-6, the figure should be Figure 6-2.

Pg 6-7, line 13, BC emissions decreasing to half of the 2010 levels by the end of the century is not supported by Figure 6-3. Should show 2100 instead of just 2050.

Pg 6-7, the webpage link in the footnote doesn't work.

Pg 6-9, lines 25 and 26. This paragraph makes reference to on- and off-road land transport and diesels. It is not clear if and how the former is inclusive of the latter. In other words, diesel can certainly be part of land transport both in the off-road and on-road category. Conversely, land transport, both on-road and off-road can include depending on the category (ie, engine size) other fuels (ie, gasoline) nearly exclusively. Thus, clarification is needed.

Pg 6-10, words in lines 27-30 were restated in line 31-34 on the same Pg.

Pg 6-14, line 24 – I believe the reference to Fig 6-3 should be to Fig 6-6

Pg 6-16, line 12, is CRF defined?

Pg 6-21, line 13, unable to verify the passive DPF cost of $5,000 to $15,000 in Chapter 7. Chapter summary on Pg 7-17 (lines 17-18) list retrofitting costs for passive DPF as $8,000 to $15,000.

Pg 6-21, line 34, Unable to pinpoint documented text and a clear reference for this cost. It appears in Chapter summary on Pg 9-1(line 19) and then again only in Figure 9-3.

Pg 7-1, the chapter needs to address the impact of DPFs on CO2 or efficiency and define passive and active DPFs, catalyzed and uncatalyzed devices.

Figure 7-1, the first three plots. The legend for on-road is missions. I believe it may be a simple problem of formatting the figure legend because comparing the plots to Table 7-1, the columns appear of the right magnitude.

Figure 7-1, last plot. The BC inventory for 1990 is shown as 487,345. However, Table 7-1 shows 487,627. It should be the same number

Pg 7-4, line 11, suggest mentioning Direct Injection and what it may do to BC emissions

Pg 7-5, Table 7-1, Title (BC) vs. first line source category (elemental Carbon) .

Pg 7-8, Footnote #6. The is reference to ultrafine particles and these have not been defined or discussed. Also, in the same reference, the state requires clarification. DPFs reduce number of solid particles. If total particles are considered, that is solid and non-solid components, the statement is not applicable for all operating conditions.

Pg 7-8, line 19, remove "/" between "brake" and "horsepower".

Pg 7-9, line 20. The reference to the substantial growth expected in nonroad emissions from 2005 to 2030 begs the question, from recent experience, as to whether this include economic forecasting and consideration of possible economic recessions. Whether the statement does or does not is an important piece of information for the reader.

Pg 7-10, Table 7-2, Define NPV.

Pg 7-12, line 1-7. The references to the various options being considered by CARB should make reference to a possible 0.001 g/mile limit also discussed. This reference is important for context with other emissions levels discussed in the chapter.

Pg 7-12, line 7. Consider starting a new paragraph with the discussion on nonroad gasoline engines

Pg 7-15, Footnote #11 contains several errors. First, the PMP methodology does not include a thermal denuder. The methodology simple calls for us of a volatile particle remover via thermal treatment of the exhaust samples. A thermal denuder connotes conventionally usage of carbon scrubbing and this is not the case for PMP. Second, the footnotes makes reference to PM without stating whether it is particle mass or particle number. The distinction is subtle, but deterministically important. Yes, PMP excludes organics, but these organics appear roughly in the sub-50 nanometer particle size range so they do not contribute much to PM mass. PMP was explicitly designed to ascertain differences between various kinds of DPFs in the solid particle number emission range where the standard PM mass measurement was unable to distinguish.

Pg 7-17, line 17-18, The range of costs for passive DFP is inconsistent with range previously mentioned on 6-21 and 7-2.

Pg 7-18, Section 7.6.1.2. The title “other retrofit strategies” is not suitable or representative of the strategies discussed in the sections. For example, maintenance practices are not retrofits.

Pg 7-20, line 5. Reference in brackets is missing

Pg 7-27, lines 2-3. Partial flow filters are not viable options for BC reductions. On Pg 7-17, report states that there is “no consensus and limited test data on the effectiveness of PDPF to reduce BC.” Please correct statement.

It is important to point out that the overarching PM2.5 criteria pollutant control program for stationary sources in the United States and Europe has focused mainly on secondarily formed particles such as sulfates and nitrates, rather than direct PM2.5 emissions.

Pg 10-1, line 4 and Pg 10-2 Line 14, the numbers do not match each other. 340 million vs. 350 million.

Pg 10-2, lines 24-27, a reference is needed

Pg 10-8, lines 25-27, the content is confusing. Please verify if burning fuel when moisture content is high reduces fuel consumed. A general idea is high moisture content leads to inefficient burning and high CO/CH4 emissions.

Chapter 11

Q: Does report accurately describe the range and limitations of metrics available to quantify and/or communicate the climate effects of BC, to compare BC with long-lived GHGs, and to compare BC mitigation alternatives?
A: The chapter has nice and adequate description of metrics and how and when they apply. Commonly-used metrics for GHGs and four main metrics for comparing BC to other climate forces have been introduced individually. The purpose of developing metrics for GHGs and shortcoming involved in using each metric for comparisons among the long-lived and short-lived gases has been addressed. Considerations when using metrics in the context of climate policy decisions has been listed. However, similar to the overall lack of a definitive statement on whether BC should be controlled for climate, this chapter does not coalesce the various metric options into the most appropriate, single option. The global goal consensus is clear, we should work to prevent an increase in the global average temperature beyond 2^{o}C above pre-industrial levels. To do this significant emission stabilization and reductions are needed. So, with this in mind, what is the most suitable metric to treat BC in the context of long-lived GHGs so as to extract its climate benefit?

Pg 11-2, line 18, (IPCC, 2009). Close parenthesis and clarify which document (2009a or 2009b) as listed in bibliography.

Pg 11-4, line 17, Clarify which document (2009a or 2009b) as listed in bibliography.

Pg 11-5, line 22, Same comment above.

Pg 11-6, lines 5, 20, 31, and 33, Same comment above.

Pg 11-14, lines 18-19. This is a reference to differences between climate models. Thus, this points again to the previous comment concerning this report lacking an assessment of the "goodness" of the models themselves.

Ch 12

Q: Does report appropriately identify highest priority research needs for BC?
A: The priorities for future research reflected in the report are appropriate. But at some level, the expectation for the work undertake for this report was that it would yield a more definitive statement, sifting through the uncertainties and remaining unknowns, in order to address the most basic and fundamental questions related to BC control. And, alternatively, since it is the case that the report does not reach a set of "to-the-point" recommendations, it should then clearly state that the unknowns are too great so we are not ready to act on BC.

Appendices

Appendix 1-14, line 1, filter temperature per 40 CFR 1065 is 47 degrees C +/- 5 degrees C. 125 degrees F is the upper limit for filter temperature.

Additional suggestions and edits:
Use consistent unit format throughout document and in tables, figures and graphs. (Observed the use of Wm^{-2}, W m^{-2}, and W/m2).
Use consistent figure and table formatting. Many times axis are not labeled.
Add list of acronyms

Pg Ex-2, Figure A displays units of Gg without previous definition.

Pg Ex-3, 3rd to last line, suggest to add "completely" in between "yet" and "clear".

Pg Ex-4, line 3, "OC" is never defined.

Pg 1-4, line 6, replace "national" with "international"

Pg 1-4, line 29, Define Gothenburg Protocol?

Pg 2-40, line 25 add "to" after "transported"

Chapter 2. Text boxes are not numbered

What scale of picture on left of Figure 2-2?

Fix legend in Figure 2-4. Brown line should read as "Absorption by BrC". And the y axis has no units/scale.

Pg 2-24, line 11, Remove comma at end of line.

Pg 3-7, line 14 – typographical error: delete "of" before "US air quality regulation…."

Pg 4-2, line 17, inconsistent use of EPA and U.S. EPA. Consider global check.

Pg 4-2, lines 22 and 25, inconsistent use of PM2.5. Consider global check.

Pg 4-14, line 3, font size change.

Pg 4-14, line 14, end of sentence has errors.

Pg 4-15, line 14, "result (in) future BC reductions" – missing word.

Pg 12-6, line 17, Define RF.

Appendix 2-2, line 23, and Appendix 2-4 line 2, inconsistent use of definitions of ER.

Appendix 2-2, line 21, and Appendix 2-4 line 13, are these same variables of the equation? If so, they are inconsistent.

Appendix 2-3, line 25, consider specifying chapter of earlier discussion of filterable fraction and condensable fraction.

Appendix 2-4, line 26, sentence explains three examples of the category, not one.

Appendix 2-5, line 7, unclear as to what example of estimation is discussed.

Appendix 2-7, line 31, it is stated that exhaust VOC is markedly different. How?

Appendix 2-9, line 24, why is the middle of the range used? Which study presents vessels that are more representative of the population? Wouldn't a population weighted average be more appropriate?

Appendix 2-11, lines 18-19, sentence is unclear.

Appendix 2-13, lines 31-32, there is no such table as Table 2.X.

Appendix 2-16, line 3, Table reference is inconsistent and incorrect (A2-4?)

Appendix 2-18, line 2, Table reference incorrect.

Michelle Bell: Preliminary Comments

Overall, I found the draft document to be well-written and clear. There are a few places where we might discuss whether the document, although accurate, provides the best emphasis on various points. Below are some examples and other comments.

I had difficulty getting a sense of the scientific uncertainty for the indirect effects and overall impact of BC on cooling versus warming. There are some statements, such as that the warming effects "very likely" exceed the cooling effects (e.g., pages Ex-3 and Ex-4) but elsewhere the net effect is "very uncertain" and "thought to be a net cooling influence" (Introduction page 2-2) although warming is "very likely" to exceed cooling (also page 2-2). The overall sense of uncertainty seems to be a bit inconsistent throughout the document. This text needs to be revisited so there is a consistent picture of the degree of uncertainty.

Although the text notes that BC is a component of PM, this should be stated up front. For much of the early part of the document, readers may think of BC as a single pollutant. This issue is described (e.g., on page 2-12), but some brief text on this point could come earlier.

Pages 2-13 to 2-15 discuss how different co-pollutants of BC (by which this report really means other parts of the PM mixture) affect the particle properties, but most of the report refers to BC as a single pollutant with distinct properties. Is this because the influence of other PM components is minor or because the main report provides overall properties? In health studies "co-pollutants" is a term used to describe other pollutants, not other parts of the PM mixture. Although a minor point, it may be worth revisiting that language.

The lack of scientific evidence enabling us to distinguish BC PM2.5 effects from those of other types of particles is not specific to BC (e.g., page 3-1). Rather we don't have the scientific basis to distinguish effects from different types of PM2.5 particles, including BC or other components. The current wording is correct, but may give the impression that we have more uncertainty about BC than other PM components.

Many of the strategies that would reduce BC emissions, such as changes in transportation patterns, could also reduce traditional GHG. The concept of co-benefits seems missing from much of the text, such as chapter 6.

The study of BC/EC sources includes source apportionment approaches, as mentioned (page 3-4), but also other methods such as an indicator component approach, traffic modeling, etc. These need to be mentioned.

The uncertainty of source apportionment methods may be a bit overstated (page 3-4). This text should be expanded to give a better sense of the uncertainties, and consistencies, of this approach.

The comment that other pollutants are often co-emitted with BC leaves off ozone, which should be added (see page 3-5 and chapter 6).

The sentence "there is far greater certainty" that reducing BC provides health benefits, compared to the certainty of climate benefits, is understated (page 6-13). The evidence for the health benefits is overwhelming based on numerous studies over decades of research.

Given the large benefits to costs ratio, the text “EPA’s analyses have consistently suggested that the benefits outweigh the costs” (of PM) could be modified to include some specifics from the Benefits and Costs of the CAA documents (page 6-17). For readers who are not familiar with these EPA analyses, presentation of some of the results may be informative.

The section on various epidemiological studies that have examined BC/EC (chapter 3) could be updated. However, I’m not sure it’s imperative to have a complete review.

The point about spatial variation of BC is important and is made in the executive summary (see page 2), but Figure A giving U.S. versus global emissions might imply less variation within the U.S. This section could mention that there is regional variation of BC within the U.S.

Appendix A on ambient and emission measurement of BC is very useful and could be highlighted more in the text.

Kevin Boyle: Preliminary Comments

- Report well written grammatically, but confusing because of continual switching from:
  - U.S. focus to international focus,
  - BC to GHG, and
  - BC to $PM_{2.5}$.
- Convert all dollars to a common year, 2010.
- References to "cost effectiveness" in report appear to treat this as an absolute rather than a relative concept. Cost effective relative to what other technology (e.g., p. Ex-5, 3rd and 4th bullets, and p. 6-1, 2nd and 3rd bullets). This occurs in the cost chapters, for example, on p. 7.22, lines 4-5, but appears to be more correctly frame in lines 8-9. It appears what is referred to as cost effectiveness is simply cost estimates.
- Report clearly needs to state that BC and $PM_{2.5}$ are joint products in control technologies. Thus, health benefits and control costs are for $PM_{2.5}$. To continually refer to BC overstates both the benefits and cost of reducing BC emissions.
- As a consequence, there may be double counting of benefits across different policies designed to reduce emissions.

Charge Questions

1. With respect to benefits and costs the report does not present sufficient information to asses if the use of existing benefit estimates and cost calculations are accurately interpreted form the existing studies. However, it is very likely that the health benefits of reductions in $PM_{2.5}$, and thereby BC, exceed the control costs.
2. The use of existing benefit and cost data should follow the developing benefit transfer literature to insure these data transfers are credible.

11. The report does not provide any information about what elements of costs are included in the cost numbers reported.
12. No.
13. More could be done to explain ranges when reported (i.e., the basis for the ranges) and to explain how uncertainties that are not quantified in benefits and costs might affect the numbers reported.
15. I have noted some additions under this chapter below.

Benefits – Chapter 6

- This approach to benefit estimation is a benefit transfer. A clear statement of the benefit to be measured, an explanation of the existing benefit estimates used (VSL, compromised health day, health costs, etc.?), method(s) used to compute benefit-transfer estimates, and any adjustment done to calibrate transfer estimates to current application is needed. Readers should be able to reconstruct what was done to develop the transfer estimates.

- What percentage of the BC ($PM_{2.5}$) benefits are due to current, in place, policies and what percentage is due to optimism of new policies that have not been enacted?

- BC as a share of $PM_{2.5}$ varies by emission source. Does that influence the health consequences and consequently the benefit estimate per ton of emission reductions?

- Report discusses spatial aspects of global warming benefits of BC reductions, but does not acknowledge and spatial aspects of the health benefits from $PM_{2.5}$ reductions.

- P. 6-17, key citations appear to be wrong or typos *vis a vis* Bibliography. No Smith et al. (2008) cite in Bibliography; should it be Smith and Haigler (2008). No Baron et al. (2009) cite in Bibliography; should it be Baron et al. (2010).

- The benefit per unit of $PM_{2.5}$ reduction assumes a linear relationship. Do health benefits decline as additional reductions are accomplished? If the benefit function is nonlinear, the use of average benefits likely result on under or overestimates of total benefits.

- What year dollars are used to report the information in Table 6-2? Can these numbers be reported in per ton of $PM_{2.5}$ reduced to allow contextual comparisons with other benefit estimates reported? What are the sources of these numbers?

- Table 6-3 is for $PM_{2.5}$, but the only Pope et al. paper in the Bibliography addresses low birth weight and stillbirth from indoor air pollution. Is this the correct citation?

- No Laden et al. cite in the Bibliography.

Costs

- There is a need to define what costs should be included for the different remediation technologies and then identify the elements of these costs that are included in the cost estimates reported.

- Some of the element form individual chapters apply to all chapters here.

Chapter 7

- Cost estimates do not include what is included: installation (new and retrofit), fuel costs, operation and maintenance, and regulatory.

- No discussion of the need for maintenance to preserve effectiveness and associated costs. Only one reference on p. 7-17, line 10.

- What about disposal of filters that may contain hazardous materials?

- P. 7-10, Table 7-2, what year's dollars are the costs reported? The numbers in parentheses?

- P. 7-11, line 1 acknowledges that emission models have changed since the cost estimates were developed, but the cost estimates have not been updated. There have been large changes in the economy, which may keep existing vehicles in service longer. Increasing fuel prices may cause a substitution/shift to intermodal freight facilities. It would be good to at least acknowledge these effects and give some insights on how these moderators/confounds would affect costs and projected reductions.

- What percentage of the BC ($PM_{2.5}$) reductions are due to current, in place, policies and what percentage is due to optimism of new policies that have not been enacted?

- Cost per unit is assumed to be constant. Have the easiest reductions been accomplished and per unit costs will increase to attain marginal units need the goal reduction? Will there be increasing regulation costs to meet the goal, e.g., new vehicle inspections and required retrofitting.

- I would like to see a summary table that presents cost and expected emission reductions for each technology/policy.

Chapter 8

- Nothing in this chapter confirms that that there will be substantial domestic reduction in the 8% share. Likewise, for the international 14%.

- Table 8-1. What is the year for the dollars? Where does the boiler size fit in the range of boiler sizes? What is the basis for the range of costs?

- International changes will be difficult with many dispersed, small operations.

Chapter 9

- Nothing in this chapter confirms that that there will be substantial domestic or international reductions. Reach and effectiveness of voluntary programs not established.

- What will be the effect of increasing costs of fossil fuels?

- No adjustment substantial consideration for improper installation and operation. Actions home owners likely to do themselves. Will homeowners be careful with operation?

- Figures 9-2 and 9-3. What year are the dollars?

- Section 9.4.2. What are the costs of traditional technologies? What is the share of household income in developing countries?

Chapter 10

- What is the share of anthropogenic *vis a vis* wildfires?

- Any potential policies to reduce anthropogenic?

Conclusions – Chapter 12

- P. 12-2, fourth point, lines 18-28. Should say public health benefits come from $PM_{2.5}$, which includes BC.

- P. 12-3, sixth point, lines 3-17. Sequencing of actions may influence marginal benefits, which not possible with current benefit estimates.

- P. 12-5, fifth point, lines 20-29. Benefit estimates need to be policy specific and recognize potential sequencing in policy implementation.

- Cost estimates need to be verified, undated and include all relevant costs.

Sylvia Brandt: Preliminary Comments

My main concern about the content of the current draft is the lack of information about the economics surrounding black carbon. Specifically I'd like to see a clear summary of the costs of various mitigation approaches as well as a summary of the state of the valuation literature for the ecosystem, agricultural, climate and health effects. Without a sense of the relative benefits and costs, it seems difficult to draw policy conclusions. To the degree that these costs and benefits are unknown they should be discussed as uncertainties just as the uncertainties in the physical science are addressed.

My main concern about the presentation of the current draft is how domestic and international issues are integrated. I understand why the report is organized by topic, but to be informative to policy it is necessary to think in terms of regions. For example, I was left wondering about the relative cost-effectiveness of reducing domestic diesel contributions to BC versus subsidizing conversion to cleaner cook stoves in regions such as Africa and Latin America. Given the current structure of the report it is impossible to get a sense of how these and other mitigation approaches compare in terms of cost and benefits, or to compare them in terms of technical or political feasibility.

I would like to see a summary which starts with a ranking of the main producers of BC domestically, the relevant mitigation strategies for those sources, then scale of benefits that the technically feasible reductions could deliver taking into account the degree to which that source contributes to the distribution of BC to sensitive areas (e.g. artic). When possible the costs of these mitigation strategies and associated nonmarket valuations should be included. Obviously there are substantial uncertainties and many unknowns, but a summary could give the reader a sense of the relative scale of current impacts and potential change in impacts through mitigation. Then a similar summary could be done for international sources. I would keep these summaries separate because the strategies for reductions are obviously very different.

The figure 4-33 is very informative about the source of BC that is relevant to sensitive areas, but I think the discussion on page 4-26 should be given more attention. The fact that where the BC is emitted is a critical element of its impact on climate change means that there are different marginal benefits over regions whereas the marginal costs might be comparable. Thus, to achieve a target reduction in the contribution of BC to climate change we would want a policy that allows for different levels of reduction by regions.

Q1a. Does the draft report accurately interpret and clearly communicate the findings of the current scientific and technical literature, including important uncertainties, pertaining to black carbon?

1. Communicating findings

I found the use of text boxes such as 2-1 on page 2-9 and tables such as 2-1 or 2-1 very helpful in summarizing the information. The description of uncertainty on page 2-19, lines 10-19, is very illustrative. I'd encourage more of these types of text boxes and figures, particularly in other parts of the report where uncertainties are described within the text.

I'd encourage using similar text boxes for other critical information such as other names for black carbon (e.g. page 2-6), listing the proxy measures for BC (e.g. $PM_{2.5}$), the other pollutants emitted with BC (e.g. page 2-12 lines 21-23) or description of brown carbon (page 2-7, lines 12-13).

2. Current literature

Chapter 2 --- The discussion of economic value (section 2.7, pages 2-46 to 2-47) is incomplete. There is significant literature on the uses and limitations of estimates of the social cost of carbon.

