EPA-COUNCIL-11-xxx

The Honorable Lisa P. Jackson
Administrator
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, N.W.
Washington, D.C. 20460

Subject: Review of the Draft Report to Congress on Black Carbon

Dear Administrator Jackson:

[To Be Developed]

Enclosure

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

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1. EXECUTIVE SUMMARY

Possible Key Points:

- The Report is comprehensive and well-written, and summarizes much of the relevant scientific literature on the nature of black carbon (BC) particles; their formation, transformation and transport in the atmosphere; associated climate and health impacts; and possible mitigation technologies.
- Given the complexity of the topic, the Panel recommends that a glossary of terms be included early in the document, as a complement to the Report's successful use of text boxes (and figures?).
- Some additional references are suggested to expand the discussion of emissions from shipping, and impacts of BC on the Arctic and other sensitive regions…
- The discussion of BC climate impacts should focus more on measures of climate response, rather than on changes in radiative forcing, so that a broader set of impacts are considered and presented in terms that are meaningful to the generalist reader.
- In the discussion comparing BC to long-lived greenhouse gases (GHG) such as carbon dioxide, the Report should be clear that the analysis does not consider complex interactions between BC and GHGs with ecosystems…
- The Report should expand the discussion of health effects associated with BC, drawing upon the PM, traffic emissions and other relevant literature, and highlight the considerable health benefits that would derive from reductions in BC emissions. This health co-benefit may exceed climate-mediated benefits.
- The Report should have an additional chapter to present a more rigorous treatment of benefits and costs, and associated uncertainties, of BC mitigation to inform policy.
- Although the Report discusses uncertainties associated with emissions estimates and associated changes in radiative forcing, it fails to communicate what the total weight of evidence suggests concerning the uncertainties associated with BC. In other words, after considering uncertainties and remaining knowledge gaps, do the data suggest that the U.S. should do more, less, or the same to control BC emissions?
- Based on available data, we suggest an affirmative statement that BC appears to warm climate and should be controlled on both health and climate grounds.

2. INTRODUCTION

2.1. Background

Black carbon (BC), light-absorbing particles that result from incomplete combustion of organic materials such as petroleum fuels or biomass, has been implicated in climate change and impacts on human health. In 2009, Congress direct the EPA, in consultation with other federal agencies, to summarize the available science on the impacts of BC on climate, sources of BC emissions, benefits to climate and human health from reductions in BC emissions, and the cost-effectiveness of available mitigation strategies. The EPA requested the Council to review the draft *Report to Congress on Black Carbon* with respect to its accuracy and clarity in summarizing the available scientific literature, including uncertainties. The Council, augmented with additional experts on BC (hereafter referred to as the Black Carbon Review Panel), met on April 18-19, 2011, to hear public comments and technical briefings from Agency staff and to deliberate on responses to the EPA charge questions. A follow-up public teleconference of the Panel was held on June 27, 2011, to discuss the Council's draft report…

2.2. Charge to the Panel

The Charge to the Panel includes questions on the overall completeness and clarity of the draft *Report to Congress on Black Carbon* (the Report), including the preliminary conclusions and key messages to Congress on the state of the science on black carbon impacts and mitigation options. In addition, questions are posed on technical aspects of each of the chapters, including effects of BC on climate, public health, and the environment; BC emissions inventories; observational data; and available mitigation options (and associated control costs and benefits) for the U.S. and global emissions. Charge questions are included at the beginning of each section of the Council's report, and the full charge is included as Appendix A.

3. GENERAL COMMENTS

Charge Question 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?

Accuracy and Clarity

[Add overview discussion]

Communicating Uncertainties

The Report mentions uncertainty is a number of sections, yet could do more to present an overall sense of the uncertainty in conclusions about the net climate effects of BC, given both warming and cooling effects of BC and co-emitted particles, and the desirability of particular policy responses to BC. Sources of uncertainty include model uncertainty (i.e., the extent to which atmospheric processes are accurately included), measurement uncertainty associated with different methods and the need to translate optical reading into BC mass, uncertainty in health effects of BC as a component of the PM mixture, and uncertainties associated with mitigation cost estimates and benefit valuation. One approach to presenting an overall sense of the uncertainties associated with BC would be to include a table with qualitative discussion of the various uncertainties, similar to tables included in the Agency's recent analysis of the benefits and costs of the Clean Air Act (2nd Prospective Study reference.)

As an example, the uncertainty of BC's radiative forcing is difficult to fully understand from the Report. It is really discussing two types of uncertainty: (1) the uncertainty in the magnitude of the cooling indirect effect, and (2) the degree to which the indirect effect offsets the warming effect from combined impact of the direct effect and snow/albedo effect. The first type of uncertainty is quite large, but the second type much less. The Panel recommends that EPA review the document, especially the Executive Summary, for this issue and be explicit about which type of uncertainty is being concerned. Figure 2-8 (Estimates of radiative forcing from black carbon emissions only) is a nice way of demonstrating the uncertainty, especially relating the two types of uncertainty mentioned above. This figure might work well in the executive summary. Figure C in the Executive Summary may be omitted.

Charge Question 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?

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

In response to Charge Question 2, the Panel has recommended additional published studies that might be added to the Report, and these references are included in the relevant sections of the Panel's report. In addition, the Panel recommends that additional detail from Appendix A be brought forward into the body of the report, and this is recommendation is discussed in the sections that follow.