A review of this literature is warranted if change in climatic effects is an endpoint of interest. A starting point is the work of Frank Ackerman and Gary Yohe.
Chapter 3 --- See comments below.
Chapter 5 --- There needs to be an introduction that makes it crystal clear how Chapter 5 adds to what was presented in Chapter 4. For a novice, this chapter seems redundant at first glance.
Chapter 6 --- The separation between chapter 3 and sections 6.5-6.6 seems artificial. In both places the impacts of health are discussed. One way to differentiate the mitigation discussion would be to include the associated costs and benefits of those various reduction levels.
Chapter 6 --- Two papers cited in chapter 6 (page 6-17) are worthy of more discussion (US EAP 2006 and US EPA 2004).
Chapter 6 --- Table 6-3 is not particularly informative without the estimates of the costs of these reductions. Similarly, Table 6-2 has current regulations, but it would be good to see potential alternative abatement approaches. IE what we have right now might not be the most cost-effective way to get to reductions.
Chapter 11 --- I think it is inaccurate to state that valuation of climate changes are discussed in detail in chapter 2.7 (page 11-11, lines 32-33).
General ---Two papers regarding the contribution of BC to PM should be reviewed:
Peltier, RE and Lippmann Spatial and seasonal distribution of chemical components of ambient air fine particulate matter (PM2.5) in New York City from incineration, coal combustion, and biomass burning. *Journal of Exposure Science and Environmental Epidemiology* (accepted).

Peltier, RE Cromar, KJ, Hu, J., Fan, ZH, and Lippmann, M. Spatial and seasonal distribution of aerosol chemical components in New York City: *Road dust and other tracers of traffic-generated air pollution Journal of Exposure Science and Environmental Epidemiology* (accepted).

Q1b.Based on this literature, what are the Panel's views on the preliminary conclusions as summarized in the Executive Summary and in the key messages for each chapter?
Executive summary --- Ultimately, a decision maker needs to answer the following question. What combination of reductions by source would be the cost-effective way to get to a target reduction? With the current state of knowledge we can't fully answer this question, but we can identify what we *do know*, what we *do not know*, and what *we need to know* to start to answer this question. I'd think that an executive summary should provide that overview.
Executive summary --- the proportion of US BC emissions that reach sensitive areas needs to be stated upfront.
Chapter 6 --- The second key message (page 6-1, lines 16-30) is much too broad. Are these cost-effective reductions in the US or internationally? For which BC sources is this true?
Chapter 6 --- The third key message (page 6-2, lines 1-3) is not supported by the discussion in the chapter. There is no critical review of the literature used to base the benefits of reducing PM.
Chapter 9 --- I really wonder if the issue of emissions from cook stoves should be in this report. First, the BC/OC ratio is low, as stated many times in the report (e.g. page 9-4, lines 21-25. Thus is it really the relevant outcome? Second, it is a significant emission only in other countries. Thus, I question the degree to which US policy can affect it. Third, the issue of adoption of new and efficient cook stoves is a very complicated economic, anthropological and social issue. Previous attempts to reduce exposure in households by providing efficient stoves have found insignificant benefits because provision does not imply use.

Chapter 12 --- The need for more research on the costs of the mitigation approaches and the economic benefits of reduced BC (reduction of direct and indirect costs of negative impacts plus nonmarket valuation) need to be added to the list of high priority areas.

Somewhere there needs to be a summary of the scale of reductions in BC that are technically feasible over the range of sources. Then those technically feasible reductions need to be described relative to their impact in areas important to climate change or the other impacts (e.g. visibility). As the report now stands there are various levels of reductions mentioned casually without any context describing if those reductions are technically feasible or cost prohibitive. An example is on page 6-15, line 1-2, where a halving of emissions is used to characterize health benefits. Yet, the reader has no information to judge if that is a reasonable goal. If it is a reasonable goal what combination of abatement of sources would get us there?

Q7. Does the draft report accurately summarize and interpret the body of scientific evidence with regard to potential for non-climate environmental (welfare) effects of BC? (Chapter 3)

There needs to be clarification of the distinction between environmental effects and the welfare changes that these effects imply. Then there needs to be a discussion of how welfare changes could be measured: typically we measure welfare changes with nonmarket valuation.

The report cites literature finding effects on ecosystems (page 3-5, lines 16-21), crop productivity (page 3-5, lines 22-27), building materials (page 3-5, lines 28-38), and visibility (page 3-6, lines 5-34). There is no assessment of the robustness of the findings of these studies, the degree of uncertainty or the range of magnitude of impacts. For example, what is the practical significance of the measured changes in Chameides et al (2009) and Auffhammer et al. (2006)? Do these findings suggest a crop loss on the order of 1%, 10%, greater? Is it reasonable to extrapolate from these studies to important crops in the US? Similarly do the ecological studies cited imply changes in ecosystems that are perceptible?

More information on the scale of the effects is critical to the next step in estimating what the benefits would be of mitigation. If we can't measure the change in these effects and the degree to which these changes are perceptible, then we cannot move forward to nonmarket valuation.

Visibility effects receive more attention than the other impacts (subsection 3.4.1), but it is still difficult for this reader to get a sense of the scale of the problem that BC causes with respect to visibility.

A discussion of the benefits for which nonmarket estimates are available is missing. There is an extensive literature on valuation, and a good starting point would be the recent EPA report on the costs and benefits of the Clean Air Act. Similarly, I'd like to see more attention given to reviewing and evaluating estimates on the cost mitigation. There is one study of the cost of black carbon that should be mentioned, even though it is admittedly incomplete (Sarofim et al., 2010).

It would be most helpful if at the end of 3.4 there could be a summary box that characterizes the scale of these impacts. Can the changes in these effects for different levels of BC abatement be presented (along the lines of is done for health in section 6.6?

Section 3.5 (and section 2.7 as well) is insufficient. First, only mortality and visibility are mentioned. All the impacts reviewed in this chapter (and all climate impacts) have associated benefits and nonmarket values. Second, a major impact that needs attention is human morbidity, primarily asthma, for which there are valuation studies available. Third, the descriptions of use value and non-use values (page 3-7, lines 29-32) are inaccurate. Fourth, if the report is going to raise the issue of applying VSL over different populations, it should investigate the extensive literature on equity-weighting for cost-benefit analysis. For a introduction, see

http://ageconsearch.umn.edu/bitstream/9325/1/wp070043.pdf. Fifth, the text states that visibility is one area for which there is valuation work, but no citations are provided.
A minor point is that the references for these papers cited in the text are missing: Levy (2009), Hubbell (2009), and Tagoris (2009).

Linda Bui: Preliminary Comments

General Charge Questions 1 and 2:

- The different roles of BC, OC, and BrC need to be more made more clear in the document as they have different effects on climate and health. It would be helpful to have stronger evidence (if it exists) on how varying the composition of these types of carbonaceous particles may affect health and the environment.

- The level of uncertainty in our understanding of the effects on climate and health are very high. These should be quantified throughout the document. In graphs, would it be possible to include confidence intervals (particularly for warming) (integrate chapter 2's uncertainty to the rest of the document)?

- More information/detail would be useful on the seasonal effects of BC. If wind patterns differ at different times of the year, does that imply for management purposes that it may be more/less advantageous to burn at one time of the year versus another? When are the "optimal" times?

Charge Questions for Specific Chapters:

Chapter 2:

- Over-all, chapter does a good job. Although many GHGs are different than BC, it might be useful to include some of them in the report, simply for comparative purposes wrt warming.

- Seasonality issues could be expanded, as well as the importance of the short life-span of BC's effects.

- Indirect effects section isn't very clear.

- Impact on the Arctic: other than saying that uncontrolled burning of biomass in Northern countries (can we please list these areas) as well as agriculture burning, where else are those emissions – particularly those that lead to deposition on the ice – come from? Does it change by season? What's the role of shipping?

- Similarly for the Himalayas: are emissions from some countries more important than others? If wind patterns change (due to changes in global temperatures/sea level), will that affect things?

Chapter 3:

- Because BC is always released with other pollutants, it's difficult to parse out the effects of BC alone on health. To the extent possible, it would be worth while to try to find some more studies that try to isolate those effects.

- Non-climate effects of BC (PM 2.5) – are not carried forward from this chapter elsewhere (particularly in the mitigation section) when discussing benefits.

- Not comfortable with the thought of using VSL (or any monetary metric) here – particularly as many of the benefits will accrue to people in the developing world. Would be worthwhile to expand this section (3.5).

Chapter 4:

- BC estimates from mobile sources seems completely reasonable, given the limitations involved.

- BC from biomass combustion: although the second largest source – not much can be done about wildfires (other than management). But, with the high OC/BC ratio, maybe less of a problem in the US EXCEPT for Alaska (which can affect the Arctic). All other sources seems small I the US and even smaller, globally. Should focus on mobile and Alaska.

- Are there large differences in estimates depending upon if you use a bottoms-up versus a top-down approach?

- How will the development of better infrastructure (which could lead to more mobile source pollution) change the composition of BC emission sources in the developing world? Will need the infrastructure if we are to think seriously about some of the global mitigation strategies. Do we need to consider this?

- It's not until Section 4.4.2 when we learn about which areas might be important contributors to Arctic BC problems. This needs to be referenced throughout the Report. Table 4.7 could be expanded to also include relative contributions of BC from all countries and sources north of the 40th parallel. Include in that shipping, if it's a contributor. (Similarly for the Himalayas (Section 4.5, pg. 4-32)).

- Long-range source-receptor discussion: do these relationships change across seasons? Affected by temperature? Sea level?

Chapter 5:

- NC.

Chapter 6-10: Mitigation Approaches to Reduce Black Carbon Emissions

13. Does the draft report appropriately characterize the range and magnitude of potential benefits for both climate and public health that could result from reductions in BC emissions?

- The potential benefits from mitigation approaches is confusing. In part, this may be driven by the uncertainties underlying the science, but it may also be due to conflicting "recommendations." For example, we are told that moving away from diesel fueled transport

methods (e.g. trucks) to rail and shipping would reduce BC emissions. Yet, we are also told (in an earlier section) that shipping – particularly in areas close to the Arctic could lead to BC deposition which could lead to warming. Which is the lesser of these two evils?

- Health benefits are based almost solely on studies for PM. We know a lot about those benefits (and their associated costs). The Report makes it clear that BC health effects cannot be studied in isolation and that the composition of BC in PM varies greatly, depending upon its source. In my mind, this greatly undermines the evidence. At the very least, it would be useful to cite any small studies that measure BC directly and investigate their health effects For example, Loh, et al (EHP 2002) make use of data from Roxbury, MA from the AirBeat program that measures BC hourly in this community and tries to investigate the health effects of fine particulate emissions and BC. Are there any other communities that have adopted similar programs (or have similar types of monitoring) that could be used to study the relative effect of BC on health outcomes? (Can we exploit deregulation of the trucking industry?)

- Not all of the possible mitigation strategies appear to be realistic. For example, the ability to provide more efficient cookstoves to potentially millions of people in certain parts of Africa, India, and Asia does not strike me as being feasible – at least not without an infrastructure that can support such a program.

- WRT health effects in developing countries: how will better access to better health care affect the morbidity/mortality numbers? Is this something we need to consider in the benefits section?

- Because some of the benefits are global in nature, but the pollutants are more local/regional, it would be helpful to have a clearer understanding of the geographic nature of the "important" polluters with respect to climate change. In particular, which countries are primarily responsible for the deposition in the Arctic? Does it differ by season? How would it change if shipping were to increase in these areas?

- Developed countries in general, and the US in particular, have already put into place regulation that has, and will continue to reduce PM emissions (and, as a result, BC emissions). In the big picture, the reduction in emissions by the US will have only a small effect on the global effects of BC (unless the US has a large impact on BC deposition on ice and snow in the Arctic or elsewhere) and the benefits to the US from future reductions will be small (relatively speaking). Most of the benefits will accrue to developing countries (from the local/regional effects. How do we want to put this into perspective? (NOT VSL.)

- A more developed discussion on uncertainties I think is important. In particular, an expanded section on mitigation and OC versus BC (and BrC) as some has cooling potential, some has warming potential, and some has something in between (incorporate more of Chapter 2's uncertainties into Chapters 6-10?).

- Do the effects (positive/negative) of atmospheric brown clouds warrant more discussion? Virtually nothing is discussed regarding changes in rainfall, reduced UV at the surface, etc. in these later chapters.

- Perhaps we want to use a measure of "potential years of life lost" or "avoidable mortality" instead of mortality (or other measures) because of difference in health care for developing countries? ("Avoidable mortality—a tool for policy evaluation in developing countries?" G. Stevens, Eur J Public Health (2010) 20 (3): 241-242. doi: 10.1093/eurpub/ckq051?)

- Sources and supplies of very low sulfur fuels? By geographic area/country?

- Relative size of BC versus OC: will make a difference WRT mitigation technologies for stationary sources? (Will it affect outcomes?)

Chapter 11:

- NC.

Chapter 12:

- C-B analysis would be very difficult, but would probably shed light on policy feasibility – both for the US and for a global agreement. Particularly given that the costs to the US (as well as the regional benefits) would be small (relatively speaking), but the global implications could be quite large. Costs and benefits would be much larger for developing countries.

- Very little discussion on the TIMING of emissions. Since this may matter – might want to know more about it.

Appendices:

- Okay.

James Corbett: Preliminary Comments

This report is a very well prepared study and overview of all the major issues related to Black Carbon, including characterization and representation of scientific and technology information important to U.S. and international concerns.

General Questions for All Chapters

1. In the Panel's view, does the draft Report accurately interpret and clearly communicate the findings of the current scientific and technical literature, including important uncertainties, pertaining to black carbon (BC)? Based on this literature, what are the Panel's views on the preliminary conclusions as summarized in the Executive Summary and in the key messages for each chapter?
    a. The term “contained combustion” appears to be a new term for this report. This is ill-defined. Biomass burning may merit its own category, but combustion types include: a) open burning (uncontrolled combustion processes, sometimes “contained” within a geographic space, nominally inclusive of biomass burning); b) open combustion (inclusive of steam boilers, some gas turbines, etc., where combustion occurs in technologic devices under near-atmospheric pressures but under controlled fuel-flame conditions) c) closed combustion (inclusive of internal combustion, reciprocating engines such as diesels). This term is used in at least two places, defined vaguely in Section 8.3 for the first time. It should be defined at first use – if used – and perhaps an alternate term is better, per this comment. (aside from below, see use in Section 11.6.1, Page 11-16, Line 21)
    b. Domestic:International or non-domestic, and Global may be okay. But there are U.S. sources that are not "domestic" in location, and (more importantly) there are international sources that occur within the "domestic" domain, such as shipping. Merits clarification.
    c. Given the range of non-road diesels, the report should be more inclusive of control technology discussions, especially with regard to particulate filters. Many persons and most literature in the past fifteen years or so focus on catalytic filters – in others words, filters that can operate effectively across load changes that reduce exhaust temps below a certain temperature. These perform well only in the presence of low-sulfur conditions (thus low sulfur diesel fuels).
        i. Some non-road equipment using fuel combustible in diesel engines can burn less costly higher sulfur fuels. These would poison the catalytic particulate filters, as the Corbett et al 2010 paper in Carbon Management discusses and cites (reference below). Therefore, we went to the earlier literature on non-catalytic filters where the requirement for high temperatures must be maintained; these requirements match conditions for some non-road operations (e.g., marine engines), and may be more cost-effective than requiring low-sulfur fuels for all non-road conditions.
        ii. This is “counter” to conventional wisdom and perhaps counter to corporate memory within parts of EPA, so it bears noting for your masters. Given that EPA may be moving toward lower fuel sulfur standards for marine engines, the report will need to note that 1000 ppm (the coming ECA standard) is still too much sulfur for catalytic filters. This will apply

to possible mitigation technology for ships in the Great Lakes, Alaska, and other coastal areas unless marine fuel standards are further regulated to reduce sulfur such that catalytic technologies become feasible.

iii. Corbett, J.; Winebrake, J.; Green, E., An assessment of technologies for reducing regional short-lived climate forcers emitted by ships with implications for Arctic shipping. Carbon Management 2010, 1, (2), 207-225.

d. There are many "tables" that are really pasted pictures from Excel. These need to be replaced and formatted as tables. The data are not able to be searched as pictures.

2. Is the Panel aware of any additional, policy-relevant studies that should be included in the draft Report to inform the preliminary conclusions? Are there specific studies that should be given more or less emphasis?
   a. Yes, see below.
      i. Corbett, J.; Winebrake, J.; Green, E., An assessment of technologies for reducing regional short-lived climate forcers emitted by ships with implications for Arctic shipping. Carbon Management 2010, 1, (2), 207-225.
      ii. Arctic Council, Tromsø Declaration. In Arctic Council: Tromsø, Norway, 2009.
      iii. Arctic Council, Arctic Marine Shipping Assessment 2009 Report. Arctic Council: Tromsa, Norway, 2009; p 194.
      iv. Corbett, J. J.; Lack, D. A.; Winebrake, J. J.; Harder, S.; Silberman, J. A.; Gold, M., Arctic shipping emissions inventories and future scenarios. Atmos. Chem. Phys. 2010, 10, (19), 9689-9704.
      v. Paxian, A.; Eyring, V.; Beer, W.; Sausen, R.; Wright, C., Present-Day and Future Global Bottom-Up Ship Emission Inventories Including Polar Routes. Environmental Science & Technology 2010.
      vi. Norway; Sweden; United States, MEPC 60/4/24 Reduction of emissions of black carbon from shipping in the Arctic. In Organization, I. M., Ed. International Maritime Organization: London, UK, 2010; Vol. MEPC 60/4/24. AND any later submittals by the United States.

Chapter 2: Black Carbon Effects on Climate

3. Does the draft Report accurately identify and characterize light-absorbing carbonaceous particles, including BC and brown carbon?
4. Does the draft Report adequately explain and appropriately characterize the differences between BC and long-lived greenhouse gases such as CO2?
5. Does the draft Report appropriately characterize the mechanisms by which BC affects climate and the full range of climate effects of BC (including best available estimates of the magnitude of those effects)?

Chapter 3, Black Carbon Effects on Public Health and the Environment

6. Does the draft Report accurately summarize and interpret the body of scientific evidence relating to the potential public health effects of BC?

7. Does the draft Report accurately summarize and interpret the body of scientific evidence with regard to potential non-climate environmental (welfare) effects of BC?

Chapter 4: Emissions of Black Carbon

8. Does the draft Report appropriately characterize available information on historical, current and future emissions of BC and related compounds in the United States and globally, and present this information clearly?
    a. Executive summary of Chapter 4 (Page 1-6) uses the phrase "provided detailed information regarding emissions from sectors that are the most significant contributors to U.S. emissions…" This is "correct" as long as one is considering contribution to emissions – as opposed to impacts to health and climate, etc. In fact, Chapter 4 has no text I could find that declares which sources are most significant; the chapter presents which are the larger sources, what their characteristic source properties are, etc. I would rephrase the summary in this regard, especially given that "significant" emissions may be those in special regions (e.g., Arctic, or population exposure, etc.) and these are not presented in Chapter 4. This also better connects the Executive Summary, Chapter 4, and Item 5 under Section 12.2, where source-specific and *location-specific* analysis is recommended.
    b. Section 4.1: Perhaps clarify that "domestic" sources (8% of global, and 52% mobile) may under count the BC emissions impacting U.S. receptors or sensitive U.S. regions, given non-U.S. shipping in U.S. waters, and transport from other non-U.S. sources proximal to U.S. territories and states.
    c. Section 4.1, lines 9-10: add text at end: "... due to different combustion conditions and various fuel specifications (e.g., onroad diesel, nonroad diesel, and heavy fuels used in diesel systems)." To read: *The ratio and mass of BC and OC varies by source. Diesel combustion emissions produce the largest fraction of BC while emissions from open biomass burning are dominated by OC due to different combustion conditions and various fuel specifications (e.g., onroad diesel, nonroad diesel, and heavy fuels used in diesel systems).*
    d. Section 4.1, lines 14-17: I don't understand this conditional statement. What makes sources with higher OC/BC ratios less strong candidates for mitigation? Also add "typically" to read: "Diesel sources typically have a low OC/BC ratio…" so that this is accurate. Not all diesel engine sources have low OC/BC ratios (heavy-fueled diesels).
    e. Section 4.1: The term "contained combustion" appears to be a new term for this report. This is ill-defined. Biomass burning may merit its own category, but combustion types include: a) open burning (uncontrolled combustion processes, sometimes "contained" within a geographic space, nominally inclusive of biomass burning); b) open combustion (inclusive of steam boilers, some gas turbines, etc., where combustion occurs in technologic devices under near-atmospheric pressures but under controlled fuel-flame conditions) c) closed combustion (inclusive of internal combustion, reciprocating engines such as diesels). This term is used in at least two places, defined vaguely in Section 8.3 for the first time. It should be defined at first use – if used – and perhaps an alternate term is better, per this comment.