4. RESPONSE TO SPECIFIC CHARGE QUESTIONS

4.1. <u>Effects on Climate</u>

4.1.1. Types of Carbonaceous Particles

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

[Leads: Mauzerall, Watson]

The Report defines black carbon (BC) as "the carbonaceous component of PM that absorbs all wavelengths of solar radiation, … commonly referred to as 'soot'" (pp. 2-1 and 2-5). It might be more accurate to state that "soot"—the mixture consisting mostly of organic carbon (OC) and BC resulting from incomplete combustion—is the major light-absorbing component of air pollution emissions, and that the efficiency of this absorption varies with the composition, size, and morphology of the particles. A BC or elemental carbon (EC) measurement is the best indicator of soot, as it is directly emitted from incomplete combustion, whereas OC can derive from several sources (e.g., pollens, spores, condensed vapors, secondary aerosol). It might be worthwhile to note that EPA uses indicators where there is some ambiguity concerning precise quantification of the pollutant causing adverse effects. Neil Frank put this well in his comments on the 1995 critical review (Watson et al. 1995): "EPA uses a surrogate measure, referred to as an indicator, to represent the agents of concern. In the case of PM, this indicator is PM mass concentration in a specified size fraction. In order to treat the regulated community fairly and to provide a uniform level of health protection across the nation, the indicator must be consistently defined in terms of stable, reproducible measurements." PM_{10} and $PM_{2.5}$ are indicators defined by the measurement method, for example. In the U.S., EC is also an indicator defined by the method (Chow et al. 1993; 2007; 2011) applied in EPA's urban (CSN) and non-urban (IMPROVE) air quality monitoring networks.

Given the variations among studies in what was measured, and the number of terms in use for different categories of particulate materials, an up-front glossary of definitions would be very helpful to generalist readers. Appendix 1, which describes the various forms and measurements of BC, EC, etc., is extremely helpful, and a brief version of this appendix could be added to the main text. Figure A1-1 is particularly useful for conveying the properties of the different carbonaceous particles, and the figure should be included in the body of the report.

Further, the Report should clarify that, of necessity, the analyses draw upon studies that use differing definitions of BC. Text boxes could be used to highlight critical information such as other names for black carbon (e.g., page 2-6), listing the proxy measures for BC (e.g., $PM_{2.5}$), and other pollutants emitted with BC (e.g. page 2-12 lines 21-23) or description of brown carbon (page 2-7, lines 12-13). Suggested wording for a text box that provides a context for the use of the term black carbon is:

> We define black carbon (BC) as carbonaceous material that absorbs light at all wavelengths. However, we use historical data labeled variously as elemental carbon (EC), carbon black, soot, light absorbing PM, and black smoke as surrogates of BC for our review in this report. Although these terms are not strictly equivalent, we believe the validity of our analyses and conclusions are not materially compromised by our adopting this convention.

Comment [s1]: But, does this definition match what is requested in the previous paragraph?

In the Report, the physical characterization of the BC particulate matter is relatively limited, and for the most part BC is described simply as a component of PM2.5, with both BC and PM2.5 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 (PM10-2.5 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). A color photo (Figure 1, 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 Report should clarify that BrC may exist within the same particles as BC in soot or may exist in separate particles, and often both. Near the beginning of Chapter 2, there should be an emphasis on the mixing state of BC that can enhance its absorbing properties. Somewhere in this section, or in Appendix 1, a reference to the excellent Moosmüller et al. (2009) review of light absorption concepts and measurements should be added.

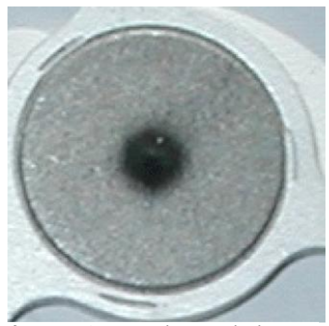


Figure 1. Coarse urban particulate matter with a black surface coating (Source: NESCAUM)

The Report's discussion of brown carbon (BrC) includes a somewhat misleading, idealized depiction of light absorption by BC and (BrC) as a function of wavelength (Figure 2-4); this figure appears to be based on the humic acid absorption plots of Andreae and Gelencser (2006) and Sun et al. (2007), or solvent extracts of emission samples (Chen and Bond 2010). The figure implies that brown carbon from biomass burning does not contain or absorb light like BC. However, samples taken by Chen et al. (2010) (Figures 2 and 3, below) show that even the brown carbon can absorb strongly at longer wavelengths. Of course, it is only possible to obtain a flaming or smoldering sample in laboratory tests, as real-world ambient samples are always mixtures of emissions from the smoldering and flaming phases.