f. Section 4.2: Domestic:International or non-domestic, and Global may be okay. But there are U.S. sources that are not "domestic" in location, and (more importantly) there are international sources that occur within the "domestic" domain, such as shipping. Merits clarification.
g. Section 4.3.1, line 31: delete “how” in the sentence for grammar.
h. Section 4.3.1, Page 4-3, line 17: okay, so extra time invested makes RPO estimates for biomass burning better than national EPA-developed estimates, but how accurate are they in general? Perhaps add sentence (if valid): *Nonetheless, biomass burning BC estimates remain more uncertain than engine combustion BC due to year-year variability and for other reasons addressed in this chapter.*
i. Figure 4-1 needs to be reformatted. It is currently alphabetically presenting source categories, but the report presents these in different groups: STATIONARY, MOBILE onroad, nonroad, OPEN BIOMASS BURNING. The figure should be grouped according to the report discussion for Figure 4-3, by reordering the presentation of whisker plots – and I would suggest using an inserted vertical line demarking the groups discussed in the report. And (of course) the text is too small on the x-axis unless you rotate the figure your use landscape page for full presentation.
j. Also related to Figure 4-1, and Page 4-4, Line 11, and Figure 4-2: Each time you “label” a source category that includes both distillate and heavy fuel diesel engine technology, you should be very clear – using the term “Heavy-duty on-road” each time. For example, the report says, “that heavy duty diesel vehicles have the largest fraction of PM2.5 that is BC (about 77%).” This is ONLY true for HDD vehicles using distillate fuel; it is not (for example) true for non-road HDD vehicles operating on heavy fuel (ships). This can be avoided by being very clear in the report that the term HDD vehicle is an EPA term referring specifically to onroad vehicles (e.g., trucks) or other clarifying statement. For example, Section 7.5 and Appendix 6 are clear in defining of “heavy-duty on-road vehicles”, but Section 7.6.4.4 is not clear that the discussion is limited to on-road HDDVs. I understand that that non-road HDD vehicles (including locomotives, construction machines, and ships) may be included in other definition used in this report, and this is generally clear.
k. Page 4-6, Line 4, related to Figure 4-3: The discussion leads with the BC pie chart, but that chart is presented second – I suggest aligning the pie chart order with discussion (or vice versa). Also, it would be useful here to use two gray-shades for mobile to begin to call out on-road versus non-road mobile. Later when locomotive and marine get discussed, this will assist the presentation of emissions by source category.
l. Table 4-1 is NOT a table, but a pasted image; this needs to be reformatted. Also, Table 4-1 currently says Table 4--1. (two dashes)
m. Figure 4-4 should carry forward the whisker plots for uncertainty from Figure 4-1. This should be doable as the ranges can be combined with some attention to the categories. The “box” values are unclear – are these the OC ratios referred to in caption?
    i. For a good example how other parts of the report do this, see Figure 2-13.

n. Page 4-9 and global: “data” are plural, so replace “this data” with “these data” and similar throughout report.
o. Table 4-2 and elsewhere (first presented in Table 4-2): All references to “Commercial Marine” should be associated with text or label that identifies this as U.S. registered commercial marine – i.e., US-flag. This means that it becomes unclear whether EPA is reporting U.S. registered commercial marine “operating in the U.S.” or inclusive of the oceangoing fleet of U.S. ships in foreign trade. The number is small, perhaps, but clarity would be merited.
p. Table 4-3 is NOT a table, but a figure. Replace and reformat.
q. Section 4.3.2.1, Page 4-11: Text says, “In general, diesel PM2.5 consists of about 70-80% BC and about 20% OC.[5]” This is ONLY true for diesel engines using distillate fuel – which may be true “in general” consideration of the population of mobile diesel engines, but needs to be clarified. Revise to read, *In general, diesel PM2.5 from combustion of distillate petroleum consists of about 70-80% BC and about 20% OC.*[5] And consider adding a statement: *Diesel PM2.5 from combustion of other fuels (e.g., marine residual fuels) have very different BC:OC ratios (and cite a paper or two).*
r. Table 4-3 is NOT a table. Replace and format.
s. Section 4.3.2.2, Page 4-13, Line 18: Modify to be more accurate in OC/BC ratio comparisons, to read: *Unlike distillate-fueled diesel mobile sources,*
t. Section 4.3.2.4: This area too, must consider that some very large stationary engines need not be limited to burning low-sulfur distillate - at least mechanically. There are power-generating IC diesel engines burning heavy oils and coal emulsions, and these would be ill-suited for catalytic DPFs.
    i. One more important caveat, given the range of non-road diesels. Many persons and most literature in the past fifteen years or so focus on catalytic filters – in others words, filters that can operate effectively across load changes that reduce exhaust temps below a certain temperature. These perform well only in the presence of low-sulfur conditions (thus low sulfur diesel fuels).
    ii. Some non-road equipment using fuel combustible in diesel engines can burn less costly higher sulfur fuels. These would poison the catalytic particulate filters, as our paper in Carbon Management discusses and cites (attached). Therefore, we went to the earlier literature on non-catalytic filters where the requirement for high temperatures must be maintained; these requirements match conditions for some non-road operations (e.g., marine engines), and may be more cost-effective than requiring low-sulfur fuels for all non-road conditions.
    iii. This is “counter” to conventional wisdom and perhaps counter to corporate memory within parts of EPA, so it bears noting for your masters. Given that EPA may be moving toward lower fuel sulfur standards for marine engines, you will need to note that 1000 ppm (the coming ECA standard) is still too much sulfur for catalytic filters.
u. Section 4.4.1, and Figure 4-7, Figure 4-8: Shipping is not depicted, and Arctic shipping is visibly absent, given the reports earlier discussions and later discussion of the Arctic as a special area of concern. The omission AT LEAST

needs to be noted, and could be rectified using existing data from published studies.

v. Table 4-5 and Table 4-5: How is the Arctic region, discussed above allocated within these domains? How is shipping - which is stated to be included - allocated in these domains, given that much of the activity does not occur within national boundaries but on the high seas. This source (Lamarque, 2010) needs to be clarified WRT to shipping and aricraft (international), and/or augmented with another source or new statement from the Task Force.
    i. Tables are NOT tables but pasted pictures. Replace and format.
    ii. The uncertainties discussed above will absolutely affect (confound) the ratios presented comparing emissions from other countries with U.S. BC emissions. This is not addressed and leaves a misleading sense of confidence in comparisons. Some text should address this. For example, if the emissions inventories vary by a factor of ~2x, then the comparisons with China could be ½ of 3.48 times (or greater). This comment applies to may bar graphs and comparisons as well, and should be carried into these discussions (at least throughout Chapter 4, and perhaps summary sections of other chapters, including Executive Summary). This comment directly addresses issue of "accuracy and clarity".

w. Table 4-6 is NOT a table. Replace and reformat.

x. Figure 4-10: If these carried uncertainty bars, then the figure would be really useful. Given the uncertainty in global inventories, do we think the uncertainties across nations (at least the leading emitting nations) are symmetric? I bet not. This would presumably involve combining the whiskers in Figure 4-1 with the data for this figure. Similarly, the uncertainties described elsewhere in this chapter seems to undermine the value of interpreting Figure 4-11. Do we really think the fractioning of sectors with a 2x uncertainty is represented by this chart? In particular, consider Figure 4-11 in light of discussion in Section 4.4.3.

y. Section 4.4.2, title: Rename to avoid the jargon term "parallel" to read: 4.4.2 Black Carbon Emissions Above 40 North Parallel of Latitude, or 4.4.2 Black Carbon Emissions North of the 40th Line of Latitude. Global replace 40^{th} parallel with 40^{th} parallel of latitude north of equator or 40^{th} line of North latitude – or (more easily) define the 40^{th} parallel as a term representing the 40^{th} line of latitude north of the equator.

z. Section 4.4.3, Page 4-28, Lines 30-31: This statement should include a citation to the AMSA report (at least) and perhaps to work that followed. I suggest a sentence that states something similar to the Asian reference to literature on inventories: "In addition, recent regional inventories for Arctic Shipping have been published (Corbett et al, 2010), and for global shipping with special regional attention on Arctic emissions (Paxian et al 2010).
    i. Arctic Council, Tromsø Declaration. In Arctic Council: Tromsø, Norway, 2009.
    ii. Arctic Council, Arctic Marine Shipping Assessment 2009 Report. Arctic Council: Tromsa, Norway, 2009; p 194.

iii. Corbett, J. J.; Lack, D. A.; Winebrake, J. J.; Harder, S.; Silberman, J. A.; Gold, M., Arctic shipping emissions inventories and future scenarios. Atmos. Chem. Phys. 2010, 10, (19), 9689-9704.

iv. Paxian, A.; Eyring, V.; Beer, W.; Sausen, R.; Wright, C., Present-Day and Future Global Bottom-Up Ship Emission Inventories Including Polar Routes. Environmental Science & Technology 2010.

aa. Table 4-8 is NOT a table. Replace and format. Table 4-8 comparison with EPA estimates is also less interpretable given uncertainties discussed earlier. A better representation would show a range by which EPA estimates were different (e.g., high).

9. Does the draft Report accurately summarize and interpret currently available information regarding the transport of BC emissions downwind of sources and the relationship between the location of emissions sources and the geographic region of climate and nonclimate impacts?
    a. Section 4.5, Page 4-32, Lines 35 to end: The paragraph makes a point (valid for the HTAP information used) that, "Given the paucity of anthropogenic sources of BC in the Arctic, a large fraction of the climatic impact of BC in the Arctic can be attributed to long-range transport." While this may remain true, it is conditioned on the fact that in-Arctic inventories (e.g., shipping) were either not available or coarsely included (1x1 degree resolution based on larger global inventories without Arctic-specific traffic data). Higher resolution, more complete Arctic inventories are now available to be included, and these may both address the "paucity" of data and modify the statement. I would suggest adding a sentence that says, "However, recent efforts to improve inventories of Arctic activity were not considered in the TF HTAP study described herein, and may improve the data for assessing impacts from in-Arctic activity."
    b. Figure 4-14: What do the arrows represent? Are they fractions of 1.00, by thickness, suggesting that Z% of total? The caption suggests so. If this is true, then the arrows need to be labeled with the value (multi-model mean percentage). Or, less visual, but more insightful: use a table to present the numbers behind this image.

Chapter 5 Observational Data for Black Carbon

10. Does the draft Report appropriately characterize and interpret the information on BC that is available from the observational record?
    a. Section 5.3.2, Page 5-6, Note 5: This note is cryptic. The explanation in Chapter 4 does not discuss this in the same way as suggested in the note.
    b. Section 5.3.3, Page 5-8, Note 7: Is the 40th parallel (Latitude 40N) an important parallel? Why? Is it simply cherry-picking the most useful (or one useful) US parallel? This seems arbitrary. Also, "Parallel" may be jargon, this is a line of latitude, more fully parallel of latitude, or some such, per Oxford English Dictionary.

Chapters 6-10: Mitigation Approaches to Reduce Black Carbon Emissions

11. Does the draft Report accurately reflect and clearly communicate information on the available technologies, control strategies, and costs of reducing BC emissions in various

sectors? Are there additional control technologies or mitigation strategies for specific sources or sectors that have significant potential to reduce U.S. or global BC emissions that should be included in the Report?

a. Section 6.4, Page 6-7, first paragraph: Good discussion of Arctic. There are now additional sources emerging on this (publication expected within early 2011, perhaps). Need to check due date for RTC and whether other literature needs to be included.
    i. Also this section should cite the technology assessment paper by Corbett et al that shows potential for non-catalytic DPFs, emulsions and other technologies. This won't get at the majority of BC from non-road mobile sources in the U.S., but it will be a faster and less costly path to reductions for some diesel sources.
b. Section 6.4, Page 6-8, Lines 31-34: Good discussion. Need citation to meeting notes, reports, or other official source.
c. Section 7.1: Technologies are mainly catalytic, but possibly including non-catalytic DPFs in some non-road sectors using higher sulfur fuels.
d. Section 7.4.1, Page 7-9: Need to dust off and describe the non-catalytic technologies too, for a smaller portion of stationary and non-road (marine) diesels using higher sulfur fuels.
e. Section 7.4.3, Page 7-13, Lines 1-8: Indeed. This seems to be adequate - perhaps euphemistic given the attention internationally at IMO. I suggest documenting the US position statements at IMO (MEPC) that would be stronger, and connecting the US opinion (EPA) with stronger statements by other governmental bodies internationally.
    i. Norway; Sweden; United States, MEPC 60/4/24 Reduction of emissions of black carbon from shipping in the Arctic. In Organization, I. M., Ed. International Maritime Organization: London, UK, 2010; Vol. MEPC 60/4/24. AND any later submittals by the United States.
f. Section 8.1, third bullet: Today, DPFs are mainly catalytic (especially for mobile sources) but non-catalytic DPFs may also serve some stationary and nonroad (marine) applications. These may also serve international goals because the low-sulfur fuel switching burden is eased or avoided. This is included in discussion in Section 8.4.3.
g. Section 8.3, Page 8-2, Lines 24-26: This is ill-defined. Biomass burning may merit its own category, but there traditionally is: a) open burning (uncontrolled combustion nominally inclusive of biomass burning); b) open combustion (inclusive of steam boilers, some gas turbines, etc., where combustion occurs in near-atmospheric pressures but under controlled fuel-flame conditions); c) closed combustion (inclusive of internal combustion, recipricating engines such as diesels).
h. Section 8.4.3, Lines 25-26, Page 8-6: At underlined at the end of sentence: "Catalysts are used to enhance the oxidation process, *especially to enable efficient particle filtering across transient loads where exhaust temperatures may not be maintained sufficiently high to achieve removal targets.*"
i. Section 8.4.3, Lines 24-26, Page 8-6: Add this sentence: *However, if diesel systems are performing in stationary or some nonroad conditions (e.g., marine)*

where the loads are not transient and the exhaust temperatures are high enough, then non-catalytic DPFs can be used with higher sulfur fuels (range of ~300-700 ppm or more).

12. Can the Panel suggest other reliable sources of information on the costs of reducing BC emissions, particularly for international sources, that should be considered in the Report?
    a. Corbett, J.; Winebrake, J.; Green, E., An assessment of technologies for reducing regional short-lived climate forcers emitted by ships with implications for Arctic shipping. Carbon Management 2010, 1, (2), 207-225.
13. Does the draft Report appropriately characterize the range and magnitude of potential benefits for both climate and public health that could result from reductions in BC emissions?

Chapter 11 Metrics for Comparing Black Carbon Impacts to Impacts of Other Climate Forcers

14. Does the draft Report accurately describe the range and limitations of metrics available to quantify and/or communicate the climate effects ofBC, to compare BC with long-lived greenhouse gases such as C02, and to compare among BC mitigation alternatives?

Chapter 12. Conclusions and Research Recommendations

15. Does the draft Report appropriately identify the highest priority research needs regarding BC?
    a. There is an inaccuracy in Section 12.1, Page 12-3, Lines 5-7. If you insert "Catalytic" before DPFs, the sentence is correct. However, as noted above, non-catalytic DPFs on mobile sources are feasible IF the exhaust temperatures are maintained high enough and IF the transient operating times are short. This is the case for large marine engines, some large stationary IC diesel engines, and these engines often use higher sulfur fuels. Admittedly, marine fuels need to be within the range of ~000s ppm – lower than heavy residual fuels, but typical for nonroad diesel fuels. Some additional attention to this technology development for these sources may be more cost-effective than 15 ppm diesel for all non-road sources.
    b. No mention of the Arctic as such. But this seems covered in general here.
    c. Key uncertainties section is very general, not really a clearly recommended action set.

Technical Appendices

16. Do the technical appendices to the draft Report contain any information that should be included in the main body of the Report?

Ivan Fernandez: Preliminary Comments

General Questions for All Chapters

1. In the Panel's view, does the draft Report accurately interpret and clearly communicate the findings of the current scientific and technical literature, including important uncertainties, pertaining to black carbon (BC)? Based on this literature, what are the Panel's views on the preliminary conclusions as summarized in the Executive Summary and in the key messages for each chapter?

Based on this reviewer‟s expertise on these topics and time for review, the document appears to be relatively comprehensive in its treatment of the various issues regarding black carbon, with appropriate referencing to the peer reviewed scientific literature. The preliminary conclusions are consistent with the review materials provided as a basis for the statements contained herein.

- First paragraph of EC could refer to the magnitude of range in importance for BC climate forcing relative to GHG and other drivers of climate.

- While the climate and human health benefits of BC emission reductions are clear and overwhelming, the report never asks the question "Are there potential negative consequences of BC controls?" Addressing this question somewhere in the report offers balance to the approach taken by EPA in this analysis.

2. Is the Panel aware of any additional, policy-relevant studies that should be included in the draft Report to inform the preliminary conclusions? Are there specific studies that should be given more or less emphasis?

There are places where points made elsewhere in the report might be referenced, but wherever key points are made the coverage of supporting literature, at least in areas where this reviewer has expertise, appear satisfactory.

Additional Questions for Specific Chapters

Chapter 2: Black Carbon Effects on Climate

3. Does the draft Report accurately identify and characterize light-absorbing carbonaceous particles, including BC and brown carbon?

The report offers an informative discussion of the processes and key terms associated with the broader topic of „black carbon‟. The figures presented were very informative. It would seem to be 2

beneficial to, early in the report, provide a clear delineation of the gradient from EC to non-C PM showing phases where certain terminology was applied, the light and energy characteristics, and the %C characteristics of the materials. This would range from 100% elemental BC on one end through BrC and OC phases (and their %C and light absorbing properties) with non-C particulates included. What this would do is synthesize the light and %C character of PM relevant to this discussion. Later it also becomes clear that mixed particle composition is equally important, and those concepts are (e.g., OC/BC) are easily confused with individual particle composition. [Figure A1-1 may do most of this, although it is buried in the appendix and it does not provide %C. Indeed, it would suggest there is a range of elemental C with varying refractory characteristics, but the figure does not readily clarify if that is truly 100% elemental C with different physical configurations, or a range of %C materials.]

4. Does the draft Report adequately explain and appropriately characterize the differences between BC and long-lived greenhouse gases such as CO2?
Generally yes, for this reviewer it seemed to be strong in clarifying the important differences between these two materials.
5. Does the draft Report appropriately characterize the mechanisms by which BC affects climate and the full range of climate effects of BC (including best available estimates of the magnitude of those effects)?
This reviewer felt it provided an adequate, and policy-relevant, treatment of this subject. It would require at atmospheric scientist to truly judge the level of appropriateness.
Chapter 3, Black Carbon Effects on Public Health and the Environment
6. Does the draft Report accurately summarize and interpret the body of scientific evidence relating to the potential public health effects of BC?
There is a strong case made for the importance of the human health effects of BC.
7. Does the draft Report accurately summarize and interpret the body of scientific evidence with regard to potential non-climate environmental (welfare) effects of BC?
The report does a seemingly good job of covering these topics for the most important consequences. It is not exhaustive on human health, nor on ecological implications.
Chapter 4: Emissions of Black Carbon
8. Does the draft Report appropriately characterize available information on historical, current and future emissions of BC and related compounds in the United States and globally, and present this information clearly?
Yes. 3

9- Does the draft Report accurately summarize and interpret currently available information regarding the transport of BC emissions downwind of sources and the relationship between the location of emissions sources and the geographic region of climate and non-climate impacts?
Yes.
Chapter 5 Observational Data for Black Carbon
10. Does the draft Report appropriately characterize and interpret the information on BC that is available from the observational record?
Yes.
Chapters 6-10: Mitigation Approaches to Reduce Black Carbon Emissions
11. Does the draft Report accurately reflect and clearly communicate information on the available technologies, control strategies, and costs of reducing BC emissions in various sectors? Are there additional control technologies or mitigation strategies for specific sources or sectors that have significant potential to reduce U.S. or global BC emissions that should be included in the Report?
Coverage appears accurate and no additions are recommended by this reviewer at this time.
12. Can the Panel suggest other reliable sources of information on the costs of reducing BC emissions, particularly for international sources, that should be considered in the Report?
Coverage appears accurate and no additions are recommended by this reviewer at this time.
13. Does the draft Report appropriately characterize the range and magnitude of potential benefits for both climate and public health that could result from reductions in BC emissions?
Coverage appears accurate and no additions are recommended by this reviewer at this time.
Chapter 11 Metrics for Comparing Black Carbon Impacts to Impacts of Other Climate Forcers

14. Does the draft Report accurately describe the range and limitations of metrics available to quantify and/or communicate the climate effects of BC, to compare BC with long-lived greenhouse gases such as CO2, and to compare among BC mitigation alternatives?
Yes, the treatment of these comparisons is well written. There is a tendency to present the BC and GHG vectors of climate effects as related in the climate phenomenon, but uncoupled in the carbon cycle. The linkages between the two could perhaps be strengthened as noted in a couple of specific comments below.
Chapter 12 Conclusions and Research Recommendations
15- Does the draft Report appropriately identify the highest priority research needs regarding BC? 4

Yes.
Technical Appendices
16. Do the technical appendices to the draft Report contain any information that should be included in the main body of the Report?
Mostly no, although addressing some of the issues raised in the body of the report might draw on some of the additional information in the appendices as a part of this review.
Specific Comments on the Text
p. 2-10, line 18 – How are ecosystems different from components in this list? It seems like replacing ecosystems with freshwater would then be a more symmetrical list of ecosystem and global system components.
p. 2-11, line 3-4 – Wouldn‟t this statement „technically‟ be true for BOTH BC and CO2? Concept intended seems valid, but needs rewording.
p. 2-11, line 29-30 – While there is no argument with the statement regarding direct effects of GHGs on climate; it is possible that there are indirect effects of GHGs through their influence on ecosystems. To the extent that CO2 fertilization of forests and CO2-induced ocean acidification alters the exchange of GHGs with ecosystems, there could also be important indirect or secondary interactions that influence the climate system.
p. 2-11, line 34-36 – This is a central point of the concern for BC relative to emissions control policies and climate. Are there any references that can support this logical conclusion from experimental studies to be cited here? Were unique events such as 9/11 or Beijing during the Olympics studied for local effects?
p. 2-13, line 10 – Speaking as a non-atmospheric scientist, I would simply ask if „volume‟ is the right or only metric to use here? It would seem as though surface area was most important, which would be a reflection of at least particle size as well as volume? Below is mentioned particle number, but not size, in this discussion.
Given the importance of methane in climate forcing, it might be useful to have BC compared to both CO2 and CH4 in Table 2-1
p. 2-17, line 1-3 – This box is neither a numbered text box nor a definition. It is not clear to me how these boxes are being used as this repeats text is already in the narrative.
p. 2-19, line 13 – missing a word in this line? „is‟?
p. 2-19, line 36 – „many studies‟ of what? Should state clearly the objective of this sentence. Is this the physics of radiative forcing in the atmosphere, or the flux of BC from the surface, or some other topic of the research? 5

p. 2-20, line 25 – „much higher‟ values of what?

Section 2.6.1.4 focuses on OC and sulfates and nitrates, without much discussion of the importance, or lack of importance, of dust/soil aerosols. Can their relative abundance and importance be referred to in this discussion of co-pollutants?
p. 2-35, line 11 – It is unclear to me what „until it is buried by fresh snowfall.“ means here. I presume the fresh snow refers to the new winter season and new snow, but perhaps this can be clarified.
p. 2-43, line 30-33 – In mentioning the role of forest fires on BC contributions to Arctic forcing; it would seem relevant to also indicate that warming then can alter the fire regime, representing a feedback on the climate system. There is expectation and some evidence that direct and indirect consequences of climate change will increase the frequency of forest wildfire, which is later referred to with appropriate references in this document and should be also indicated here (p. 10-4 lines 24-33).
p. 2-44, line 23 – What kind of „deposition“ is intended here? BC? Wet atmospheric?
Figure 2-21 – I like the synthesis and simplification provided by this figure. However, (a) the Hartman 2010 reference in the caption did not seem to be in the document references when I looked, and (b) everything ends in “damages”. While I personally concur that the “net” concerns are for a negative effect, some of the “effects” could be considered positive individually. CO2 fertilization, for example, would probably be welcome to those concerned with food and fiber production as a singular effect. Therefore, is this the best depiction? The figure is not titled „damages“ pathways, but simply cause-effect chains.
p. 3-2, line 38 Has EC already been defined?
p. 3-5, lines 16-21 This paragraph appears to be an appropriate discussion of some highlights of the ecological effects of PM. Many of the effects noted in the references here have to do with heavy metals, and not black carbon per se. Indeed, there is a field of science exploring the advantages of adding BC as biochar (see p. 10-9, lines 4-7 of this report) to soil productivity and carbon sequestration. Therefore, it seems like this paragraph would benefit from a statement indicating that the ecological effects literature of PM are strongly influenced by the chemical composition of PM, particularly heavy metals, with little available evidence for direct effects of the atmospherically derived BC alone although some relevant research is underway.
p. 3-7, line 12 The Levy, Hubbell and Tagaris refs are not in the references. The entire draft report should be carefully edited for completeness and accuracy in the referencing.
p. 6-6 Should this be labeled Figure 6-2?
p. 6-10 Lines 31-34 are a repeat of the text above.
p. 10-9 Throughout this page, as noted above, there are descriptions of PM and BC reduction techniques that seem well defined. However, I have not seen where the GHG impact, carbon 6

footprint, or other analysis is discussed, referenced, or carried out. If it is not here, it should be, and presented in a way that does not imply PM management is anything but positive. This discussion also should not imply that the climate impacts are eliminated when PM is reduced. As noted above, diverting OC from soil storage, and increased combustion efficiency which diverts OC to atmospheric CO2, has a climate consequence. In addition, where equipment, fans, road construction or vehicles are involved in these various mitigation techniques and consume fossil fuels themselves, this adds to the climate cost of the practice.
p. 10-2 lines 14-15 While numbers like 350 million ha are useful facts to inventory in this type of document, for many readers they are without context. Could this additionally offer what percent of the land base this represents, or the land base that is not under ice or in deserts and therefore is

subject to burning? This could be a useful context to many readers and underscores the importance of fire on this topic.
p. 10-3 Table 10-1. This is a very informative table to define the burning types. Is the sequence of listed “Land types” intended to imply degree of importance as a source? They are not alphabetical (e.g., rangeland comes above grassland). If no sequence is intended, then I would make them alphabetical. If the sequence is intended to indicate relative importance, then that should be stated here, even if it is already spelled out in the 1998 US EPA report from which this apparently was derived.
p. 10-7 line 33 The goal of increasing combustion efficiency makes sense to me for reducing PM, and perhaps BC. However, it raises the question of the effects on climate, since more efficient combustion converts BC+OC to CO2. Is there a discussion of the climate trade-offs here? Certainly the technique is warranted on air quality/human health concerns regardless. Likewise, below (p. 10-8 lines 31-38) mitigation by burning more frequently or using more of the biomass does reduce PM, but likely results in more CO2 emissions that would otherwise be stored as soil organic carbon since the C is released directly or indirectly by fire or utilization (unless the products made from the harvested material are very long-lived).
p. 11-1, lines 28-32 Following earlier comments, this bullet is fine, but reads a bit like BC reductions and GHG are separate issues regarding climate. I might follow the last line of this bullet with an additional sentence that reads: Indeed, some mitigation strategies for BC reduction result in increasing GHG emissions.
p. 12-2, line 20 In the parenthetical example of environmental benefits here, the term „deposition“ is used. Since many more will likely read this section than the overall document, seeing this term raises two questions. One, what kind of deposition? I presume this refers to BC on ice and snow, but it does not say that and perhaps should. Second, in the world of atmospheric deposition and carbon sequestration, many would wonder just how much EC is represented by the deposition of these particles that have a relatively short atmospheric life-time? Are they an important flux in the C cycle? In C sequestration calculations? I do not recall anywhere in the document there being a reference to the rate of deposition of C attributable to these PM materials, which would be good to include for readers with this context. Emissions estimates are not directly do not directly translate into kg/ha or g/m2 elemental C. EPA is certainly interested in these facets of C in the environment

Chris Frey: Preliminary Comments

Response: Overall, the report is well organized and generally thorough. The main findings appear to be reasonable and well-supported. There are opportunities to improve the linkage between statements of fact and citations to relevant literature. Important uncertainties are discussed qualitatively and in some places are quantified. The preliminary conclusions are reasonable. The report could more clearly emphasize key findings and implications, especially that BC is a key contributor to short-term climate change and that programs already underway in the U.S. will continue to substantially reduce emissions from transport.

EPA views the report as a "foundational vehicle" rather than prescriptive. However, to be of policy relevance, the document would be more useful if it indicated some specific recommended prioritized action items.

Chapter 2: The chapter does a generally good job of explaining the effects of BC on climate change. However, the report does not do much if anything to explain what are the impacts of climate change on human health and public welfare (including ecosystem effects). While it is not necessary to have a detailed treatment of this broad issue, some summary of the IPCC assessment report is useful to indicate the key policy context. Summaries in terms of global average direct forcing, while useful, may be a bit esoteric for a Congressional staffer or member of Congress to translate into impacts that affect constituents. For example, more could be said about topics such as changes in precipitation patterns and spring melting, and implications for agriculture and other endpoints such as sea level rise. (a summary of climate response from the endangerment finding could be very helpful).

Table 2-4: "Scientific uncertainty is very low" – is this meant to say that uncertainty is high? Or that certainty is low?

Chapter 3: The health effects appear to be reasonably summarized. The current health effects estimates that are the basis for the NAAQS are based on total $PM_{2.5}$ mass and total PM_{10} mass. BC is a component of these mass indicators of PM. Visibility impairment is an important public welfare impact. As far as a summary of non-climate impacts, the chapter is generally reasonable. The panel had other valuable comments which I generally support.

Chapter 4: Generally, the chapter is very good and clearly conveys a lot of background material and what is known about BC emissions, and limitations of the existing inventories.

p. 4-6; line 1, "Table 4-1 shows the actual tons per year of BC." Delete "actual." These are estimates or approximations.

Table 4-1 and many other tables in this report: please be very careful about significant figures. There is no way that the PM2.5 2005 total national inventory is known to 7 significant figures (5,521,456 short tons) with an implied uncertainty of only plus or minus 0.5 short tons. Given that in other parts of the document the uncertainty in BC emissions is described as a "factor of 2," there is no justification for having so many significant figures. In fact, even one significant figure is too many. However, it would be reasonable, perhaps, to show two significant figures,

and to explain that totals may differ from the sum of the numbers shown due to rounding. E.g., Table 4-1 should look like (two examples given below):

	Annual Emissions in Short Tons			Mass Ratios	
Source Category	$PM_{2.5}$	BC	OC	OC/BC	$BC/PM_{2.5}$
Open Biomass Burning	2,300,000	220,000	1,100,000	4.7	0.10
Residential	460,000	23,000	200,000	9.0	0.05
Energy/Power	710,000	44,000	65,000	1.5	0.06
Industrial	220,000	6,100	16,000	2.7	0.03
Mobile Sources	630,000	330,000	210,000	0.6	0.53
Other	1,200,000	6,700	110,000	17	0.01
Totals	5,500,000	640,000	1,700,000	2.6	0.12

	Annual Emissions in Million Short Tons			Mass Ratios	
Source Category	$PM_{2.5}$	BC	OC	OC/BC	$BC/PM_{2.5}$
Open Biomass Burning	2.3	0.22	1.1	4.7	0.10
Residential	0.46	0.023	0.20	9.0	0.05
Energy/Power	0.71	0.044	0.065	1.5	0.06
Industrial	0.22	0.006	0.016	2.7	0.03
Mobile Sources	0.63	0.33	0.21	0.6	0.53
Other	1.2	0.007	0.11	17	0.01
Totals	5.5	0.64	1.7	2.6	0.12

In addition to the summary given in the chapter, it would be useful to review the data quality ratings for AP-42 emission factors for $PM_{2.5}$ mass and the data quality of speciation profiles, including the relative range of uncertainty in estimates of the fraction of $PM_{2.5}$ mass that are in the form of BC for a given emission source. Such uncertainty is influenced by measurement errors for $PM_{2.5}$ mass and for the measurements of individual species from the $PM_{2.5}$ sample.

The artifact of the use of only one speciation profile for natural gas is an important point.

Chapter 5: no major comments.

Chapter 6: The overview of the impact of trends in BC emissions and role of some key emissions management programs is helpful.

The discussion of climate impacts seem to focus mostly on changes in global mean temperature. Some implications for other endpoints are briefly mentioned but could receive further discussion.

The report generally cites existing literature, but on p. 6-17 cites a study that is not yet published. Is this an acceptable type of reference to cite?

p. 6-21: although elsewhere in the report, there is some discussion of low utilization rate as another factor that would make retrofit unattractive, that point is not mentioned here (e.g., line 15) and should be.

Lines 16-23: point out that the estimated contribution of 8 percent depends on what seems to be a spurious estimate for speciation of PM2.5 from natural gas.

Lines 27-28: EPA emissions standards are not for BC; they are for PM mass. This distinction should be more clear.

Other issues:

- Implications of biodiesel for composition of PM and emissions of BC? Looking forward, there is likely to be more emphasis on biofuels and "low carbon" fuels.
- Given that diesel vehicle are typically about 30% more fuel efficient than gasoline vehicles, the marginal change in emissions when comparing diesel vs. gasoline is a reduction in CO_2 emissions per unit of activity but typically an increase in black carbon emissions. Is this a net improvement? How much does the net benefit or disbenefit of diesel vehicles depend on BC carbon emissions and control of such emissions? E.g., if a consumer is choosing whether to purchase a diesel vehicle, which one is more 'climate-friendly'?

Chapter 7:

Table 7-1 is very nice, but there is a substantial lack of transparency/documentation of the basis of these estimates. Basic supporting information that underly these estimates should be given in an appendix. Examples: vehicle age distribution by calendar year, and implied rate of turnover of vehicle fleets; emissions deterioriation; fleet mix; changes in VMT or other indicators of activity; etc.

My general comment about significant figures applies to Table 7-1.

Page 7-4: line 4-5: it would help to clarify if the percent reduction given here is per engine (vehicle) or is based on a fleet estimate taking into account fleet turnover, lingering contributions from legacy vehicles, and emissions deterioration from in-use vehices?

Table 7-1: be careful about significant figures. The basis for these numbers is unclear. There should be at least a brief explanation in the main text, and supporting information in an appendix. Some examples of key input assumptions to mention are: (a) growth in vehicle miles traveled (VMT); (b) vehicle lifetime distribution or other indicators of fleet turnover; (c) changes in relative market share of gasoline and diesel for each of light duty and heavy duty vehicles; (d) deterioration rates (if any) related to PM2.5 and BC emissions; and (e) any other influential assumptions, such as regarding changes in fuels, and vehicle technology.

Page 7-9, lines 13-21: here again, significant figures and uncertainty need to be taken into account. What is the uncertainty in the estimated reduction from 110,000 tons of BC in 2005 to 14,000 tons in 2030.

Page 7-11, lines 14-16 – another example where significant figures need to be conveyed appropriately.

Page 7-12, lines 1-7 – does this paragraph refer just to onroad vehicles? This should be more clear.

Line 7: The sentence that starts "Nonroad gasoline engines…" seems to start a new paragraph on a different topic, but there is no paragraph break. This text on nonroad should more clearly explain that 2-stroke engines typically tend to be smaller engines (except that locomotive engines are also 2-stroke) and are used for lawn and garden equipment such as handheld string trimmers, whereas 4-stroke engines are more widely used for lawnmowers and for larger nonroad equipment such as construction, farm, and industrial (CFI) equipment.

Lines 24-26: it would help to explain what tier of standard would apply to a remanufactured locomotive engine based on the date of remanufacture. My understanding is that the requirement is somewhat mitigated by possibly lack of availability of certified retrofit kits for some makes and models of engines.

Page 7-21, lines 5-8: What is the basis for this statement about lower BC fraction at idle or low load? This is one example (there are others) where a statement is made that appears to be factual but no supporting literature is cited or no explanation of the basis is given.

General issues for reducing vehicle emissions that should be introduced and discussed:

- Reducing demand would reduce emissions; what policy options could be considered for reducing transportation demand? E.g., landuse change
- Increased use of modal substitutions – pedestrian, bike, mass transit as alternatives to personal transport; modal substitutions for freight among truck, rail, waterway (inland, coastal)
- Fuel reformulation and substitution; electrification
- Engine technology
- Efficiency (reduced aerodynamic drag, idle reduction, hybrids, etc.)

Chapter 8:

Page 8-2, line 38, the term "direct PM emissions" is used in several places throughout the report, but the intended meaning seems to be "primary PM emissions." This terminology should be reviewed and harmonized with more typical usage.

Page 8-3: Define "ICI" (Industrial, Commercial and Institutional).

Page 8-5… is the 'hybrid PM collection system' one that is commercially demonstrated and widely used? If not, this should be noted. Discussions of technology should be more clearly

delineated with respect to those technologies that are demonstrated and available versus those that in development.

Page 8-7: lines 17-20. Smaller and older coal combustion units might also typically be lower in efficiency than newer units and thus have lower capacity factors (utilization) than newer or larger units. It is not clear that such units "may demonstrate greater cost effectiveness." This statement needs further justification either based on citation to relevant reference or perhaps development of a sensitivity analysis in an appendix that supports this statement.

Page 8-8. Section 8.6 needs some clarity of organization. First, there needs to be text between sections 8.6 and 8.6.1 that provide an overview of the content.

In section 8.6.1, "mass transfer operations" are mentioned as if there would be more explanation, but no further explanation is given.

Page 8-8, lines 23-25 seem to confuse two different types of efficiency metrics. One is combustion efficiency, which typically refers to how close the combustion process comes to complete oxidation of the fuel. The second is boiler efficiency, which as to do with the ratio of thermal energy available for input to another process area (e.g., steam cycle) relative to the thermal energy of the fuel (based on heating value and mass fuel flow rate). These are distinct concepts, and the text appears to intend to refer to combustion efficiency (which would affect emission rates per unit of fuel consumed) rather than boiler efficiency. For example, combustion efficiency of 60 percent would be truly horrendous. The paragraph from page 8-8 line 23 to page 8-9 line 3 needs to be rewritten.

Page 8-9, line 11, 'btu' should be 'BTU'.

Lines 12-14: fuel switching can include switching among coals that have different sulfur and ash content. This type of fuel switching usually entails capitals costs for replacing pulverizers and possibly enlarging the ESP due to differences in coal hardness and fly ash resistivity, respectively.

Lines 24-26: this sentence is a bit awkward but it is also not clear that is really valid. Coal sulfur content is generally higher than for distillate oil, and even if similar, the heating value of coal per unit of mass is much less, leading to higher SO2 emissions per unit of energy released.

Page 8-9, line 29. "Conversion from coal to gas or wood will reduce CO_2 as well as BC." This statement is not very clear. Presumably, the idea is that there will be a reduction in "emissions rates." If the concept is a reduction in emission rate, what kind of rate is implied (e.g., per unit of energy released during combustion)? Furthermore, such a comparison should take into account the fuel cycle emissions. For example, would natural gas obtained from hydraulic fracturing of shale have lower marginal CO_2 emissions impact than all coals? If not, then the statement as made is not only vague, but potentially not true.

Page 8-10, line 14. Not sure what is meant by "with poor conditions for workers"? The vagueness of this phrase coupled with an apparently lack of relevance indicate it could be deleted.

Line 26: not clear to the reader why numbers are given per ton of CO_2 equivalent – is this based on effects related to BC or to GHGs?

Bottom of page 8-10: it would be helpful to have some discussion of the technical potential to reduce BC emissions, as a bounding estimate, even though some portion of the potential may be expensive to realize.

Page 8-11, section 8.7.2. It would be useful to explain what portion of PM emissions from coke ovens are fugitives associated, for example, with removing coke from the oven, versus emissions from stack gases. Insight regarding control measures and their effectiveness depends on some basic process information. Section 12.2 of AP-42, Compilation of Air Pollutant Emission Factors, would be a useful reference here.

Page 8-13, the use of the word "contained" on line 6 is unclear. If this is meant to refer, for example, to stationary and mobile combustion sources, this could be stated in other ways.

Other issues:

- Tendency for electrification of countries as they become more developed
- In discussing power generation from fossil fuel combustion, it would help to quantify/compare uncontrolled versus controlled PM and BC emissions. This will help set the stage for discussion of the International context, for which some fossil-fueled power plants are uncontrolled with respect to PM and black carbon, or controlled using ineffective technologies such as cyclones or multicyclines.
- The uncertainties or needs for more assessment of the role of ESPs in controlling black carbon could be discussed more, leading to recommendations. There seems to be lack of sufficient information regarding the efficacy of ESP-based control of very small particles, and highly carbonaceous particles that are small.
- Role of plug-in vehicles on electricity demand should be mentioned. Thus, even though US power generation BC emissions have generally decreased, there could be future increases in power demand that might change the trend.
- The possibility of carbon capture and sequestration for fossil-fueled power plants could be mentioned. The use of an amine-based scrubber on a pulverized coal fired power plant may lead to some decrease in PM emission rate per kWh of electricity generated.
- Electrification that is based on an increasing share of nonfossil power generation would lead to lower BC emissions per kWh available from the grid.

Other Comments:

- The conclusions and executive summary should more clearly convey the following points:

- BC is a component of PM
- Existing emissions programs focus on PM, not BC.
- There is strong evidence that PM2.5 is associated with adverse health effects.
- In the U.S., health effects have been the main motivation for implementation of stringent emissions control programs for PM from both stationary and mobile sources. Based on mobile source emissions programs now being implemented, substantial reductions in PM emissions are anticipated.
- Although there are fewer health studies that specifically focus on linkage between BC exposures and adverse health outcomes compared to exposures to PM2.5 mass, there is scientific evidence that exposure to BC produces adverse health effects.
- Setting aside the role of BC with regard to climate, the health benefits of reducing BC emissions are substantial and sufficient to justify programs for emissions reductions.
- The climate co-benefits of reducing BC emissions provide additional incentive for targeting emissions reductions not just to PM mass, but also more specifically at BC.
- Whereas the U.S. has achieved substantial reductions in stationary source BC emissions, and is implementing programs that will substantially reduce BC emissions from mobile sources, open burning sources will continue to be significant and should be the focus of improved management practices.
- There is a significant opportunity for international leadership by the U.S. in transferring technologies and programmatic expertise to assist other countries with implementing BC emissions reduction programs in areas such as stationary sources, mobile sources, residential cooking, and open burning. Developing countries may be able to leapfrog from very high emissions to very low emissions technologies.

Jan Fuglestvedt: Preliminary Comments

1) In general, the report gives a thorough and clear presentation of the issues related to BC. The uncertainties discussed throughout the text could be given somewhat more emphasis in the summaries and conclusions. Some clarifications regarding time perspectives are also needed (see below).

The preliminary conclusions are reasonable; e.g. the final paragraph in 12.1. on uncertainties in net effect of a strategy. But this message could also be better represented in the Executive summary.

2) Additional policy relevant studies that could be included:

Balkanski, Y., Gunnar Myhre, Michael Gauss, G Rädel, E Highwood and K P Shine, 2010. Direct radiative effect of aerosols emitted by transport: From road, shipping, and aviation. *Atmos. Chem. Phys*, 10: pp. 4477-4489.

Andrews et al. 2010: Precipitation, radiative forcing and global temperature change. *Geophysical Research Letters*, Vol. 37, L14701, doi:10.1029/2010GL043991, 2010

Berntsen et al., 2010. Does black carbon abatement hamper CO_2 abatement? *Climatic Change Letters*, 103 (3-4): pp. 627-633

Penner, J.E. et al., 2010: Short-lived uncertainty? *Nature Geoscience*, (3): pp. 587-588.

Tanaka, K. et al., 2010. Multicomponent climate policy: why do emission metrics matter? *Carbon Management*, 1 (2): pp. 191-197

Aunan, Kristin, Terje Berntsen, Gunnar Myhre, Kristin Rypdal, David G. Streets, Jung-Hun Woo and Kirk R. Smith, 2009. Radiative forcing from household fuel burning in Asia. *Atmospheric Environment*, 43: pp. 5674–5681.