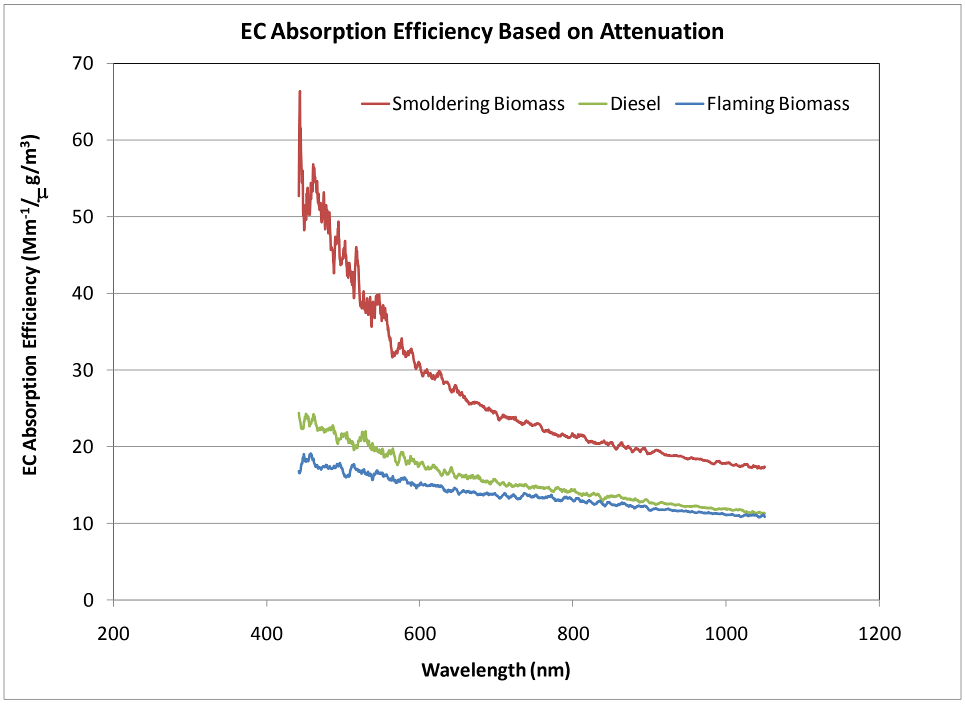


Figure 2. Absorption efficiencies as a function of wavelength for biomass smoldering, biomass flaming, and diesel exhaust (data from Chen et al. 2010). EC was measured by the IMPROVE thermal/optical reflectance method and light transmission was measured with an Oceans Optic spectrophotometer. Biomass burned consisted of moist (smoldering) and dry squaw carpet stems. Diesel exhaust was generated with an Onan Cummins diesel generator operating at 32% of full capacity (Chow et al. 2006). Filter transmittance attenuation is correlated with, but not the same as, particle absorption in the atmosphere owing to scattering within the filter and changes in particle shape after collection on the filter (Chow et al. 2010).

Figure 3. Pictures of Teflon filter samples of biomass smoldering, biomass flaming, and diesel exhaust emissions, as described in Figure 2.

Chapter 2 also notes that BC generally is expressed in mass units. However, the Panel questions the practice of converting a light absorption (Mm^{-1}) measurement to BC ($\mu g/m^3$), then converting it back to absorption for radiation balance determination. It is well demonstrated in this Report and elsewhere that the mass extinction efficiency varies by particle shape, size, composition, and wavelength. There should be a stronger recommendation for reporting light absorption in the original units of absorption, or at least being more specific about the wavelengths and mass absorption efficiencies used (usually the default values programmed into an instrument by the manufacturer). Additional discussion of this issue is provided in the response to Charge Question 10 (section 4.4 below).

The Panel has the following additional comments:

- A table showing the radiative forcing (RF) of BC as a ratio of the mass of BC would be a helpful addition to the chapter. 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.
- The suggestion (p. 2-8, line 12) that carbonaceous particles might be considered along a continuum from light-absorbing to light-scattering is quite important and should be included in the conclusions or overview as well.
- The discussion of physical transformations within emission plumes (p. 2-14) would benefit from some quantitative estimates of the change in light absorption associated with these transformations. The extent to which these transformations are included likely explains the discrepancy between results from different models and thus it is useful to look at the range that exists currently.
- 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. 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.

4.1.2. Comparing BC to Long-Lived Greenhouse Gases

Charge Question 4: Does the draft Report adequately explain and appropriately characterize the differences between BC and long-lived greenhouse gases such as CO_2?

[Leads: Fernandez, Hansen]

To a large extent, the Report adequately describes the differences between BC and long-lived greenhouse gases (GHG), including atmospheric lifetime differences, the differences in direct and indirect radiative processes (including snow and ice albedo effects), the vertical and horizontal distribution differences in the atmosphere, and the much more complicated physical characteristics and atmospheric behavior of BC/BrC/PM relative to long-lived GHGs.

The use of text boxes such as 2-1 on page 2-9 and tables such as 2-1 or 2-2 are very helpful in summarizing information. Figure 2-6 and Table 2-2 are particularly clever and informative in their depictions of particles and their evolution over time. The description of uncertainty on page

2-19, lines 10-19, is very illustrative. Perhaps more of these types of text boxes and figures could be developed, particularly in other parts of the report where uncertainties are described within the text.

The Panel suggests clarifications and improvements in the following areas:

Model Uncertainty

A central issue has to do with models as tools to assess impacts; tools to help us understand BC's effects. The Report is silent on the reliability of simulations from the existing models, in that it doesn't say whether these tools are sufficiently sophisticated to capture the complexities of the climate system. The reader needs EPA's key message as to how useful and reliable the model results are.

Climate Response

Although radiative forcing is a useful metric, the climate response—which involves interactions between all the components of the biogeophysical system (atmosphere, hydrosphere, lithosphere, biosphere, cryosphere)—is more relevant to BC effects on climate. So while there is merit in presenting the differing contributions of BC and long-lived GHGs to radiative forcing, it would be more enlightening and realistic to discuss the climate response modeling results of Jacobson and coworkers, which take into account a larger suite of BC direct, semi-direct, and indirect effects than has been done in the various modeling exercises considered by the IPCC and referred to in the EPA report. (Additional effects that should be considered are discussed in response to Charge Question 3, in section 4.1.3 below).

Time Scale

An important consideration when comparing the effects of BC and long-lived GHGs is the time scale over which effects are assessed. The Report provides a clear explanation of the different radiative forcings over multiple time scales from short-lived particles vs. long-lived gases. A figure showing the RF and dT response to pulses, as well as sustained constant emissions of BC and CO2, would further illustrate the differences in temporal behavior of these components. (Note that the estimated atmospheric lifetime of CO2 is constrained by available data to between 30 and 95 years, not centuries.)

However, there is less 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 gases 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. A more thorough discussion should be given of these implications and desirability of placing a higher priority on the control of a short-term climate forcer such as BC than on long-lived GHGs alone. Meaningful reductions in short-term climate forcers could have profound effects on opportunities for society to implement climate change adaptation as well as transition to low carbon economies. It is essential, however, that the real potential for BC mitigation would be meaningful for the climate system to make that case, which the report does not directly address.

Another perspective on the nonlinearity of shorter-term climate responses should be mentioned, which is that of "tipping points" (see Kriegler et al. 2009, and "Tipping points in the Earth system" by Timothy M. Lenton, at http://researchpages.net/esmg/people/tim-lenton/tipping-points). In this context, our lack of understanding for triggers of "abrupt climate change" (Alley et al. 2003) and the significance of its consequences warrants a discussion of non-linearity in the climate system and the potential role of BC on shorter timeframes.

Differences in the temporal behavior of BC relative to long-lived GHG are important when selecting metrics to evaluate various policy goals and mitigation strategies; this topic is discussed further in Section 4.7 in response to Charge Question 14. (or move that discussion here?)

Ecosystem feedbacks

The Report should clearly state that the comparison of BC and GHG does not consider the complex linkage between the climate system and ecosystems, and the differential role of BC and CO_2. Both BC and GHGs, particularly CO_2, can alter ecosystems and thereby potentially influence feedbacks between ecosystems and the atmosphere. For example, CO_2 can enhance plant growth through the well-known CO_2 fertilization effect, capturing C through photosynthesis, and increasing water use efficiency of plants through reduced stomatal conductance (IPCC 2007). Both of these effects influence feedbacks for CO_2 and water vapor to the climate system. These physiological effects on plants from CO_2 are largely absent for BC. Terrestrial and marine mechanisms for CO_2 uptake are not infinite, however, which raises concerns for an increasing airborne fraction of CO_2 over time with a decreasing land and ocean CO_2 sink (Canadell et al. 2007), mechanisms not relevant to BC. On the other hand, soot deposition to plant foliar surfaces can inhibit photosynthesis (Kozlowski and Keller 1966), while BC deposition to soils could enhance soil productivity (Laird 2008; Lehman et al. 2006). Climate change can increase wildfire frequency due to increased risks of drought and lightning (Amiro et al. 2009; Liu et al. 2010), resulting in feedbacks of PM and GHGs to the atmosphere.

4.1.3. Climate Effects

> *Charge Question 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)?*

In general, Chapter 2 is a well-written, comprehensive description of the various mechanisms through which BC affects climate. However, the draft Report is missing a discussion of some processes that affect climate and could delineate some terminology related to different processes affecting clouds more clearly. Specifically, the Panel recommends clarification in the following areas:

Climate response

The Report should distinguish better between radiative forcing and climate response and emphasize that radiative forcing terms are not linearly additive so do not necessarily give the full climate effect of a substance. Climate response calculations account for nonlinear interactions and give a better overall assessment of the effects of a pollutant. Climate response calculations

capture feedbacks and effects not captured by radiative forcing. An example is the effect of warming on water evaporation and the additional warming that results from the increase in water vapor.

The Report states in one place that GHGs are by far the largest contributor to global warming. This should be modulated to just "GHGs are the largest contributor." Similarly, the text states in a table (2-1) that BC is the 3rd leading cause of warming although many studies suggest it is the 2nd leading cause. We would suggest stating "either the 2nd or 3rd leading cause."
As noted previously, several additional effects should be discussed and clarified in the report:

- 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).

The first and second aerosol indirect effects differ from the semi-direct and other cloud effects. A distinction between these various effects should be included. The cloud absorption effect and semi-direct effects act to warm and evaporate clouds, increasing surface warming whereas aerosol indirect effects tend to thicken clouds, reducing surface radiation, causing cooling. Satellite data suggest that aerosol particles tend to increase cloud thickness with increasing aerosol optical depth at low aerosol optical depth but decrease thickness at mid and higher aerosol optical depths. This curve is referred to as a boomerang curve (reference?). Note also that the glaciation indirect effect is introduced (on page 2-12, line 13) but is not really described within the section.

Figure 2.8 (page 2-18) should be re-evaluated. Does the cloud lifetime and albedo effect (aerosol first and second indirect effects) exclude other aerosols but BC? If so is the sign possibly correct? The caveats for the indirect effects are stated in the legend but the title of the figure (Estimates….Black Carbon Emissions Only) could be misleading.

Uncertainty

The Report should provide a consistent sense of the scientific uncertainty for the indirect effects and overall impact of BC on cooling versus warming. Some statements note 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). Later in the section (p. 2-24, line 12), the Report states that "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 do nothing to mitigate BC effects on climate because not enough is known. The Panel does not think this is the correct inference to draw based on the preponderance of evidence so

far. Based on available data, we suggest an affirmative statement that BC appears to warm climate and should be controlled on both health and climate grounds.

Arctic impacts

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. Other than saying that emissions impacting the Arctic come from uncontrolled burning of biomass in Northern countries (the Report should list these) as well as agriculture burning, where else are those emissions – particularly those that lead to deposition on the ice – coming from? Does it change by season? What is the role of shipping? The Report (on page 2-40, line 32) attributes 50% of sea-ice retreat to BC, but that seems high; did the studies really attribute retreat as due to BC alone?

Radiative Forcing

Early in the Report, there should be a discussion of how the mixing state of BC can enhance its absorbing properties. Some of the differences in results from different models as to the forcing from BC could be attributed to the way models treat optical and physical properties of BC and also the amount of BC that is present. It would be helpful to include a table showing forcing and associated physical and optical properties treated in each of the models to shed light on the different forcing results obtained from the models.

A table showing the radiative forcing of BC as a ratio of the mass of BC also would be useful. Comparing forcing/g (W/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 (e.g., as shown in Fig. 2-9, p 2-21). Other references that could be cited include Kopp and Mauzerall (2010), which attempts to reconcile forcings from different studies in uniform units; their presentation of effective radiative forcings for carbonaceous aerosols from combustion and biomass sources from four studies may be a useful addition to the Report.

Other suggestions include:

- Page 2-33. Lines 7 to 15. A useful addition is to also account for how much change is present between PD and PI BC amount when characterizing forcing ranges between different studies.
- Page 2-45, Table 2.6: It would be useful to include a regional distribution of radiative forcing effects.
- The discussion of seasonality (the temporal aspects of emissions and effects) could be expanded, as well as the importance of the short life-span of BC's effects.

Economic valuation

The Panel recommends that that Section 2.7 (Economic Value of BC Impacts on Climate) be deleted, and that valuation of the BC impacts be consolidated and treated more comprehensively

in a separate chapter. (Recommendations on the treatment of economic benefits and costs of BC mitigation are discussed in section 4.6).