Rypdal et al., 2005. Tropospheric ozone and aerosols in climate agreements: scientific and political challenges. *Environmental Science and Policy*, 8 (1): pp. 29-43.

Jackson SC (2009) Parallel pursuit of near-term and long-term climate mitigation. *Science* 326:526–527.

New studies:

Manning, M. and A. Reisinger, 2011: Broader perspectives for comparing different greenhouse gases. *Proceedings of the Royal Society A*, in press.

R. B. Skeie, T. Berntsen, G. Myhre, C. A. Pedersen, J. Ström, S. Gerland, and J. A. Ogren: Black carbon in the atmosphere and snow, from pre-industrial times until present *Atmos. Chem. Phys. Discuss*., 11, 7469-7534, 2011

Studies that could be given more emphasis:

Rypdal, et al., 2009. Costs and global impacts of black carbon abatement strategies. *Tellus Series B: Chemical and Physical Meteorology*, 61 (4): pp. 625-641.

Shine K.P., Berntsen T.K., Fuglestvedt J.S., Skeie R.B., and Stuber N. (2007) Comparing the climate effect of emissions of short- and long- lived climate agents. Phil. Trans. Royal. Soc., a-Mathematical Physical and En gineering Sciences 365

As far as I can see, the results from Shindell and Faluvegi (2009) showing Arctic cooling effect of BC forcing at middle latitudes are missing. These results could be incorporated in the discussions.

3) In general, yes.

4) The differences between BC and CO_2 are stressed throughout the report, but could still be explained and illustrated better. The implications of these differences could also be given somewhat more attention. A figure showing the RF and dT response to pulses as well as sustained constant emissions of BC and CO_2 would illustrate the differences in temporal behavior of these components. The presentation of effects of BC (and other components) also needs a clear distinction between a *backward* looking perspective (as used in figs. 2-7, 2-8, 2-9, 2-11, 2-13 and 2-14) and a *forward* looking perspective (fig. 2.16). The purposes of these perspectives (attribution/understanding vs policy making) could be explained. I think the report would benefit from more emphasis on forward looking perspectives since the motivation is mitigation and policy making; e.g. as fig 6.4, but by component and/or by sector. In figure 6.4 it is not clear how big the contribution from BC is; only the combined effect of methane and BC. It would also be useful to know how deep cuts (% or mass) in BC and methane emissions that are assumed in the calculations behind fig. 6.4.

A figure like fig. 2.22 from IPCC AR4 WG1 (or an update) would be useful. The effects of BC mitigation on short term warming and rate of warming (assuming ranges for the magnitudes of the various effects of BC) could also be illustrated by a figure.

5) As far as I can see, the studies by Andrews et al. (2010) and Balkanski et al. (2010) are missing here (see references above).

12) These two papers may be useful (with costs estimates taken from IIASA/GAINS):

Rypdal et al., 2009. Climate and air quality-driven scenarios of ozone and aerosol precursor abatement. *Environmental Science and Policy*, 12 (7): pp. 855-869.

Rypdal, et al., 2009. Costs and global impacts of black carbon abatement strategies. *Tellus Series B: Chemical and Physical Meteorology*, 61 (4): pp. 625-641.

13) and **14)** The chapter on metrics gives a reasonably good overview of metrics and related issues, but needs to be more linked to (possible) overall climate targets or purposes of BC mitigation strategies (see below).

Various metrics are discussed but the report could apply some of these metrics to gain insight to the climate impacts of BC and BC reductions; i.e. $E_i \times M(H)_i$; and implications of this. See e.g. fig. 7 in Fuglestvedt et al., 2010.

The issues discussed in Section 6.5 are important and could be given more attention in other parts of the report. It could also be more integrated into the final recommendations. More examples and quantifications like fig 6.4 would strengthen the report.

More discussion of implications of figure 2-8 would be useful; i.e. implications on potential net effect of BC mitigation. I find Section 11.6. important and I think this should be more closely linked to chapters 6 and 12.

Regarding the adequacy of metrics: This depends on the overall purpose, which is, as far as I can see, not clearly stated beyond a certain focus on rate and short term warming. An evaluation of adequacy of metrics must be put into a context (see IPCCs report from Expert meeting on metrics, section 4.1.1.)

I think the report would benefit from a more focused and structured discussion of what role BC mitigation might play given various types of climate targets; i.e. a long term stabilization target, a short term target, or a rate-of-change target. In the introduction it is stated that *"BC offers a promising mitigation opportunity to address short-term effects and slow the rate of climate change*". It could be emphasized if this is given as the overall target (as opposed to a long term stabilization target in line with the statements from Cancun and Copenhagen).

If short-term climate effects and slowing the rate is the motivation of the BC study and mitigation, then this should be followed up throughout the report. It will have impacts of the use of metrics and potentially also further on identification of cost-effective multi-component mitigation strategies.

In the background motivation for the report it says "…and comparing those effects to the effects of carbon dioxide and other greenhouse gases". This leads to some methodological challenges regarding timescales, and needs some more attention and discussion wrt to implications for formulation of mitigation strategies for BC.

The multi-component approach (or "basket approach") is problematic if we try to "force" components with very different lifetimes into the same basket with one static metric like GWP100. One alternative is a multi-basket approach (e.g. Rypdal et al., 2005; see reference

above), or a single basket with a metric that is function of time (e.g. Manne & Richels (2001) or the GTP(t) from Shine et al. (2007)).

In some parts of the report it seems unclear whether the report is searching for metrics for i) choosing among various BC reduction alternatives or ii) across components.

More discussion is needed on how to handle the large uncertainties related to net effects of emissions and of emission reductions. A more explicit discussion of the risk of implementing polices with small effects, zero effects or negative temperature effects is, in my view, needed (i.e. more discussion of implications of figure 2-8 and the references on page 6-9,line 38-39). It would also be useful with more discussion of the weight given to other climate effects of BC; precipitation, etc.

D. Alan Hansen: Preliminary Comments

General Comments and Questions

1. Why is there no author attribution?
2. Much repetition of salient points and conclusions: in summaries of key messages, introductory paragraphs, main text, overviews (lead sections), and backup detail (subsections). I have mixed feeling on the need for this level of repetition. Some is clearly beneficial for hammering in important points, but isn't there some middle ground?
3. Commendably thorough and up-to-date. An excellent reflection of current understanding and uncertainties.
4. The discussion (e.g., in Section 1.1) of co-emissions (e.g., OC, SO_2, CH_4 and NOx) with BC and their complicating effects appears to be limited to those emissions emanating from the same (or simultaneously emitting?) sources. It should be made clear that these emissions, separated in time and space, may still combine in the atmosphere to complicate things.

Charge Questions

5. *Does the draft report adequately explain and appropriately characterize the differences between BC and long-lived greenhouse gases such as CO_2*? [This question strongly overlaps with Charge Question 14, as do Chapters 2 and 11.] To a large extent it does an adequate job. It covers the atmospheric lifetime differences, the differences in direct and indirect radiative processes, including albedo effects, the vertical and horizontal distribution differences and the very much more complicated physical characteristics and atmospheric behavior of BC/BrC relative to GHGs. It makes a valiant attempt to explain the difficulties in finding metrics to facilitate mediation comparisons; but, in my view, some of the explanation gets a bit obscure when describing the finding of researchers on this topic in Chapter 11 (see comments on page 11-16).

   Test boxes and figures are well chosen and executed. Figure 2-6 and Table 2-2 are particularly clever and informative in their depictions of particles.

10. *Does the draft Report appropriately characterize and interpret the information on BC that is available from the observational record?* The references to the sources of data are very thorough and up to date. The report author(s) has chosen to characterize only a subset of the referenced data. This is particularly true for the sediment records, but understandable if the intent is to focus on data for the U.S. as well to control the size of the report.
    I found several minor errors in the translation of cited papers' finding (shown in suggested edits, below), but generally judged the report to be an excellent characterization of the information available.

Comments and Suggested Edits
{Typos in brackets]

Page,lines
Ex-5. Re:"Controls applied to reduce BC will help reduce all of these harmful constituents." Shouldn't this be qualified to include only PM constituents? Otherwise, it might be construed to include gaseous material emissions that may or may not be reduced, depending on the control technology.

1-1,4 to 6. Stating that "substantial and immediate reductions in long-lived greenhouse gases are essential for solving the problem of long-term climate change," appears to me to be a bit off base in that it is optimistically naïve, in light of widespread geopolitical pushback and ignores the fact that long-term climate change is inevitable. We simply do not have the power to control the climate, only to influence it. It's the intermediate term climate change that's in question. I would suggest substituting "crucial" for "essential" and using a geologically more precise expression than "long-term."
1-2,35. Add "dimming" to the list of effects.
[2-19,12. This **is]**
2-24,27 to 27. Didn't Jacobson estimate indirect or semi-direct radiative effects for BC at a regional level?
2-10, Table 2-1, Cloud interactions, CO_2. Actually, the effect of CO_2 on cloud droplet acidity is a direct interaction. Also, "Dimming" should be added to the table.
2-36, Section 2.6.3. It's not clear to me why dimming is described as a non-radiative effect. Dimming is a result of light extinction (scattering and absorption) by atmospheric aerosols (including the suspension gases). Light extinction is a radiative effect, is it not? I believe the point trying to be made is that dimming can have non-radiative (indirect) consequences.
[2-37,14. …stability ~~**in**~~ may…]
[2-42,22. …suggested **that** the potential…]
2-43,11. Explain what the ranges (4-50 and 57-105) represent (standard deviations, 95% confidence intervals, …?)
[2-43,17. …will help ~~validate~~ **evaluate** and constrain modeling effort**s.]**
2-43, 16 to 20. This identifies an important collaborative survey, but gives no results.
2-43,36. In mentioning record high pollution levels, the period of record should be given.
2-44,17-19. Were these surface temperatures modeled or measured? (This may be answered in lines 30 to 31.) Was the organic matter modeled or measured and did it include BrC and, if so, did it include a high or a low specific absorptivity? If BrC was not included, the warming may have been underestimated.
2-45, Table 2-6, Precipitation Effects, U.S., second bullet, "…particulate matter emissions**, but not specifically those of BC."**
3-1, third bullet. In my opinion the statement that "we have insufficient information to fully assess the health effects of BC, relative to other constituents of $PM_{2.5}$" is a red herring. Strictly speaking we do not have sufficient information to *fully* assess the health effect of $PM_{2.5}$ itself. Tom Grahame has done a pretty good job of *partially* assessing the relative effects of BC, based on published data (see Bibliography Additions, below), in his presentation on Adverse Health Effects of Black Carbon (BC) to the National Coal Council on March 18, 2001 (attached). It appears to me that the argument that we cannot discriminate the relative toxicities of PM constituents is getting pretty weak.
3-7,12. The three references are not in the Bibliography.
[4-3,20. …because ~~**the**~~ there…]
4-3,27 to 28. "…(which is the OC plus the mass **of other elements in organic compounds such a H, O, N, and S ~~that accrues to primary OC through photochemistry in the atmosphere~~**)," Keep in mind that *all* OC occurs as OM.
4-5, Figure 4-2. Explain the percentage ranges in the parentheses. Reference footnote 4 in the caption.
4-8,9 to 12. This really needs to include the heteroatoms in OM as "unidentified."

4-11,21 to 22. I don't understand how this statement can be made, without some qualification. People have been measuring the specific light absorption of BC in diesel exhaust since the 1970s (e.g., Steven M. Japar and Ann Cuneo Szkarlata (1980). Measurement of Diesel Vehicle Exhaust Particulate Using Photoacoustic Spectroscopy. *Combustion Science and Technology* 24(5,6) 215-219. –here's a quote from the abstract, "A laser spectrophone operated at 514.5 nm has been shown to accurately monitor the "graphitic" carbon component of the airborne particulate emitted in the exhaust from a 2.3 liter Opel diesel vehicle.) More detailed studies have been carried out since.
4-16,20. I believe this is the first reference to the AR5 inventory. It should be defined. I note that the label on the first 3 bars in Figure 4-13 on page 4-29 is "Lamarque/Bond (ARS, 2000" [sic]. Does this mean that Lamarque et al. compiled the AR5 2000 inventory?
4-31,18 to 19. "…do not occur where the radiative forcing occurs and may occur downwind of the source region…" implies that radiative forcing only occurs in the source region, which doesn't make sense.
4-32,6 to 8. 80%(±25%) is not physically possible since it includes percentages >100%. 12%(±17%) and 8%(±17%) are physically impossible since they include negative contributions. This looks to me to be an inappropriate use of standard deviations for non-normal distributions. Some more reasonable method for expressing uncertainty or error should be devised.
4-32,10-19. This is a very confusing paragraph. It might clarify things if it was explained how contributions to surface concentrations can be minor while contributions to radiative forcing are major.
5-8,14 to 16. The statement that black smoke data are 3 to 4 times higher than collocated BC data appears to be poorly paraphrased from the Quincey paper so that it hinders interpretation. According to the abstract, what Quincey actually did was determine a "simple quadratic relationship" between black smoke and the signal from a colocated aethalometer.
5-11, Figure 5-3. I understand that this figure is intended to convey spatial variability of BC, but the labels, captions and footnotes lead to confusion as to what was measured. Footnote 9 says that black smoke reflectometers were used for the measurements, the caption on the picture (figure EC-4) says that EC was estimated, and the caption under the figure says BC gradients are displayed. I assume that EC and BC are being used interchangeably, but how were these values derived from the BS measurements? The key on the picture says "BC (absorption units)", but BS is measured in reflectance units and converted to $\mu g/m^3$, plus, the key did not reproduce clearly, so is mostly unreadable
5-12, Figure 5.4. The key, the caption and footnote 10 are inconsistent. The footnote suggests OM is displayed and calls it organic matter, the caption refers to OM as Organic Carbon Mass (which would be OC), and the key says Organic Carbon. Usually when one displays PM composition in a pie chart, OC has been converted to OM using some multiplicative factor, typically, 1.4 to 1.8. What was done?
5-13,13 to 15. I find the comparison in Figure 5-5b to be a little more complicated than "revealing a corroborative similarity." Note that there is exact relative correspondence in 1955 and in 1995, yet a factor of two difference in 1965. Why is this?
5-15, Figure 5-6. The graph shows the small increase starting after January 2004, not 2005, as the caption states, which suggests that the change in the analytical protocol may not have been responsible. The caption should explain the averaging basis for the black and red lines.
5-17, Figure 5-9. The caption should explain the averaging basis for the red line.

5-18,13 to 15. The rates of decrease given for Alert (-2.1 ng/m^3 yr) and for Zeppelin (-1.4 ng/m^3 yr) do not agree with the slopes of the lines in Figure 5-11, which are -3.5 ng/m^3 yr and -5 ng/m^3 yr, respectively. Also, it's hard to believe that a 42% decrease over 20 years at Barrow (determined from the trend lines) is not statistically significant.
5-19, Figure 5-11. There appear to be three symbols in the key, AO, PA, and AM, that are not plotted. Why? Could the explanation in the caption for the plotted circles be clarified? Firstly, I can't interpret what is written and, secondly, what is the point of plotting horizontal lines on a time series that shows a downward trend?
[5-20,6. Insert "nm" after 1020.]
5-20,11. In my view considering the OMI and AERONET results to "broadly agree" is being too kind. One point is a factor of ten off; many others are greater than a factor of two off. For a precise description of the agreement, why not give the correlation coefficient and coefficient of variation?
5-23, Figure 5-14 caption. GSFC should be spelled out as Goddard Space Flight Center, the first time it is used. Since GSFC has four facilities in four states, ranging from urban to rural, the specific location(s) should be specified. Actually, the **ratio**, not the percentage, is shown.
5-23,20 to 21. The ratio is approximately 6, not 10, during the summer in Figure 5-34. This makes the claim questionable that this exactly matches the ratio in Figure 5-2. Further, the reader may be able to identify the grid cell for Beijing as the urban one on the 40th parallel, but how is she supposed to figure out which grid cell represents GSFC if she tries to verify what the actual ratio is?
5-26.18-21. The implication that brown carbon may be lost from fresh snow due to sunlight-driven reactions, should be further qualified by adding, "However, neither Grannas et al. nor Hagler et al. specifically measured brown carbon nor the time evolution of light absorption. The reference should be to Grannas et al., **2004**, not 2007.
5-28,18 to 19. Change to read, "Studies of ice cores collected to date find associations between elevated BC and human activities. However, the trends vary significantly by location."
5-31, Figure 5-20 and caption. I cannot see the "gray shaded region (between the black and blue dotted lines)" in the top figure.
5-32, Figure 5-21 caption. More explanation should be given of what the box and stem plots represent.
[5-33,10. "…filter-based $PM_{2.5}$ **EC/OC** measurements in the United States…"]
6-1,6 to 10. The second sentence is a non sequitur, although couched in caveats. Further, it ignores the analysis of Jacobson and Streets referred to on page 6-6.
6-4,37; 6-5,2. What is SRES? Describe the scenarios.
6-9,32 to 34. I found two Magi references for 2009, neither of which is in the bibliography nor discusses brown carbon. See the bibliography additions, below. Magi, 2009, discusses measurements; Magi et al., 2009 discusses modeling.
6-13,37 to 38. I find the conclusion "that biofuel combustion causes eight times more premature deaths globally than fossil fuel combustion, largely because biofuel combustion occurs mainly in very populated regions of the world" hard to understand. Doesn't biofuel include wildfires?
6-15, 1 to 5. This discussion apparently is based on the questionable assumption that BC and OC toxicity are equal. To get a very different perspective, see the attached presentation by Thomas J. Grahame of US DOE.
6-16, figure 6-6 caption: Add for clarification: "The three open symbols on the right of the graph represent global values."

6-17, footnote 3: I'm curious why the more recent Section 812 analysis report is not cited.
6-21, second bullet. If all 11 million in-use mobile diesel engines were retrofitted with DPFs, the estimated cost would be 55 to 550 billion dollars. That's much higher than the costs shown in Table 6-2 on page 6-18.
[6-22,28. million **old with** improved."]
6-22,33. What factors?
[7-4,9. during **the** 1990]
[7-4,22. shows the **projected** emission]
7-5, table 7-1. What is the source of this table?
7-6, Figure 7-1. The legends for the first three graphs omit the on-road diesel key in red. Further, the order of legend key colors do not appear to follow the order of the colors in the stacked bars. This makes interpretation very difficult. Same with the pie charts.
[7-8,19. per **brake horsepower-hour]**
[7-9,33. Change "reflecting" to "non-absorbing", since sulfate particles actually scatter light, which can include refraction as well as reflection.]
7-10, Table 7-2. What does NPV mean?
7-10,7. Most people have no idea how many hours a diesel engine of a given size must operate to emit a ton of BC. Could this also be stated in way that is more understandable, such as hours for a specific hp engine? The fact that earlier in the report the range of costs per engine could range from $5000 to $50,000 for a DPF does not help much.
7-11,39. Define, LDGV.
7-17,4 to 5. Delete the sentence beginning "Because BC…" It is redundant with the first sentence.
7-18,11. I could find no PCV, 2009 in the bibliography. There is a Partnership for Clean Fuels and Vehicles (2009). If that was intended, the citation should be PCFV (2009).
7-20,16 to 17. Is the 1 billion gallons used during idling just for the US?
8-5, Section 8.4.2 should mention somewhere that, for a given particle size, ESPs are less efficient in collecting electrically conductive materials, such as carbon, than non-conductive materials.
8-8, Table 8-1. Were the capital costs for wood combustion controls not available or were they mistakenly omitted?
9-7,15. I Find it unlikely that residential wood smoke reduction measures will help reduce CO_2. Higher combustion efficiency (more CO_2) will offset the less fuel used (less CO_2).
9-7,35. Is "benzeno(a)pyrene" intended or should it be "benzo(a)pyrene?" These are very different aromatic hydrocarbons.
9-14,32. It is not strictly true that cooking with electricity produces zero emissions. Relatively substantial emissions can result from frying or broiling foods using electricity.
9-15,24. Should add CO as a problem of poorly made kerosene stoves.
11-2,34 to 35. I would suggest that dimming is caused by aerosol light extinction (scattering and absorption) and is therefore a type of radiative forcing. Thus, I don't quite understand this sentence.
11-5 to 11-16, results of Sarofim (2010). It would help in interpreting these results if the time evolution of the GWP for the two scenarios were plotted. I made a rough plot from the description and it appears that the area under the BC curve is greater than that under the CO_2 curve.

11-16, Figure11-5. The entire caption from Bond 2007 should be given: (**Figure 6).** Integrated forcing by aerosols emitted from burning 1 kg of fuel; results from 250 Monte Carlo simulations for each of three major sources. Note the scale difference. A few high points are excluded from diesel superemitter and traditional biofuel so the remaining distribution can be better seen. Zero is marked with a thick black vertical line to indicate the fraction of simulations that produce cooling or warming. Dashed lines mark quartiles.

11-16,16-27. This paragraph is very difficult to interpret. It refers to the benefits of BC reductions at the end of the century, but gives no clue what those reductions are. Further, Kopp and Mauserall state, "we estimate that failing to reduce carbonaceous aerosol emissions from contained combustion would require CO2 emission cuts about 8 years (range of 1–15 years) earlier than would be necessary with full mitigation of these emissions.
This sounds quite different than the statement in the report, "However, if this were tightened to accommodate the positive radiative forcing from carbonaceous aerosols (both OC and BC) from contained combustion source [sic] (fossil fuels and biofuels), then the 50% reduction of CO_2 would need to occur 1 to 15 years earier.."

12-2,16 to 17. Here, in the conclusions, is repeated the statement that, in the long run, CO_2 reduction will be necessary to mitigate climate change. While I don't take issue with the statement per se, somewhere in the report, the term, "long-run," should be qualified; it has to be pointed out that climate change is inevitable, natural, and historically responsible for major ecological, evolutionary, and cultural shifts. Our efforts to mitigate climate change ultimately may be shortlived and ineffectual. Who knows how the Anthropocene will play out in the scheme of things?

Bibliography additions:
Some of the 2010 references can now be updated to accurately show their publication status.

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Joseph Helble: Preliminary Comments

1. In the Panel's view, does the draft Report accurately interpret and clearly communicate the findings of the current scientific and technical literature, including important uncertainties, pertaining to black carbon (BC)? Based on this literature, what are the Panel's view on the preliminary conclusions as summarized in the Executive Summary and in the key messages for each chapter?

The report presents a comprehensive assessment of the current literature pertaining to black carbon (BC) emissions, control, and effects on climate. Key messages at the outset of each chapter nicely summarize the key findings and uncertainties in each area. While the report would benefit from a full read-through and edit to eliminate some chapter-to-chapter redundancies, overall it is well written and clear.

The listing of research needs is also fairly comprehensive, although some prioritization of needs would be helpful. This is particularly true in the discussion of pollutant mixtures. It is noted at several points that black carbon is always emitted as part of a complex mixture of pollutants, and that mixing, aging, and general transformations to a more internally mixed pollutant stream may affect the magnitude of the BC effect on climate. These uncertainties are qualitatively discussed within the report, and literature is fairly limited; EPA should consider whether these uncertainties are important research needs, and whether modeling could help address the potential magnitude of BC effects as particle surface composition changes with aging.

The importance of the heterogeneity of BC could also be addressed in more detail. The draft report notes as early as the second paragraph of the Executive Summary that "important uncertainties remain regarding… the impact of emissions mixtures…" but there is little discussion of the specific details. Atmospheric transformations, including heterogeneous condensation of other species on the surface of black carbon, may affect its radiative properties. It would be helpful if the report presented a more detailed discussion of particle size, composition, and surface composition heterogeneities, including the impacts of the transformation to internal mixtures, and addressed the current understanding of the relative importance of each to help guide prioritization of research.

2. Is the Panel aware of any additional, policy-relevant studies that should be included in the draft Report to inform the preliminary conclusions? Are there specific studies that should be given more or less emphasis?

The draft Report presents a comprehensive review of the available literature, including studies and papers that have not yet been published. I am not aware of any relevant major studies that were not considered.

11. Does the draft Report accurately reflect and clearly communicate information on the available technologies, control strategies, and costs of reducing BC emissions in various sectors? Are there additional control technologies or mitigation strategies for specific sources or sectors that have significant potential to reduce U.S. or global BC emissions that should be included in the Report?

Chapter 6

Chapter 6 presents a good overview of the current state of understanding, and the Summary of Key Messages is appropriately representative of the content of the chapter. There are a small few points of clarification that would help strengthen this chapter.

The final bullet on page 6-1 needs to be clarified. It is not clear whether the statement "Reductions in directly emitted PM2.5 can substantially reduce human exposure…" is referring

to the benefits of reduced exposure to PM2.5, reduced exposure to directly emitted PM2.5, or reduced exposure to black carbon. In regions where PM2.5 is dominated by secondary aerosol, this statement as written would be misleading without further clarification.

The BC globally emissions trends studies presented in Figure 6-1 were published in 2004, at the very beginning of the significant upsurge in primary energy consumption that occurred in China throughout the 2003-2008 period. Some discussion of the potential impact of this trend on global BC emissions from all energy-related combustion sectors would be helpful.

Figure 6-3 presents emissions trends through 2050, but the text discussions trends through 2100. The time period discussed in the text should correspond to the data shown in the figure.

Line 8 page 6-10, the meaning of "or more realistic strategies" is unclear

Line 35 page 6-14 it is unclear what is meant by "all three sectors" [having] "the greates mortality impacts." Presumably the text means that all three sectors have large impacts on mortality per unit of emissions – clarification needed.

Table 6-2, in the presentation of benefit to cost ratio, the cumulative benefit (v. cumulative cost) in constant dollars is at least as important as the cost-to-benefit ratio at some distant point in the future. Both need to be presented and discussed, perhaps by plotting the ratio (or annual benefits and annual costs) v. time for the full period of the analysis.

It will be challenging at best for many of the control strategies described in section 6.8.2 to be implemented in the developing world. Cultural barriers, the challenge of repairing broken stoves, and differences in cooking may make it difficult to achieve any significant penetration of improved cooking stoves into developing countries, particularly in rural areas.

<u>Chapters 9 and 10</u>

Chapters 9 and 10 provide a good overview of the challenges associated with controlling emissions from residential heating and cooking and from open biomass burning. While a full range of options is presented, it should be noted that implementing the fire control options presented in the report in many part of the developing world will be challenging at best. In addition, the cookstove discussion would benefit from a more detailed discussion of what might be applicable in which regions of the world. Local cultural barriers, resource availability, and the challenge of maintaining cookstoves in regions that presently rely on three-stone cookstoves make solutions in one area likely inapplicable to others. Going forward, a more refined region-specific assessment of the opportunities in this area would strengthen this analysis and discussion.

It is unclear whether the strong seasonality of the use of wood-burning appliances was considered in the discussion in chapter 9. For example, it is noted on the first page the US residential wood combustion is responsible for "approximately 3% of the domestic BC inventory." While this is true in aggregate, it is responsible for a much higher percentage of the emissions during the winter season (presumably a value closer to 10%) and as a result, may be responsible for a significant fraction of the indirect effects of BC through deposition on snow and ice –covered surfaces.

Similarly, the discussion on page 9-22 of 90-95% reductions in BC emissions per household seem very optimistic. Is there any literature suggesting a reasonable level of penetration?

Chapter 10 is comprehensive and adequately describes the relatively limited options for reducing BC emissions from open burning of biomass. Section 10.6 nicely discussions the challenges in implementation of any of these strategies in the developing world.

Mark Jacobson: Preliminary Comments

Page Ex-2. “BC…is commonly referred to as soot.” BC is one component of soot rather than soot itself. Soot particles consist of black carbon, organic carbon (e.g., PAHs, lubricating oil, unburned fuel oil), sulfate, metals and other components. The main component of fossil-fuel soot is black carbon. However, in small diesel soot particles, no black carbon exists (only lubricating oil, unburned fuel oil, sulfate, and metals).
****
Page Ex-2. “However, since GHGs are by far the largest.” Please remove “by far” since relevant climate response studies show that fossil fuel plus biofuel soot may cause 17-23% of gross global warming to date.
****

Page Ex-3. “BC influences climate through multiple mechanisms:” The list is missing three of the most important effects, including the following:
-- Cloud absorption effect: Heating of BC inclusions within cloud drops burn off clouds, increasing solar radiation to the surface (Jacobson, 2006; 2010; Ten Hoeve et al., 2011).
-- Semi-direct effect: BC in the air stabilizes the air and reduces the relative humidity, reducing the vertical transport of moisture and energy to a cloud, reducing cloudiness, increasing the penetration of radiation to the surface (Hansen et al., 1997; Ackerman et al., 2000)
-- BC-water vapor effect: the warming of the air due to BC increases evaporation of water vapor, itself a greenhouse gas that triggers further warming (Jacobson, 2010).
****
Page Ex-4. “The full effect of BC on climate must be assessed in the context of coemitted pollutants.” This is not correct in many cases. Instead, the effect of BC much be assessed in the context of only those co-pollutants that would be removed if a particular control measure was applied. Thus, for example, the addition of a particle filter to a diesel vehicle or engine would control the soot particles, but not the gases from the vehicle, so it is not necessary to consider the climate effect of the co-pollutant gases. The effect of BC must be assessed, in this case, in the context of removing the particles that the filter removes, namely the soot particles (which contain BC, POM, and sulfate). In the case of solid biofuel burning, however, the only practical control mechanism is the control of all the gases and particles, so full effect of BC in that case should be assessed in the context of all co-emitted pollutants.
****

P. 1-1, line 19. “Thus, BC must be studied in the context of the total emissions mixture coming from particular sources.” This is not correct in many cases. Please see the comment to p. Ex-4 for an explanation.
****
P. 1-2, lines 21-24. “The composition of the total emissions mixture is also key…” This is not correct in many cases. Please see the comment to p. Ex-4 for an explanation.”
****
P.2-1, line 6. “It is commonly referred to as “soot.” This is not correct. Please see the comment to p. Ex-2 above for an explanation.
****

P. 2-1, line 8. Please clarify that BrC may exist within the same particles as BC in soot or may exist in separate particles.
****

P. 2-1, line 11. “The full effect of BC on climate must be assessed in the context of coemitted pollutants.” This is not correct in many cases. Please see the comment to p. Ex-4 for an explanation.
****

P. 2-2, line 6. The list is missing three important effects of BC, previously listed under the comment for p. Ex-3.
****

P. 2-2, line 23. “BC has additional indirect effects….” These additional effects are not “indirect effects” but separate effects that should be listed separately as stated under the comment for p. Ex-3. “Indirect effects” have been specifically used in the literature as the first and second indirect effects.
****

P. 2-3, line 33. “…such as cloud formation”. Change to “…such as cloud formation and evaporation”
****

P. 2-4, lime 19. “Clouds containing suspended BC…” Change to “Clouds containing BC inclusions in drops and BC interstitially between drops…”
****

P. 2-5, lines 16-17, “…organic carbon, which is often described as non-light-absorbing.” Please clarify that all organics absorb UV and thermal-IR radiation and some selectively absorb short visible radiation. The UV, visible, and thermal-IR global direct radiative forcing due to organics was first calculated in Jacobson (2001a). UV and visible optical properties of brown carbon organics were provided in Jacobson (1999). Please include a discussion of these papers.
****

P. 2-5, line 22. “Per unit of mass in the atmosphere, BC can absorb a million times more energy than CO2. Please note additional references for that statistic and parameter (Jacobson, 2002, paragraph 64; Jacobson 2010, Table 4).
****

P. 2-5, line 35. “BC is a product of incomplete combustion, and is commonly called soot.” This is not correct. Please see the comment to p. Ex-2 above for an explanation.
****

P. 2-7, lines 2-11. “Many different forms of BrC exist.” Please clarify that Jacobson (1999) identified many major UV- and visible-light absorbing organics (BrC) in the air and Jacobson (2001a) provided global UV, visible, solar-IR, and thermal-IR global direct radiative forcing estimates due to such brown carbon.
****

P. 2-10, Table 2-1, first entry. The data-constrained atmospheric e-folding lifetime of CO2 is not 100+ years but rather 30-95 years, and more likely 30-50 years (Jacobson, 2005). This lifetime can change over time (hence the time series of a pulse CO2 emissions is often fitted to a curve with multiple time constants, as is done in some studies). However, this does not mean that CO2 has a long lifetime. It means only that the lifetime can change over time as CO2 becomes saturated in the ocean over hundreds to thousands

of years. In fact, the data-constrained lifetime of CO2 from 1960-2000 virtually never exceeded 100 years (Figure 1 of Jacobson, 2005).
****

P. 2-10, Table 2-1. The radiative forcing estimate of BC in the table should include the effect of BC on cloud absorption (Jacobson, 2006; 2010).
****

P. 2-10, Table 2-1. The statement "Likely 3rd largest contributor" to current global warming is unjustified on two grounds. First, it implies that radiative forcing of aerosol particles is a proxy for global warming of aerosols, when in fact radiative forcing is not proportional to climate response for aerosols nor are radiative forcings (e.g., from direct and indirect effects) linearly additive. Second, the radiative forcing estimates provided by IPCC (2007) did not account for the radiative forcing due to BC absorption in cloud drops and were based on results from several models that unphysically ignored internal mixing of black carbon as well as those that included it, thereby underestimating the BC radiative forcing (Ramanathan and Carmichael, 2008; Kopp and Mauzerall, 2010). Third, climate response studies show that BC is the 2nd-largest contributor to global warming (Jacobson, 2002, 2004, 2006, 2010). My suggestion would be, at a minimum, to state that BC is "either the 2nd or 3rd contributor) to global warming.
****

P. 2-13, line 5. "The absorptive properties of emissions plume from a specific source will depend on all of the co-emitted pollutants…." This is not correct in many cases. Please see the comment to p. Ex-4 for an explanation.
****

P. 2-14, line 8. "For example, coating of a BC particle by a clear (light-scattering) shell has been shown to enhance light absorption…" The most pertinent citations for this finding are Ackerman and Toon (1981) in terms of the phenomenon and Jacobson (2000, 2001b) in terms of the global radiative impact.
****

P. 2-16, line 22. "…several different kinds of forcing…" Please include the three additional types of forcing discussed under the comment for Page Ex-3.
****

P. 2-17, line 4. "The most widely utilized estimates come from the IPCC's Fourth Assessment Report…" Please clarify that the radiative forcing estimates provided by IPCC (2007) excluded the radiative forcing due to BC absorption in cloud drops and were based on results from several models that unphysically ignored internal mixing of black carbon as well as those that included it, thereby underestimating the BC radiative forcing (Ramanathan and Carmichael, 2008; Kopp and Mauzerall, 2010).
****

P. 2-18, lines 9-11 "Semi-direct effects are so uncertain that it is not even possible to determine direction (though there are preliminary indications that semi-direct effects may be cooling on a global level)" There is no study that truly isolates the semi-direct effect and no credible study showing that semi-direct effects cause cooling. Physically, the semi-direct effect should cause warming. It is not possible to isolate the semi-direct effect from other effects as all effects of aerosols on clouds are not linearly additive. The only climate response studies that include semi-direct effect, cloud absorption effect, and indirect effects together (Jacobson, 2006; 2010) show strong overall warming due to BC;

those that include the semi-direct and indirect effects together (Jacobson, 2002, 2004) also show strong warming.
****

P. 2-18, Figure 2-8. The second and third panels don't show any information and can be removed.
****

P. 2-19, line 3, "There are a number of factors that may contribute to the lack of consensus among modeled estimates of net global radiative forcing from BC." First, please clarify if you are referring to direct forcing or total forcing. Second, please clarify up front that the reason for differences is that most models are missing dozens to hundreds of physical processes and feedbacks, yet models that are missing physical processes are still included in assessments rather than excluded. While it is also true that differences arise due to differences in aerosol microphysical calculations and values of some physical constants and emission inventories, these are not the major reasons. The major reason is that most models are missing numerous physical processes and feedbacks so cannot properly simulate the climate response of aerosol particles and their feedback to clouds.
****

P. 2-21, Figure 2-9. Please include radiative forcing results from Jacobson (2000) and Jacobson (2001a) as well. Currently, results from only Jacobson (2001b) are shown.
****

P. 2-23. Table 2-3. The "semi-direct effect" is not an indirect effect. There are two specific indirect effects, the first and second indirect effects. The other effects discussed are cloud effects but should not be called indirect effect. Also, there is no evidence from a rigorous model that the semi-direct effect causes "cooling" or that its magnitude is "small"
****

P. 2-23, Table 2-3. Please add the "cloud absorption effect" (Jacobson 2006, 2010; Ten Hoeve et al., 2011). Please note that the combination of the semi-direct effect, the cloud absorption effect, and the indirect effects were found to be a net warming of climate in Jacobson (2010). The semi-direct plus cloud absorption effects offset the indirect effects at medium to high aerosol optical depth, as demonstrated by satellite data and model results in Ten Hoeve et al. (2011) and unpublished results.
****

P. 2-23, line 21. "More recently, Koch and Del Genio find in their review…that most model studies generally indicated a global net negative effect (i.e., the effect of atmospheric heating by absorbing aerosols on cloud formation and lifetime cause net cooling." Please state that none of these studies that find net cooling account for the cloud absorption effect and none treat multiple scattering of light within clouds accounting for the size resolution of cloud drops thus do not account for enhanced absorption by interstitial BC thus do not fully account for the semi-direct effect.
****

P. 2-25. Figure 2-11. Please include Jacobson (2004a) in the table. The estimated direct forcing extrapolated from that paper is +0.06 W/m2, ranging from +0.03 to +0.11 W/m2.
****

P. 2-28. Figure 2-13. Please include results from Jacobson (2001a).

****

P. 2-29, Figure 2-14. Please include results from Jacobson (2001a) and Jacobson (2001b).

****

P. 2-31, line 14. The paragraph is missing a discussion of the effects of biomass-burning particles and gases on climate from Jacobson (2004b).

****

P. 2-34, line 1. “There have been some efforts to translate regional direct radiative forcing estimates into regional changes in temperature.” The first such study examining the regional climate response of particles containing black carbon was that of Jacobson (1997). Additional global-scale climate response studies that showed regional results not discussed in this section include studies of Jacobson (2002, 2004a, 2004b, 2006, 2010).

****

P. 2-37, line 10. “Surface cooling combined with atmospheric heating from BC may increase the stability of the boundary layer and reduce vertical mixing.” First, please clarify that dimming occurs only during the day. All aerosols and aerosol-enhanced clouds increase surface temperatures at night. Modeled versus observed atmospheric heating combined with surface solar radiation reduction during the day and surface thermal-IR radiation increase at night due to aerosols are compared in Table 6 of Jacobson (1997) and discussed at length in Jacobson (1998).

****

P. 2-37, line 12. “This increase in atmospheric stability reduces natural removal processes for air pollutants, resulting in worse air pollution episodes” This phenomenon is referred to as the “Rainout Effect” (Jacobson, 2002, Section 3.9, paragraph 48).

****

P. 2-37. “A number of studies have found that dimming effects are particularly acute in certain regions associated with high aerosol pollution levels and the presence of ABCs” Please include the studies of daytime surface dimming together with atmospheric heating for Los Angeles (Jacobson, 1997; 1998).

****

P. 2-39. Table 2-4. Caption. “Overview of the different aerosol indirect effects.” The semi-direct effect is not an “indirect effect” It has its own name “semi-direct effect.”

****

P. 2-39, Table 2-4. The table is missing the “Cloud absorption effect” (Jacobson, 2006, 2010). The cloud absorption effect reduces precipitation (Jacobson, 2006, Table 3).

****

P. 2-40, line 29. “BC can have significant snow albedo effects…” BC in snow and sea ice was found to cause a reduction in surface albedo of 0.4% globally and 1% in the Northern Hemisphere (Jacobson, 2004).

****

P. 2-45, Table 2-6. “No studies were identified for U.S. temperature effects from BC.” All global modeling studies include results over the U.S. The text could be modified to state that the results are difficult to extract.

****

Chapter 2 in general. There is an overemphasis on radiative forcing and little discussion of climate response. As climate response studies are more relevant to understanding the effects of BC on climate, this is an important omission.

****

P. 6-6, Figure 6-1 caption. The reference for Jacobson and Streets (2009) is missing. It is in the attached reference list.

****

P. 6-9, line 29, The reference for Jacobson (2004) on biomass burning is missing and should be Jacobson (2004b) as in the list below.

****

P. 6-10, line 1, "…studies did not include the snow albedo effect or associated GHG reductions." Those studies also did not include the cloud absorption effect or the effect of BC heating on water evaporation, and Chen et al. did not include the semidirect effect.

****

P. 6-10, lines 3-4, "the different treatments in the latter studies resulted in larger estimates…" It was not the different treatments but the omission of many physical processes, including the effect of BC on snow albedo, the effect of BC on cloud absorption, the semidirect effect, and the effect of BC on water evaporation, that caused the differences.

****

P. 6-14, line 17. "The Anenberg et al. study…" Please clarify if these results quantify the effects of just BC or of soot, which contains BC.

****

P. 8-8, line 7, Under mitigation approaches, there is little discussion of the large-scale conversion to clean, renewable energy (e.g., converting electric power, transportation, heating/cooling, industry completely to electric power and hydrogen, where the electricity for both is derived by wind, water, and solar power, WWS). A plan describing such a conversion is given in Jacobson and Delucchi (2011) and Delucchi and Jacobson (2011).

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Ten Hoeve, J.E., L.A. Remer, and M.Z. Jacobson, Microphysical and radiative effects of aerosols on warm clouds during the Amazon biomass burning season as observed by MODIS: impacts of water vapor and land cover, Atmos. Chem. Phys. 11, 3021-3036, 2011

Jonathan Levy: Preliminary Comments

Overview:

As my expertise is on the health effects of black carbon, I focus my primary comments on the materials in Chapter 3, with some reactions to specific pieces of information in other chapters as well as the overall structure and content of the report.

In general, this report is technically proficient, thorough, and detailed. It appears to cover all relevant topics appropriately, though I defer to climate experts on whether the bulk of the material in the report represents the best interpretation of the technical and scientific literature. My one overarching comment is that the report is heavy on technical detail but light on the kinds of synthesized and substantive conclusions that might be needed to inform policy. For example, the key messages in the executive summary point to many specific lines of evidence, but not a synthesized sense of whether jointly considering public health and climate leads one to prioritize BC emissions reductions (and if so, through what interventions). If the report is specifically arguing that BC is both a potent climate forcer and has a direct effect on human health, with near-term benefits for both, and is arguably one of the only pollutants with these characteristics, it should say so more directly and emphatically.