4.2. <u>Effects on Public Health and Environment</u>

4.2.1. Public Health Effects

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

[Leads: Bell, Levy]

Chapter 3 of the Report provides a nice overview of the health impacts of particles, but is brief and lacking in detail and sophistication compared to other parts of the report. The Panel recommends that this chapter be expanded, and that health be given more of a focus throughout the document. In particular, the executive summary should include a stronger focus on the health benefits of lowering BC emissions, and should mention health earlier in the text. Currently, the executive summary's mention of health (page Ex-5) is under-stated, and the imbalance and sequencing reduce the impact that the document could otherwise have. Below are some specific suggestions:

Uncertainty

While there is uncertainty regarding the health impacts of BC, the Report should highlight that this uncertainty relates to the differential health impacts of any individual PM component or sources relative to PM as a whole, rather than from a lack of knowledge about BC specifically. As written, the Report may give the impression that there is disproportionately more uncertainty about BC than other particulate matter components. In fact, there are many studies that relate BC to health, including studies that examine sources of BC rather than BC itself. Relevant studies include land-use regression modeling to estimate traffic exposure (especially in countries where diesel vehicles are prevalent), proximity studies that estimate exposure based on distance from major roadways, and indicator components (which may or may not be BC) for general traffic or diesel sources. There are numerous studies available, and this report does not need to be a comprehensive list, so we will leave the choice of studies to EPA. Some studies that could be added are those that provide evidence for respiratory-related illness (see Ostro 2009, Beelen 2008, Maynard 2007, Clark 2010, Morgenstern 2008).

Comment [S2]: Get these from Levy

In addition, the text describing the uncertainty around source apportionment may give the impression that such results are not meaningful. However, despite the uncertainties, multiple source apportionment methods have shown that similar results are achieved (e.g., see Thurston et al. 2005). We also recommend adding more source apportionment references in general, as the Report ultimately emphasizes source-specific control strategies that would capture the mixture of pollutants from these sources.

Expand the Discussion of BC Health Effects

While it is reasonable to rely on the most recent Integrated Science Assessment (ISA) for particulate matter (U.S. EPA 2009) as the foundation of Chapter 3, given its relevance and the amount of effort that went into its compilation, this approach leads to some holes in the chapter. For example, the ISA includes studies only within defined dates and primarily focuses on studies relevant to the regulation of total $PM_{2.5}$ mass. There is also significantly more detail in the ISA than in Chapter 3, some of which could be leveraged to give a richer portrayal of the evidence. We do not recommend an extremely lengthy chapter, but targeted expansion to inform the readership about a few key points.

- Many epidemiological studies are not focused on BC, but use EC, or a source (e.g., traffic). In fact, even in single pollutant studies, the BC may be considered an indicator pollutant for all traffic-related emissions. This concept should be discussed in the text. The evidence linking BC to health effects is not just from those that studied BC directly, but from a broader set of studies that include traffic, etc.
- Only a handful of chemical components contribute to ~80% of PM2.5 total mass, and one of these is BC; for example, Bell et al. (2007) examined level of PM components on a national basis, and identified EC as one of the 7 main contributors. While this large mass from BC does not preclude the possibility that other smaller contributors are harmful for human health, the large contribution of BC to PM2.5 will help put the studies of PM2.5 specifically into context.
- The document repeatedly notes that BC is often emitted with other pollutants, such as other types of particles. However, the reader may infer that there are BC particles and other particles emitted at the same time, and that these are entirely separate entities. In reality, BC is part of a complex mixture within a single particle. This should be highlighted.
- The point about spatial variation of BC is an important point that is made in the report and in the executive summary. Figure A provides an example of the U.S. versus global emissions, which might imply less variation with the U.S. Including the figure is fine, but interpretation would be aided by mention of the within-U.S. variability. This would help provide more specific guidance about optimal locations for intervention strategies, and could also be discussed in Chapter 3 in relation to possible exposure misclassification and underestimation of health effects of BC relative to some secondarily-formed particle constituents.

Comment [j3]: I didn't keep editing a lot of these bullets, but there is a bit of redundancy, and some of this could be resequenced to keep key concepts together (related to the health evidence first, and other components later, for example).

Comment [j4]: I wasn't sure what exactly this statement was referring to

Co-Benefits

The concept of co-benefits could be greatly expanded in this section, and also throughout the report (e.g., Chapter 6). There are several studies relevant to this topic that could be added to the report: Li et al. (2011), Huang et al. (2011), Ganten et al. (2010), Bell et al. (2008). The Panel also recommends that this concept be discussed further in the Executive Summary, which could be re-oriented to emphasize that there would be "no regrets" strategies available to mitigate climate change if (as seems likely) the public health benefits of BC control strategies outweigh the costs. EPA's most recent assessment of the Benefits and Costs of the Clean Air Act (U.S. EPA 2011) could be referenced with specific numerical examples of the relative impact of

particles on human health. This is an excellent way to use EPA's existing studies to provide quantitative evidence of the health, and related economic, benefits of reducing particulate matter levels. Chapter 6 provides the $/ton health benefit estimates from one study, but other quantitative insight is available from previous work and should be discussed. In particular, the concentration-response functions for mortality, which are the foundation of the $/ton health benefit calculations and related conclusions, are never discussed explicitly or presented in Chapter 3 or elsewhere.

Economic valuation

The economic valuation section of Chapter 3 provides a general overview, but not specifics. The Panel recommends that the discussion of health benefit valuation be included in a new consolidated chapter on benefits and costs, and that the discussion of health benefits be expanded either to provide some results or explicit reference to other sources. If expanded, it should provide a very careful definition of VSL for the uninitiated reader, and talk about the values typically used in regulatory analyses. (Recommendations on the treatment of economic benefits and costs of BC mitigation are discussed in section 4.6).

4.2.2. Non-Climate Environmental Effects

Charge Question 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?

[Leads: Brandt, Poirot]

The report provides a very brief (2-page) summary of the effects of "$PM_{2.5}$, including BC" on ecosystems, on damage and soiling of building materials, and on visibility. As with the preceding discussion of health effects, the implication that welfare effects are only associated with BC as a fractional contributor to $PM_{2.5}$ mass seems like an unnecessary generalization. While more detailed and species-specific assessments of BC effects could be developed here, it is likely that more refined estimates of BC welfare effects will remain relatively small compared to those on human health and climate.