Chapter 3:

While reasonable in content, I found this chapter lacking in a few key respects. Broadly, it is far shorter and less detailed or synthesized than any other substantive chapter. The direct health evidence of BC comprises only 4 pages, and is largely a recapitulation of some studies listed in the $PM_{2.5}$ ISA. Though a good number of epidemiological studies and endpoints are listed, unlike in other chapters, there are no numbers - no concentration-response functions, no estimates of the public health impact of BC (other than from biomass burning in developing countries). This is particularly important if there is a desire to help interpret the $/ton values reported later. There is also no discussion of the diesel health effects literature, which is quite pertinent to the topic of BC, and no discussion of the co-exposure topic that runs through some other chapters. While this chapter should not be a voluminous entry tantamount to the $PM_{2.5}$ ISA, it should be more thorough and specific to the context of this report, rather than a brief and somewhat disconnected summation of ISA evidence. A few more specific comments:

- p. 3-2: The report concludes that there is insufficient evidence to determine if there is differential toxicity of BC relative to $PM_{2.5}$; this is a reasonable conclusion, which I support, but it is clearly a controversial and important one. Rather than simply quoting the ISA and presuming that the reader will be familiar with the core arguments, it may be worth adding some brief text that summarizes some of the key lines of evidence. Most other chapters present the underlying evidence generated elsewhere in tables or figures so that the reader can examine it – a parallel effort here would be warranted. Similarly, at the bottom of p. 3-2, it is stated that “the effects observed with BC in these studies are

similar to those observed for $PM_{2.5}$…", but with no citations for this specific point, nor any figure or table.

- p. 3-3: Related to the above, the paragraph on studies linking BC and cardiovascular events is a bit limited. First, it was evidently derived from the ISA, which does not review studies published before the previous Criteria Document, so this summation largely includes more recent studies. It also doesn't provide the kind of synthesized evidence needed for the report – it is a nice listing of some key mechanisms and studies, but the report should establish the mechanism but then focus on the ultimate public health impact of BC concentrations, which is somewhat lost in the description of mechanisms.

- p. 3-4: There is definitely a bit more evidence linking EC/BC to respiratory effects than indicated. For example, Clark 2010 and Morgenstern 2008 both found associations with asthma development. Beelen 2008 and Maynard 2007 found an association with respiratory mortality. Ostro 2009 found an association with respiratory hospital admissions in children. There is also the array of diesel exposure studies that are not included in this review. The report is not obligated to list each and every study on the topic, given the size of the field, but should provide more of a reasoned synthesis of the evidence.

- P. 3-7: The valuation section could use some refinement and enhancement. First, the damage functions include the concentration-response function, which is never discussed explicitly or presented in the chapter. The description of VSL is also a bit lacking, and could be bolstered by using some of the more detailed text in the Section 812 analyses and elsewhere. Aside from the fact that numbers are not given, VSL is not carefully defined (important given its potential for misinterpretation), and EPA derives VSL mostly from wage-risk studies in which WTP is imputed based on additional compensation required in the labor market. This text gives the naïve reader the impression that it is based in individual surveys where people are asked how much they are willing to pay to avoid dying from air pollution, which will just confuse matters. Using some of the verbatim text from the Section 812 report would help clarify and provide consistency with other EPA materials.

Chapter 4

- P. 4-4: Figure 4-1 is hard to read and should be modified. In general, the captions and labels on many of the tables and figures in this chapter are hard to read.

Chapter 6

- P. 6-14: It is valuable for the report to include a specific study on the benefits of BC emission reductions for human health. However, it is a little difficult to evaluate the evidence and compare it with the other cited studies without more detail, especially since

this study has not yet been published. At a minimum, the text should mention which global atmospheric model was used, and the specific concentration-response function should be listed, since Krewski 2009 reports numerous values. I would also imagine that the core reason why the mortality impact per unit emission of BC is higher in South Asia than East Asia is population density, probably less so than a smaller impact on concentrations.

- P. 6-18: Including the $/ton results from Fann et al. is a nice way to make the impacts tangible. The range reported in the text on p. 6-18 is a bit misleading if taken out of context, since the range includes both concentration-response function uncertainty and variability across source types. This should be clarified. Also, Table 6-3 gives estimates by Pope and Laden – while people in the field know what this refers to, there is no mention in the text (here or in Chapter 3) of what these studies are, what they provide, why they differ, etc. The studies aren't even listed in the references, so a reader couldn't even dig up this information if they wanted to. This would be far more interpretable if Chapter 3 talked about how cohort mortality dominates the monetized health benefits, which studies most contribute, and what the functions look like. This doesn't have to be expansive, given the scope of the report, but a paragraph would allow the uninitiated reader to better understand what went into these values.

Chapter 12

- Per comments above, this chapter synthesizes some of the scientific background nicely but could be stronger if it reinforced some of the quantitative information presented in earlier chapters. For example, it appears in Chapter 7 that many interventions for mobile sources cost on the order of $10,000 per ton reduced, and in Chapter 8, the costs for stationary sources are on the order of $35-$500 per ton. Chapter 6 shows monetized benefits per ton, just considering direct public health benefits, of $210,000-$820,000 per ton. From this information, it would seem that BC controls are the consummate "no regrets" strategy, highly cost-effective even when not taking account of the appreciable climate benefits. This paragraph would be stronger if it made more tangible conclusions and synthesized some of the quantitative benefit and cost estimates presented in earlier chapters. These ideas are mentioned on p. 12-2 lines 18-28, but could be more emphatic and specific. Similarly, p. 12-2 lines 29 through p. 12-3 line 2 nicely summarize the importance of local for both public health and climate, but the text could be more specific – if public health benefits argue for controls in highly populated areas and climate benefits argue for controls where emissions will influence alpine regions, what is the intersection of those spaces in the US and elsewhere? This wouldn't need to pinpoint specific cities, but more specificity could help the non-scientific reader to understand what this means – for example, mobile source diesel controls in large cities north of the 40^{th} parallel would seem to be ideal targets.

Appendices

- There is text on page Appendix 2-3 that is not complete – parenthetical statements and mention of “references needed”. Table numbering is also off at times in this chapter, so this needs to be cleaned up a bit.

On p. Appendix 2-10, should LTO be below 3000 meters, not 3000 feet?

Denise Mauzerall: Preliminary Comments

Executive Summary

The executive summary provides a generally solid overview of the scientific understanding of BC, with some minor errors that can be corrected as noted below. However, it is dense reading for the uninitiated and leaves the reader unclear on what the next steps should be. Can any clear statement be made as to what priorities the US government should have on research and mitigation on BC? Should domestic initiatives be emphasized (if so, what specifically?) or international initiatives (if so, what specifically?)? Can a list be created approximately prioritizing mitigation initiatives (by sector? by location?) that are most cost effective per unit RF reduced or life saved?

Detailed comments:

Ex-1: Mention of the Himalayas and Tibetan Plateau should be included along with Arctic in first paragraph.

Figure A: This figure, although correct, is misleading to the reader who does not understand that coemitted organic carbon (OC) reduces the RF of BC from biomass burning emissions relative to BC from transport. This difference should be mentioned in the paragraph referring to Fig. A

Ex-2: Last paragraph mentions regionally and seasonally dependent effects of BC on climate. One thing that is not mentioned is that BC emissions can result in local surface cooling because they absorb heat above the surface, but lead to warming of the top of the atmosphere which has a global effect. May not need to be included in the executive summary though…

Ex-3: Why is the IPCC analysis being emphasized? In later work it has been found to be a low estimate due in part to some of the models not including internal mixing. A more recent meta-analysis (Kopp and Mauzerall, 2010) found the IPCC to be at the low end of other recent estimates (Jacobson, Ramanathan and Carmichael, 2008; Hansen et al., 2005, 2007).

The last paragraph is confusing to the reader.

Ex-4: Care needs to be taken in defining organic carbon (OC), black carbon (BC), brown carbon, etc. OC pops up here for the first time in the second paragraph. Brown carbon is not a term I've heard before – OC is typically used. The report must be explicit about what its definitions are and stick to them throughout.

The third paragraph is missing the obvious concluding sentence, "Therefore the same quantity of carbonaceous aerosols emitted from diesel engines result in greater warming than those emitted from biomass burning".

Ex-5. Last paragraph discussing co-benefits is good.

Ex-6. First paragraph discussing location and timing of emissions is good.

When mobile sources are discussed, ships are not mentioned. They are a large source of BC and should be included in the discussion.

Last sentence referring to new engine standards and retrofits – "may" should be changed to "would". Also, when fuel standards are mentioned, mention for the need for low sulfur fuel in order to allow the functioning of particulate filters is needed.

Ex-7. High priority research topics, third bullet, should include "emission factors".

One thing that seems to be entirely missing is mention of health impacts of inter-continental transport of BC. Although not a huge number, it is significant, and EPA through HTAP has been coordinating work in this area.

Chapter 2. Black Carbon Effects on Climate

p.2-2. Under "BC influences climate through multiple mechanisms, should include the "semi-direct effect" where BC can warm clouds and result in cloud evaporation.

Additional Studies:

Impacts of Inter-continental transport of PM including BC on health:

Liu, J, DL Mauzerall, LW Horowitz. Evaluating Inter-continental transport of fine aerosols: (2) Global Health Impacts, *Atmospheric Environment,* doi:10.1016/j.atmosenv.2009.05.032, 2009.

Radiative forcing of BC from Open Biomass burning:

Naik, V., D. L. Mauzerall, L. W. Horowitz, M. D. Schwarzkopf, V. Ramaswamy, M. Oppenheimer, "Sensitivity of Radiative Forcing from Biomass Burning Aerosols and Ozone to Emission Location," *Geophys. Res. Lett.*, VOL. 34, L03818, doi:10.1029/2006GL028149, 2007.

Update reference to paper appearing in ACPD with final ACP reference below. Can also replace EPA conference report with this reference:

Kopacz, M., D. L. Mauzerall, J. Wang, E. M. Leibensperger, D. K. Henze, and K. Singh. Origin and radiative forcing of black carbon transported to the Himalayas and Tibetan Plateau. Atmos. Chem. Phys., 11, 2837-2852, doi:10.5194/acp-11-2837-20112011, 2011.

Surabi Menon: Preliminary Comments

Chapter 1

The report does a good job of providing a comprehensive review of current research in a vaiety of areas for BC.

Chapter 2

This is a well writen comprehensive report that accurately described the mechanisms through BC affects climate. Useful suggestions to stregthen Chapter 2 include:

(1) An emphasis on the mixing state of BC that can enhance its absorbing properties earlier on. Some of the differences in results from different models as to the forcing from BC could be attributed to the way model treat optical and physical properties of BC and also the amount of BC that is present.

2) A table showing the radiative forcing of BC as a ratio of the mass of BC. Comparing forcing/g for different species helps put a perspective on the role that different aerosols play both in terms of the abundance of the species and their forcing efficiency.

For example Fig 2-9, p 2-21

3) The suggestion on 2-8, line 12 is indeed quite important and should be included in the conclusions or overview as well.

4) Page 2-14. This discussion would benefit from use of some quantitative estimates that indicate the change in light absorption when including physical transformation. As suggested in comment (1) above this causes discrepancy between models and thus is useful to look at range that exists currently.

5) Page 2-33. Line 7 to 15. A useful addition is to also account for how much change is expected between PD and PI BC amount when characterizing forcing ranges between different studies.

6) Page 2-40: Line 32. It seems that attributing 50% of sea-ice retreat to BC may be too large. Did the studies really attribute retreat as due to BC alone?

7) Page 2:45: Table 2.6 would be useful to include a regional distribution of radiative forcing effects.

8) Page 2-39, line 17: Better to imply there is also a spatial redistribution of precipitation (Chung and Ramanathan 2002, Menon et al. 2002)

Chapter 3

Can any study be cited for reduction in PAR due to BC that has importance for agriculture?

Chapter 4:

4.3: Line 29. Should also include the fact that emissions must include vertical distribution (as pointed out in 4-34, line 17) where possible and size distributions to reduce the uncertainty in models.

4.11. Table 4.10 Not too useful. Perhaps better in an Appendix?

Fig. 4.9 Color should be green not red for lower panel.

4.31: Line 13. Must also include a stmt that model greatly underestimate BC amount and optical properties compared to observations, especially in Asia. Deficiency in emission sources, sizes, optical representation or distribution from source to boundary layer and above.

Chapter 6:

P 6-9. Line 36-38. Are the difference between the two studies due to the way BC is treated (external versus internal mixtures)? These differences should be categorized a bit more in how the models treat BC so that differences from either physical, chemical or optical properties may be evaluated more critically.

P 6-14: Line 6. In preparation papers should not be cited unless web access is available.

Chapter 7:

P. 7.25: Lines 29-35. Time periods here would be useful.

Chapter 9

P. 9-13: Lines 1-5 More should be done to leverage revenues from carbon credits for mitigation opportunities.

General conclusions:

Time line of mitigation opportunities with maximum benefits in different areas such as climate, Arctic, Himalayas, health, visibility, agriculture, etc.

Richard Poirot: Preliminary Comments

This is an excellent draft report, covering a complex and important subject area with clearly written summary sections, supported by detailed literature reviews and discussions of key topic areas. The authors have done a good job covering and linking topics which are relatively well understood, and for which there's a reasonable degree of convergence on the direction and approximate magnitude of expected effects, with other topics where uncertainties currently remain large and where even the direction of expected climate effects is unclear.

A strong case is made that substantial, near-term reductions in BC emissions, in both developed and developing countries, are well-justified by expected, quantifiable reductions in human health effects resulting from reduced direct exposures to BC particulate matter. Reductions in BC emissions would also result in predictable and quantifiable improvements to visual air quality and reductions in the soiling and materials damage effects from BC pollution. While certain aspects of the climate effects of BC, such as the indirect effects from cloud interactions, remain highly uncertain, there is strong consensus that the direct radiative effects and snow/ice albedo effects of BC exert a significant warming effect which very likely exceeds partially offsetting cooling influence from indirect effects, and that the beneficial effects on climate would respond quickly to reductions in BC emissions. The combination of certain direct health and welfare benefits and highly probable, near-term climate benefits support the continuation, expansion and acceleration of current efforts to reduce BC emissions, as a "no regrets" policy to be pursued in both developed and developing countries.

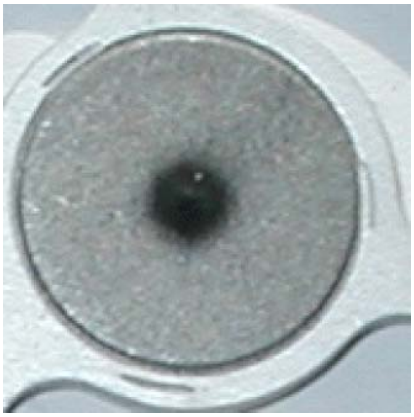
While the overall quality and clarity of presentation of the report are very high, following are several general subject areas where some additional discussion would be helpful:

- There is a clear explanation of the different climate responses over multiple time scales from short-lived particles vs. long-lived gases. However, there is not really much discussion of the implications of actions with a more rapid, near-term response time. The report strongly emphasizes that BC reductions should not be viewed as a substitute for needed reductions in long-lived greenhouse gasses over the long term, but is relatively silent on the unique benefits (if any) that might be expected from changes in more near-term influences. The Chapter 11 discussion of metrics for comparing BC effects to those from longer-lived climate forcers is informative, but is mostly presented in terms like "depending on policy goals" or "if a focus on short-term benefits is desired", but there's no discussion of "why" a short-term response might be a desired policy option. Beyond the general concepts of "delaying effects" or "buying time", might there be some improved potential (for humans or ecosystems) to adapt more successfully to climate changes if they proceed at a slower pace? Are there any sorts of leveraged or multiplicative benefits that might be expected if there were a near-term delay in the spatial extent and season length of reduced snow and ice coverage (for which BC reductions might be especially efficient)? Would this tend to reduce the near-term influence of all climate forcers?

- While the report emphasizes continuing uncertainties relating to the nature and effects of (and research needs to better understand the climate forcing effects of) "Brown Carbon" (BrC), there are inconsistencies in different sections of the report. Discussions on effects of biomass burning suggest that while BrC absorbs energy much less efficiently and for more limited wavelength spectra than BC, its overall effect on reduction of albedo over bright snow and ice surfaces may be as great as or greater than biomass burning BC due to its much higher concentrations. The light absorption efficiency of a particle with a BC core can be substantially enhanced by a coating of OC compounds. But other sections of the report advocate the BC/OC ratio (with BrC presumably included in the OC denominator) as an index to identify the most efficient control strategies from a climate-forcing perspective.

- Given the global importance of biomass burning activities (controlled and uncontrolled) as a source of BC (and other climate-forcing particles and gases), and also considering continuing controversy over the co-called "climate-neutrality" of biomass as a source of power generation and heating fuel, the report might benefit from some additional discussion on the influence of forest management practices (including prescribed burning) on carbon cycles, and on the total (gas and particle) climate forcing influences of controlled biomass combustion vs. alternative heating or power generation fuels (or processes). See for example: http://www.scientificamerican.com/article.cfm?id=wood-burning-power-plants-carbon-neutral-high-emitter

- The physical characterization of the BC particulate matter is relatively limited, and for the most part BC is described simply as a component of $PM_{2.5,}$ with both BC and $PM_{2.5}$ expressed in units of mass concentrations. However, most BC-containing particles are substantially smaller than 1 micron diameter and BC is an important component of ultrafine particles (< 100 nm). For some health, optical and absorption effects, particle surface area or particle number may be a better indicator of BC effects than mass concentration. There is also a significant BC component in (or rather on) coarse particles ($PM_{10-2.5}$ and larger) especially in urban areas, where coarse-mode particles, such as from re-entrained road dust, are often "coated" with BC (and substances absorbed onto it). This color photo (courtesy of George Allen, NESCAUM) shows coarse PM (> 2.5 microns) from a Harvard Impactor run approximately 100 m above street level in Boston. This coarse urban PM is black, not earth-colored, and likely results from a BC surface coating of coarse mode particles, rather than from a uniform BC composition. This emphasizes the point that composition of particle surfaces, and other aspects of particle morphology have important implications for the potential health, optical and climate forcing effects of BC-containing particles.



- The summary of direct health effects of BC is adequate, but seems somewhat understated and may give a misleading impression that health effects form BC can only be inferred because it is a component of $PM_{2.5}$. I don't disagree with the conclusion that "at present, we have insufficient information to fully assess the health effects of BC relative to other constituents of $PM_{2.5}$" [emphasis added], but think that BC (and/or the sources which predominantly emit it) has likely been implicated more frequently and for a larger number of cardiovascular and respiratory effects than most other components (or sources) of $PM_{2.5}$. The reliance here on findings from the 2009 PM ISA, which is focused on evaluating/supporting revisions to $PM_{2.5}$ and PM_{10} standards (and which is limited to studies published prior to May, 2009) may contribute to this slant. The absorbent nature of freshly formed BC particles makes them efficient collectors and carriers of co-emitted toxic organic compounds. Of all the major $PM_{2.5}$ components, BC tends to proportionately increase more than sulfates, nitrates, organics or crustal matter, as we move from rural areas to urban population centers (Hand et al. 2011). BC tends to penetrate to indoor environments more efficiently than other PM components (Sarnat et al., 2006, Lunden et al., 2008). The predominant sources of BC (diesel exhaust, more general "urban traffic pollution") are identified in the 2009 PM ISA and in several more recent studies as associated with a wide range of health effects. See for example the recent review by Grahame and Schlesinger (2009). Among the major $PM_{2.5}$ components (sulfate, nitrate, silicon, EC, OC, Na^+ and $NH4^+$) evaluated by Peng et al. (2009), only EC and OCM showed associations with cardiovascular and respiratory emergency room admissions of Medicare patients across 117 US urban areas. Diesel, traffic emissions and EC and OC combustion products were estimated as the most toxic components contributing to $PM_{2.5}$ mortality (compared to sulfates, nitrates & crustal material) in an expert elicitation of European air quality experts reported by Cook et al. (2007). Perhaps the summary statement could be revised to something like : "At present, we have insufficient information to fully assess the health effects of BC relative to other constituents of $PM_{2.5}$, but BC and its associated emissions sources have consistently been identified as among the most important contributors to a wide range of $PM_{2.5}$ health effects in the currently available literature."