Effects of BC on visibility have been relatively well characterized in the literature, and could be described separately from those of other $PM_{2.5}$ constituents. The report does discuss visibility effects of "carbonaceous aerosols" (BC and OC), but could provide more detail specific to BC. For example, at relative humidity below about 85%, BC contributes to light extinction more efficiently per unit mass than any other $PM_{2.5}$ species. Thus, under most conditions, and especially in populated urban areas, BC's contribution to visibility impairment is typically greater than its proportionate contribution to $PM_{2.5}$ mass. BC's extinction efficiency can also be enhanced in internally mixed aerosols combining BC with non-absorbing species like sulfates or organics. In addition to effects on light extinction, BC (and BrC) also can cause or substantially contribute to atmospheric discoloration effects (i.e., layered haze, Denver Brown Cloud, etc.), which people often find especially objectionable (reference?). Thus, the aesthetic effects of BC and BrC on visibility impairment are greater than their contributions to light extinction alone.

The Report's approach of collectively summarizing effects of "$PM_{2.5}$, including BC" also is not well suited for reviewing effects on ecosystems and crops. For example, the ecological effects of PM discussed in the second paragraph of section 3.4 are due to PM components (metals or toxic organic compounds) other than BC. Conversely, the BC (and BrC) contributions to (surface dimming-related) reductions in crop yields (discussed in the 3rd paragraph of section 3.4), or on the productivity of forest ecosystems, are likely to be of a distinctly different nature than those resulting from most other (light scattering) aerosol components. Both absorbing and scattering aerosols reduce the amount of direct photosynthetically active radiation (PAR) reaching leaf surfaces, but the increase in indirect diffuse radiation resulting from scattering aerosols can lead to increases in photosynthesis and net primary productivity under some conditions. In contrast, light absorbing aerosols decrease direct radiation but do not contribute to these increases in diffuse PAR. For additional detail on this topic, see for example: Cohan et al. (2002), Yamasoe et al. (2005), Greenwald (2006), Oliveira et al. (2007), Matsui et al. (2008), Betts and Silva Dias (2010).

A more minor point is that these non-climate effects need to be carried forward when discussing benefits. The treatment of the economics of these impacts is incomplete, in part because the literature on the impacts is not translated into endpoints that are economically meaningful. For example, there is no description of the practical significance of the measured changes described in the literature. Are the changes in crop productivity large enough to have price effects? How these effects are measured (the metrics for impacts) would determine what valuation literature is applicable. In other words, a measured environmental effect is not equivalent to a welfare effect. The panel suggests that rather than adding details of the valuation methods to chapter 3, the current section on valuation (section 3.5) be moved into a new chapter on valuation of these and other endpoints. The new chapter should include a more detailed and rigorous description of methods to value the change in these endpoints (see section 4.6, below, in response to questions 12 and 13).

4.3. <u>National and Global Black Carbon Emissions</u>

4.3.1 Past and Present Emissions

> *Charge Question 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?*

[Leads: Corbett, Fuglestvedt]—add general intro sentences

Source Characterization

The Panel recommends that the authors clarify several aspects of the discussion of source characterization across combustion sources, national domains, and source categories.

First, the Report refers to "domestic sources" but this term is not clearly defined. For example, do domestic sources include international sources operating in U.S. territories? Similarly, there are U.S. sources operating outside U.S. domains; this has special relevance for the important

discussions related to the Arctic, and to shipping in general, with other locations and sources possibly included. In particular, the category "commercial marine" needs clarification. The number of marine diesel engines is small, although the installed power can be very large. These engines can be operating in the U.S. but be international; operating internationally under U.S. registry (flag); or be both U.S. registered and operated within U.S. waters (harbor, inland river, coastal, coastwise, and Lakes).

Second, the term "contained combustion" is used in Chapter 4 but only poorly defined in Chapter 8, Section 8.3, for the first time. While used by some scientists in papers and reports, this is not a common term for engineering disciplines. Engineering and scientific combustion types may jointly include: (a) open burning (biomass); (b) open combustion (inclusive of steam boilers, some gas turbines); and (c) closed combustion (internal combustion, reciprocating diesel engines). Some discussion of this "taxonomy" – along with the glossary suggested by the Panel - and in-text definitions of the adopted terms would make Chapter 4 clearer to readers across disciplines.

Third, the characterization is really aimed at diesel engines using distillate petroleum fuels. This is not strictly correct, and it affects the technologies discussion later in the report; in fact, technologies that do NOT require distillate petroleum (or more importantly, U.S. ULSD) may be appropriate to consider – both for larger diesel engines in mobile and stationary service, and in addressing global (non-U.S.) diesel systems.

Figure 4-1 is a very important graphic requiring integration with characterizations of sources later (see discussion under uncertainty).

Geographic Characterization

The draft Report focuses primarily on nationalities of sources, poorly representing sensitive regions like the Arctic. Sensitive regions can be discussed from at least two perspectives: a) sensitive regions for climate response; and b) sensitive regions for health risk exposure and impact. These two perspectives should be presented in parallel throughout the Report since the relevant geographic areas may differ. It becomes important when chapters on "metrics" are really climate response metrics, and when chapters on social cost metrics are often (not always) health risk valuations of mortality and morbidity.

The Arctic is a sensitive response region primarily with respect to climate change, although there are communities impacted by air pollution (at least globally) in and around the Arctic front (above 40 N latitude). This can be introduced in Chapter 4 discussion with greater clarity linking to later chapters.

Despite the emphasis elsewhere that the Arctic is an important sensitive region of interest to EPA, it is unclear how BC and OC emissions in the Arctic region are characterized or allocated among the national domains reported in Tables 4-4 and 4-5 of the Report. As noted above, the information used to characterize international and geographically important emissions is unclear and incomplete. For example, shipping is stated to be included in sources for Tables 4-4 and 4-5,

but may not be allocated in domains, given that activity occurs outside national boundaries (see above section discussing domestic versus international).

Arctic importance can be better presented through additional citations to Arctic work (e.g., the Arctic Council's 2009 Arctic Marine Shipping Assessment). The Report also should reference recent regional inventories for Arctic Shipping (Corbett et al. 2010a), and for global shipping with special regional attention on Arctic emissions (Paxian et al. 2010). It is not until Section 4.4.2 that the Report discusses which areas might be important contributors to Arctic BC problems.

The discussion of emissions above 40 degrees North latitude mainly discusses U.S. emissions above this latitude without sufficient context for other nations' emissions above 40 N. For example, Table 4.7 could be expanded to include relative contributions of BC from all countries and sources north of the 40th parallel. Similarly, Figure 5-2 which could be improved with shading below the dotted line to focus and connect discussion of the region ABOVE 40 N latitude – and the figures and tables could include shipping.

Scientific understanding of geographic effects on emissions characteristics (and perhaps on modeling of impacts, on uncertainty, etc.) is not well articulated. This includes seasonality patterns (e.g., temperature, activity, etc.).

Global comparisons seem arbitrary. The purpose of similarity analysis by nation should be clarified, with attention to the readership that will include other nations and global scientists or policy makers outside of EPA and beyond Congress.

Abatement Technologies

Diesel engines use many fuels, but the Report implies that diesel fuel is of one type – especially through its focus on onroad diesel engines. Large stationary diesels, commercial marine, and other applications using diesel engines would not fit the dominant description. The Report should avoid over-generalizing and thereby leaving the impression that U.S. onroad technologies (and ultra-low sulfur diesel, ULSD) will be preferred solutions for global BC mitigation. In fact, emissions from these other internal combustion diesels using nonroad (non-distillate) fuels are not suited to mitigation using catalytic diesel particulate filters (DPF); for example, EPA lower fuel sulfur standard for marine engines specifies 1000 ppm (ECA standard), a level of sulfur that is too high to allow for the use of catalytic DPFs (e.g., see Corbett et al. 2010b).

The Report's conclusion that low-sulfur fuels are a precondition for BC mitigation is not strictly correct for all diesel sources; e.g., see written comments for examples and citations to other literature—add these. Additionally, the terms conventional diesel v. clean diesel v. other diesel should be clearly distinguished in the discussion because of differences in emissions (and mitigation options) for the different types of diesel fuels; since 2007, most new diesel engines (vehicles?) are "clean" diesel, meaning they reflect the use of advanced design, including after-treatment such as DPF which eliminates soot or BC.

Existing engine technologies (DPFs) are more applicable than EPA articulates; comments from experts with state-agency perspectives have suggested corrections. In addition, the Report's

Comment [S5]: This seems inconsistent with other statements in the report?

technology focus ignores possible systems effects, including the role of infrastructure development (especially globally) in transportation-related emissions trends. Although not specific to BC, the Report could reference U.S. DOT (2010) on this topic.

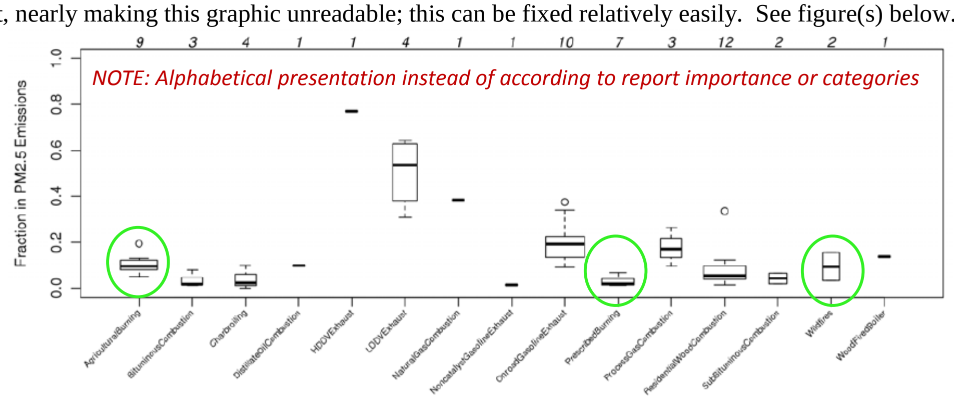
Accuracy and Uncertainties

The Report discusses uncertainty, but without providing sufficient context with respect to the overall weight of evidence – no judgment from the analysis emerges to provide context for the Congress or other readers. For example, does uncertainty in emissions source characterization have higher priority or does it contribute more uncertainty than modeled impacts, or social benefit-cost uncertainties? Without context, the report seems too uncertain for the confident conclusions about emissions source characterization – especially global comparisons.

This can be partly addressed by using language better: e.g., the report could/should use "estimates of emissions" and more fully recognize the uncertainties. The Panel believes it is important to articulate source uncertainty in comparison with other uncertainties discussed: for example, model uncertainty, BC v. BrC, mixing, metrics. Do emissions uncertainties dominate or pale by comparison? By doing so, the overall power of conclusions about mobile source dominance, biomass burning variability, etc., will be more strongly conveyed. We encourage EPA to articulate prospects or plans for reducing emissions uncertainties and for propagating these into the overall summary of insights for emissions, their impact on modeling confidence, social costs, and metrics. Propagating these uncertainties may be beyond this report, but it offers great opportunity to strengthen the insights drawn in the study.

For example, the RPO v. EPA details on biomass burning could say, "Nonetheless, biomass burning BC estimates remain more uncertain than engine combustion BC because of year-to-year variability and for other reasons addressed in this chapter."

Tables 4-4 and 4-5: uncertainties (or variability) presented absolutely affect (confound) the ratios presented comparing emissions from other countries with U.S. BC emissions. This comment applies to many bar graphs and comparisons as well, and should be carried into these discussions (at least Chapter 4). Categorization is useful in the discussion, yet the first figure does not follow it, nearly making this graphic unreadable; this can be fixed relatively easily. See figure(s) below.