Specific comments

p. Ex-4, 13th line from bottom: The term "instantaneous forcing" is likely not familiar to many readers and might be briefly defined here.

p. 1-1, line 10: You could revise this to something like "…a constituent of urban particulate matter [or "of the urban pollution aerosol"], concentrated primarily in fine particles ($PM_{2.5}$) and associated with an array…" BC exists primarily in submicron particles with a high proportion by mass and a large majority of urban BC particles by number less than 0.1 microns. $PM_{2.5}$ (filter sampling) is merely the way we most commonly sample for it in the US, but $PM_{2.5}$ is not the specific "cause" of BC health effects (you don't need "therefore"). BC is also present in coarse particles (>2.5um), especially in urban areas, where BC and associated mobile source contaminants often coat the surfaces of larger particles and likely contribute to $PM_{10\text{-}2.5}$ health

effects. The relatively higher concentrations of fine (and coarse) BC in urban population centers, is also an important contributing factor to BC health effects.

p. 2.5, line 19: Somewhere in this section, or in Appendix 1, you could add a reference to the excellent Moosmüller et al. (2009) review of light absorption concepts and measurements .

p. 2.5, line 31: You could add "and secondary [or "some"] organic aerosols" or perhaps remove VOCs from the list of associated precursors on the following line.

p. 2-6, lines 18-22: You could add something like "typically" or "usually" before "measured" in line 18, since BC is sometimes measured on PM_{10} or in other size fractions, and Aethalometers are often operated with a PM_1 inlet. The explanation that BC is expressed in mass units because emissions inventories are, isn't really correct, but is probably a harmless simplification here.

p. 2-8, footnote 1: You could change "other" in 1st line to "and/or". In the 4th line, you could say "The smaller particle size fractions of this dust can travel long distances". Most of the dust emissions are in larger size fractions; the metal oxides aren't unique to the fine fraction; and moderately-sized dust particles ($PM_{10-2.5}$) are also transported surprisingly long distances (as Sahara dust episodes in the SE US can often be identified as regional-scale PM_{10} events.

p. 2-12: As indicated above, it might be helpful to add something here, indicating any specific implications or benefits of a near-term climate response.

p. 2-19, line 12: Add "is" or "may be" after "This".

p. 2-26, line 19: A brief definition of "instantaneous forcing" would be helpful here.

p. 2-30, line 6: For clarity, you could move the "which together…aerosol mass" phrase to immediately after "sulfate and nitrate" on line 5.

p. 2-48: You could add "and soiling" to the "visibility" oval.

p. 3-1 lines 13-14: See general comments above

p. 3-5, line 2: The reference to Allen et al., 2006 should probably be to Allen et al., 1996.

p. 3-6, line 7: You could add "and quality" after "quantity.

p. 3-6, line 8: Replace "to see long distances with "to recognize and appreciate the form, contrast detail, and color of near and distant features".

p. 3-6, line 9: IMPROVE should be defined.

p. 3-6, line 10: You might revise the footnote to indicate that "The original IMPROVE equation included Rayleigh scattering (from natural atmospheric gasses) and factors for particulate sulfates, nitrates, organic carbon, elemental carbon, fine soil and coarse particles, with a

hygroscopic growth function to account for enhanced light scattering from water associated with the sulfates and nitrates. A recently proposed revision to this equation (Hand and Malm 2006) enhances the scattering from high concentrations of sulfates, nitrates or organics and adds terms for scattering and hygroscopic growth from sea salt and for light absorption from gaseous NO_2."

p. 3-6, lines 26-28: This is true, but since the focus here is on BC effects, you could add something like" At humidities below 85%, black carbon is the most efficient contributor to light extinction, per unit mass, of any aerosol species. Black carbon also exhibits the proportionately greatest increase of any major aerosol species as one moves from rural areas into urban population centers."

p. 3-7, line 28: Great Smokey Mtns. Seems like a somewhat odd single example here. Maybe precede it with "…the Grand Canyon or…"

p. 3-13, line 13: You could change the second "are" to "were".

p. 4-3, lines 27-28: This description isn't quite right. OM includes OC and other elements (primarily O and H) present in organic particle compounds. Some OM results from direct primary emissions and some is formed by secondary atmospheric processes (of which photochemistry is not the only process, as some secondary OC is formed at night, by condensation for example). Nor has all the additional O and H present along with C in OM "accrued to primary OC". There is no such thing as primary OC which is not already a component of organic matter.

p. 4-4, Figure 4-1: Its very hard to read the source category labels. Use a larger font. Also, its hard to believe that there's only a single SPECIATE profile (single black lines on chart) for such important (and often tested) source categories as heavy duty diesel exhaust and wood fired boilers.

p. 4-5, footnote 4: Maybe too much detail here, but you could add something like "It should also be noted nearly half [or whatever it is] of primary $PM_{2.5}$ emissions in the EPA inventory is composed of fugitive dust, but only a small fraction of this dust shows up in ambient $PM_{2.5}$ measurements. BC and OC contribute XX% and YY% of the non-dust US $PM_{2.5}$ emissions.

p. 4-8, line 11: You could add "crustal material" or "fugitive dust" before "direct emissions". See also previous comment. It looks from table 4-1 that previous gross overstatements of fugitive dust emissions have been at least somewhat addressed in these inventory numbers. I'm not sure if this is a relative improvement due to use of higher RPO open biomass burning estimates, or if adjustment factors have been applied to reduce the fugitive dust emissions. Perhaps, if it's still a significant fraction, you could add a footnote indicating what fraction of the large "Other" category is composed of (very, very short-lived) fugitive dust emissions.

p. 4-9, line 9 or elsewhere: Maybe add a comment about the fractions of open burning that are from wild fires, prescribed fires fires and agricultural burning – to get a sense of what's potentially controllable and what's not.

p. 4-17, line 1: In table 4-4, the US fraction is (5.1%) closer to 5% than 6%.

p. 4-18, line 4: In table 4-4, the US fraction is (2.3%) closer to 2% than 3%.

p. 4-27, Table 4-7: It doesn't seem logical (in 3rd column) that 0.38 of mobile source BC emissions but only 0.06 of fossil fuel BC emissions originate north of the 40^{th} parallel. Why do our northerly citizens drive their cars more but use less electricity and home heating fuel?

p. 5-3, lines 5-3: This discussion (or elsewhere in this section) could be expanded to include consideration of the multiple wavelengths that can readily be included in many optical absorption methods. To focus on the so-called BC here is to miss the point that a wide range of visible and non-visible "light" wavelengths are absorbed (and converted to heat energy) by different kinds of particles and mixtures.

p. 5-6, line 9: Its not clear what this set of 45 monitors refers to here. If you mean Aethalometers and PSAPs, you might at least refer to them as "continuous" or "semi-continuous".

p. 5-6, footnote 4: why not the IMPROVE data "after 2005" rather than "2005-2007". Did the method change again in 2008?

p. 5-12 figure caption and legend: I suspect, but don't really know that the blue pie slices labeled "Organic Carbon" in the legend, but referred to as "Organic Carbon Mass (OM)" in the caption represent contributions from "Organic Matter" (which is not "organic carbon mass") which includes organic carbon and associated O and H present in organic carbon-containing compounds. I also suspect that the yellow and red slices labeled "sulfate" and "nitrate" also include associated ammonium, and if you're using Neil Frank's SANDWICH approach also probably includes some associated water. If true, or approximately so, I suggest you change the legend species names to "Sulfates", "Nitrates" and "Organic Matter", and in the caption, replace "Organic Carbon Mass" with Organic Matter".

p. 5-13, Figure 5-5a: Can't read dates in X scale or concentrations in Y scale. Use a bigger font.

p. 5-14, lines 17 & 18: The reference should be to the Bahadur et al. (2011). However, I suggest deleting this last sentence, as some issues have been raised (and comment to editor submitted, Bret Schichtel, NPS, pers. com.) regarding that paper. Among other things, long-term data from 18 rural/remote IMPROVE sites were averaged together in a "20-year trend analysis" with IMPROVE data from 3 urban sites which operated for only 1 to 8 years each. Excluding the

short-term urban sites substantially reduces the observed BC trend, which is still generally downward but is neither 50% nor could the original trend be accurately described as "rural".

p. 5-16, line 1 and Figure 5-8: Its not clear here whether you mean BC concentrations in "San Francisco Bay" or in air above the "San Francisco Bay Area". I assume that you mean Bay Area, but don't know for sure. Also, the reported units on the Y scale (mg m-3) don't look right, and I assume you mean ($\mu g\ m^{-3}$) – or maybe those really are concentrations in the bay....

p. 5-17, Figure 5-9: Aethalometer should be capitalized in caption. Presumably these data have been corrected for various artifacts, such as identified by Virkkula et al. (2006) and Cohen et al. (2010).

p. 5-19, line 18: You could delete the 1st "surface". Satellites can be very helpful in identifying spatial patterns, but its questionable how well they can quantify surface BC concentrations.

p. 5-32, lines 3-4 and Figure 5-21: Its not so clear what the "marine" category refers to. Could it include biomass or other land-based BC emissions that arrived at monitoring sites after passing over ocean salt emissions areas? Site names in figure are not legible.

p. 6-5, Figure 6-1: Would be more helpful if the scenarios were described, or if you at least described the most extreme high & low scenarios.

p. 6-14, line 8: This reminds me to ask if the term "anthropogenic BC emissions" is used consistently throughout the report (seems like it should be and made clear up front), and if so does it exclude "natural" wild fire emissions" and are agricultural burning, prescribed forest burning and human-caused forest fires included or excluded from the "anthropogenic BC" category?

p. 6-14, lines 29-30: It doesn't seem logical that reducing BC emissions from industrial sources (many of which are from relatively elevated stacks and/or in "industrial areas" rather than in residential areas) should be so much more effective at reducing human exposures than cutting transportation BC emissions – which I thought were a larger % of total than industrial, and emitted at ground level and predominantly in populated urban areas.

p. 6-21, lines 13-14: Are these costs, like others on this page, expressed as $/ton? Indicate units. Also, it might be useful to include costs/vehicle for the new engine and retrofit categories, and/or to include an occasional reference back to (ratio to) the previously reported estimates of benefits in $/ton.

p. 6-22, lines 1-4: Agricultural burning is a subset of this category for which various non-burning alternatives exist and have been successfully employed in some states (CA, ID, OR, WA) and have not necessarily incurred high costs in terms of $/ton $PM_{2.5}$.

p. 7-6 and 7-7, Figures 7-1a, b and c: The legends for these 3 stacked bar charts seem to be missing the on-road diesel and on-road gasoline categories.

p. 9-1, line 6: while wood smoke may have a relatively high OC/EC ratio, wood smoke OC is also relatively efficient at absorbing shorter wave radiation, as it often "measured" using the 380 nm channel on dual wavelength Aethalometers, and wood stove use increases with colder temperatures coinciding with snow cover and proximity to the arctic.

p. 9-1, line 12: You could add at end of sentence ", and highest emissions tend to coincide with (cold winter morning) periods of poorest dispersion."

p. 10-5: Since the natural fire sequence is not what got us into the current altered climate state, it doesn't seem logical to further suppress natural wild fires (or prescribed burns which may be making up for past policies of unnatural suppression). As noted in general comments, it might be useful to include some discussion of how fire and other forest management practices may alter the general uptake and release of gaseous carbon by forests and grasslands – not just the BC or BC and OC taken in isolation.

p. 11-8, line 23: "If the focus is on achieving immediate climate benefits…" As indicated in general comments, there is no discussion of "why" short-term responses may be desirable.

p. 11-13, Figure 11-4: The ranges here seem awfully broad (and exactly the same width for each source category). How were they calculated, or is this more of a conceptual diagram? Would it make sense to fuzz up (blend -shade the background colors in) the middle zone – to emphasize that there's not really a distinct black/white separation between the different forms of carbon emitted by these sources?

p. 11-13, lines 11-12: While SO_2, NOx and VOC precursors may form additional light scattering aerosols within the plume, NO_2 absorbs light, and in combination with VOCs, forms ozone, a short-lived greenhouse gas with a short-term GWP per molecule of 1000 times CO_2, and which adversely affects human health and diminishes plant photosynthetic (and CO_2 uptake) rates.

p. 12-2, lines 8 and 18: You indicate that BC mitigation strategies "can" make a difference for climate, but that these mitigation strategies are only "likely to" provide substantial health and environmental benefits. This seems backward, and you could change "are likely" to "would" on line 18.

p. 12-2, lines 23: You could insert "substantially" before "exceed" as many of the benefit/cost ratios presented or implied in Chapter 6 were quite large.

p. 12-3, lines 18-19: This identified "strong need for additional quantitative analysis…" seems somewhat inconsistent with the general observation that health benefits (including mortality) substantially exceed costs for many BC source control strategies, and that (especially for sources with high EC/OC ratios) the "ancillary" climate benefits are almost certainly positive – but we

should wait (while the mortality continues) until we can quantitatively figure out which of these "no regrets" strategies we should have most cost-efficiently perused first…You might at least delete "strong" from line 18.

p. 12-4, line 2: You could add "(and scattering)" after "absorption".

p. 12-4, line 12: You could add "", including both surface-based and remote (satellite) sensors," after "instruments".

p. 12-4, lines 14 and 16: You could add "characteristics and" before "processes". The intent would be to emphasize needs for better measurements to characterize BC and related particle morphology, size distributions, number count, surface area, spatial and temporal patterns, etc.

p. 12-5, line 12: After "characterize" add "absorption and scattering" and after "BC" add "and related aerosol species and mixtures".

p. 12-5, line 18: Could add "and related species" after "BC".

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Ted Russell: Preliminary Comments

First, the report is generally quite good and well written, and I think for the most part at about the right level for the likely audience. Some parts could be shorter/more targeted to BC (e.g., Chapter 8). One area that I think might be more prominent is when they cover the health benefits of BC control that, alone, make further reductions look good, independent of climate effects. I would put this finding further up and with greater emphasis.

The discussion of long-range transport is fine, though I might emphasize more that there are very strong local gradients and that BC is classically viewed as a more local, even micro-scale pollutant. Much of the exposure of concern is due to very local sources, be they automotive or indoor biomass and fossil fuel combustion. As they amply show, local concentrations can be an order of magnitude higher than rural, regional concentrations. When it comes to climate, longer range transport becomes more important.

The metrics section covers the right material, and makes the appropriate point that a single metric is likely insufficient to provide policy makers a total grasp of how to do their job, and as is said, the best choice depends on desired endpoint. However, this is said way to many times in this section. In Section 11-4.3 and 11-4.4, follow3ed by 11-5.5, it is not apparent if researchers have tried to develop cost-effectiveness metrics or economic impacts for BC in comparison with CO2. If not, say so specifically. They refer back to 2.7, which basically says this is really difficult at this time. Both section 2.7 and the related sections in Chapter 11 can be more definitive as to what has and has not been done. If there is no citable work in this area, so state.

The Conclusions Chapter provides a reasonable summary, though, again, I would move up the more direct health effects to say, based on the potential, immediate health benefits, alone, control of BC is attractive. Climate benefits will provide added, potentially even greater benefits, and may be an even more cost effective strategy than reducing CO2 to achieve more immediate benefits. The research needs should be re-thought and re-expressed. For example, I don't see the current #1 as #1, and I would take issue why having a standard definition or improved instrumentation would make a big difference. The current monitoring devices deployed in the field do a pretty good job, and the cavity methods are quite attractive. What is arguably of importance is expanding the observations. For Research Area #2, the paragraph does not show how the investigations will improve monitoring. For #3, the research of which I am aware actually suggests that the US BC inventory is in reasonable shape. Might be more specific and suggest how this might be done. For #5, or somewhere else, monetizing the benefits of BC control is important. While this report was more in response to the climate issue, health impacts are very important, and there is a need to better understand the relationship of BC (and the related emissions) to heath. #5 deals primarily with analysis of controls, but should be extended to basic studies on health effects.

Chapter 8 read too much like a general manual on air pollution control, without being specific enough to BC. Control effectiveness should be given in terms of BC. Control costs should be given in terms of $/ton BC. Concentrate on the sources where meaningful reductions in BC can be found. This pretty much limits the discussion to the major sources identified in Chapter 4.

Chapter 7 needs to look, and provide information for, future BC controls from mobile sources. The $11,000-$12,000/ton is an interesting value, but what is it in terms of controlling BC beyond modern engines. Modern diesels are quite a bit cleaner. Looking at the US Mobile source BC pie, BC emissions from diesel engines in 2030 are expected to be about 25,000 tons/year, versus about 400,000 tons in 1990. What would be the cost of getting that down by, say, a factor of 2? What are the costs beyond the current regulations?

Minor Comments

Ex-2: Cite data used for Fig. A. (and all figures/tables)

Ex-5 First bullet. Studies find that there is a potentially stronger association with BC than many other PM components (some metals excluded). There is little reason not to note this here.

2-5-6: Check if "most" of the IR passes through the atmosphere and that half returns to the Earth's surface. Be very accurate here.

2-7-21/23 The absorbed energy depends on more than just these, e.g., size distribution.

2-8, Fig 2-4: Please add units to all graphs. 2-11-5 BC is a microscale-to-regional pollutant (I would actually say it is more local than regional).

2-11-28/30 I might not say GHGs do not have these indirect effects. Warming will impact cloud formation.

2-12-21 I would refrain from using "never". There are carbon black plants that emit something very close to what we might call pure BC.

2-13-7 Add VOCs to SO2 and NOx.

Fig. 2-8: The dashed box should extend further right given the information from above.

2-20-4 Does not the direct effect include IR absorption as well? What is meant by "net" direct effect?

2-35-9 Replace "the melt" with "melting"

2-40-25 "transported TO the Arctic"

2-42-13 0.5 TO 1.4 (?)

3-1: Given the discussion in the chapter, I might suggest that the relative role of BC wrt PM2.5 is better characterized as that while there is insufficient evidence to fully assess the health effects of BC relative to other constituents, there are a number of studies that suggest BC may have greater effects than other major constituents, particularly on a mass basis.

4-1: Should use "estimates of emissions" and more fully recognize the uncertainties.

4-3-27 OM includes other elements besides carbon associated with OC, e.g., oxygen and hydrogen, whether these elements are from secondary formation or not.

Section 4-3: It might be good to discuss studies that have looked at top-down evaluations of the emissions inventories in the US and how well they agree with the emissions estimates presented. They are rather supportive.

Also, in this section, one always wonders how anthropogenic biomass burning is partitioned in a figure/text/table saying "Anthropogenic" and "Biomass" as the only categories. While discussed earlier, it would be good to always be precise when used, particularly in tables and figures that get lifted and used for other purposes.

4-27-6/7: Not sure why it is of interest why to bring up the similarity with number of counties.

Fig. 4-15 Please provide cites in the figure caption.

5-2-4: Sentence repeats past information.

5-3-3 Not the best explanation of the difference between OC and EC given that a number of temperature protocols are used, and phase is ambiguous here. Be a bit more precise as to what is meant by phase.

5-3-27 Not sure that the ideal solution is to quantify BC in light absorption terms. It might work to use light absorption equivalent, but this should be understood to be different than mass of BC (or EC). I don't see what this sentence adds.

5-3, last paragraph. The comparison of BC by light absorption measurements and EC by thermal-optical methods is sensitive to source of BC/EC, and varies by location.

5-8-21 model**s**.

Fig. 5-3: Scale is messed up and be more specific than "absorption units"

5-13-12 Isn't using cleaner fuels part of fuel switching?

In Fig 5-6 there is an indication that there is an increase due to change of instrumentation, and then in 5-17-10 there is a note about a potential negative due to instrument change. Appendix A1-8-17/22 suggests there should be little change. Need to make clear and consistent.

5-21-9 date of cite.

5-24-32. Referring to 55 as the number of air quality monitoring sites in a mid-size state is a bit deceiving in that a casual reader might interpret this as referring to BC measurement locations (where the average is more like 8/state). Yes, states may have 55 monitoring stations, but part of that is because some locations only measure one or two pollutants. Even for ozone, GA only has 24 monitors. This comparison is not really needed. The real issue is if 55 provides a reasonable characterization.

Fig. 5-19. I am not so taken that this figure really shows what the authors are trying to show. First, the very different times when the levels rise should not be so readily explained away. If the difference is due to local sources, that minimizes the ability to use this figure for other purposes. The Lake Michigan levels do not show the strong decrease, (yet), either. Should explain how the conversion was done and what are the actual units being plotted.

Fig. 5-21 Define LAA in the figure caption

6-13-20/21. The is a key point, and is expressed much better here than either in the ES or the beginning of the chapter. The point should be made early on and emphatically that the public health benefits suggest that controls are very cost effective even without considering climate.

6-16-12 An uncertainty of only 22% & 55%? This seems rather low since the values used are not even species specific.

6-17-21. I would not refer to the PM NAAQS as a program. I might rephrase this to say something like "… a number of programs and rule-makings designed to meet the PM2.5 NAAQS."

Table 6-2: Define "RICE"

Table 6-3: The footnote refers to carbon referring to carbonaceous particles. The table does not refer to carbon or carbonaceous particles (as is done in the text). The table should also state that the controls are on carbonaceous particles, not PM in general. Further, the way the table is written, it seems that the values are form Pope et al., and Laden et al., not that the CRF's are. Spell this out. This is also not well described in the associated paragraph.

6-19-18. You might actually add to this "… and could lead to further radiative forcing."

6-19-21 BC is really a rather microscale (household) to local pollutant, with concentrations dropping off significantly as you move away from the source. I might rephrase how

you characterize BC. Many of the benefits noted, particularly in developing countries, come from reducing source emissions very near to the exposure location

8-5-34 The claim that the hybrid control can reduce PM emissions by 99.99% might be technically true, but this is not the effectiveness of reducing BC. As mentioned, the small size of BC limits the effectiveness of some control devices (as do other properties of BC). Throughout this chapter, be specific to the effectiveness to reduce BC and do not put out numbers like 99.99% unless they are relevant to BC.

11-4, Table 11-1 Explain "energy" as the climate impact in a Table footnote.

11-12-3/26 (and beyond). I would not call OC/BC (or, OC/EC) a climate metric. As the section amply points out, at best it is a messy one to use in that realm, and how it relates to climate is fraught with uncertainty. I was surprised to see it mentioned as a climate metric for BC.

12-2-21 The relative certainty is not due to extensive literature, alone, as that could have a mixed message. It is due to the preponderance/strength of studies showing the health and welfare impacts.

12-2-29 Careful targeting of mitigation programs is NOT essential for both public health and climate purposes, as even untargeted programs can provide benefits in both domains. Targeting is required to optimize the benefits and reap the potential gains in both areas. It is also the basis for identifying the most cost effective approaches.

12-3-11 "VIRTUALLY all sources..."

12-3-13 It is 75% of the PM from diesel emissions, not total emissions. It is a tiny fraction of all tailpipe emissions. Further, there are non tailpipe emissions as well.

12-3-15 Given the discussions, I would think targeting emissions that are transported to the Arctic, Greenland or Himalayas would have a greater likelihood of large climate benefit.