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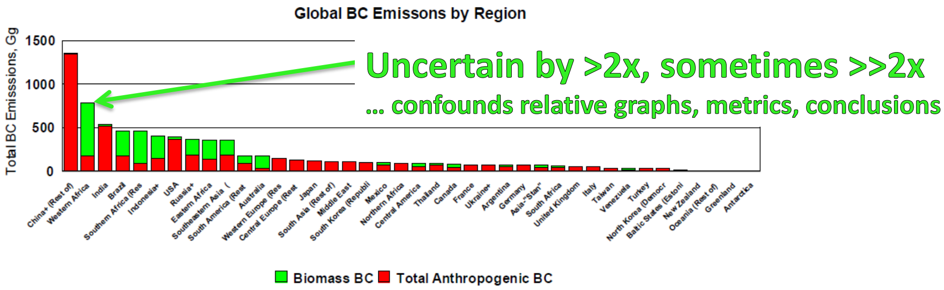
These uncertainties undermine the attempt at global comparisons, mostly by being presented with apparent over confidence. See figure below, where uncertainty in biomass burning cannot allow the U.S. ranking to be claimed to be sixth with any confidence.



4.3.2. Transport and Location of Effects

Charge Question 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?

[Lead: Jacobson, Russell]

The report addresses the issues involved in the long range transport of black carbon at an appropriate level of detail, though a couple of additions are warranted First, the Report should mention that aircraft are the only direct source of BC emissions over the Arctic. Some ships are close sources as well. Aircraft BC emissions over the Arctic persist longer than surface BC emissions since they are emitted into the stable stratosphere, primarily. They are also emitted above clouds and ice, so absorb not only downward but also reflected upward radiation. Second, the Report should point out that, during long-range transport, BC particles become more internally mixed, increasing both their warming effect due to optical focusing and their hygroscopicity (i.e., ability to retain moisture). This internal mixing increases the ability of BC particles to participate in the indirect effects, the semi-direct effect, and the cloud absorption effect. It would be appropriate to discuss the uncertainties associated with both of the above issues, as well as the relative uncertainties associated with BC transport in comparison with other issues.

The subject of aerosol transport to the Arctic and the Himalayan plateau could be bolstered to facilitate a better understanding of the implications of BC/PM effects on ice melt in these regions. This is also important since the same applies to other heavily snow-covered regions where the implications for decreases in snow cover, depth, and the timing and distribution of snowmelt have important societal and ecological consequences.

4.4. Observational Data

Charge Question 10. Does the draft report appropriately characterize and interpret the information on BC that is available from the observational record?

[Leads: Hansen, Watson]

Chapter 3 nicely describes what is known about BC from the most current observational record. The results presented are very appropriate and complete. The inclusion of satellite observational data is timely as this presents an emerging field with the 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 also introduces the practical aspects of BC measurement, including the operational definitions of BC based on the measurement methods used. The discussion is difficult because of the complexities, intricacies, and nuances, not the least of which using the same designation for what are really different measurement methods.

Some of the figures may contribute to this difficulty in that they appear to have been translated from their original sources with incomplete footnotes, captions and legends. These oversights are noted below. However, Figure A1-1 in the first appendix is a very effective graphical representation of methods and definitions. As noted previously, the Panel recommends that this figure be included in the main body of the Report to aid in the explanations concerning definitions and methods.

The references to the sources of data are very thorough and up to date, although there are some additional ones of relevance that are indicated in the specific comments below. In the discussion of sediment records, only a subset of the referenced data is presented. No characterization of the findings from any of the references cited in the second paragraph of this section (5.6.4) is given. This is understandable if the intent is to focus on data for the U.S. as well to control the size of the report, but a brief characterization of these papers might be provided.

BC Trends

The long-term BC downward trends (p. 5-1, lines 26-29) are an important finding that should be moved up the list of key messages, possibly to a first bullet. The data provide evidence that emission reduction efforts are working, including emission reductions associated with engine and fuel improvements (Bahadur et al., 2011; Kirchstetter et al., 2008; Minoura et al., 2006; Murphy et al., 2011) and reductions in residential biomass burning emissions (Burnet et al., 1988; Butler, 1988; Hough et al., 1988) to attain NAAQS (Bachmann, 2007) for PM and other pollutants. The Report should highlight for Congress that there is added benefit to what is being done already, as well as to encourage other countries (e.g., China and India) to consider climate benefits as they weigh further emission reduction measures. This should be elevated to a major report conclusion.

Averaging period

EC and BC concentration ranges (p. 5-1, lines 10-15) should apply to a consistent averaging time (e.g., 24-hours, annual average, seasonal average). Cao et al. (2007) found wintertime average EC values exceeding 15 µg/m^3 in two Chinese cities. Short duration spikes (as seen on aethalometers when a diesel truck passes) can be higher, even though a longer term average would be lower. Winter BC averages are typically higher than summer averages. The Report might note that wintertime urban averages can exceed 20 µg/m^3 and adjust Table 5-2 to match. The 5 to 10% of $PM_{2.5}$ estimate (p. 5-1, line 16) also needs some qualification with respect to location and averaging time. This range seems to be based on Figure 5-4 which is a limited picture of concentrations and may be incorrect (see specific comments). Figure 6 shows plots of major components from four urban areas. EC seems to be lower than 5% for many of the samples.

Measurement Variability

The key message regarding measurement variability (p. 5-1, lines 3-7) should be more positive and succinct, such as, "BC and EC values from different measurement methods are highly correlated, although the method-defined absolute values may differ by a factor of two or more. However, self-consistent measurements in long-term networks are sufficient to detect trends that correlate with emission reductions." This statement would be supported by the general explanations of BC, EC, BrC, and the relationships between them in Chapter 2 and a more detailed treatment of the complexities, knowns and unknowns in Appendix 1.

The chapter should emphasize the need for further research to standardize measurement methods; for example, by including a separate key message on this topic (p. 5-1, lines 7-9). The chapter also might note that $PM_{2.5}$ OC/EC measurements are currently standardized and consistent among the long-term U.S. networks of IMPROVE (IMPROVE, 2011), CSN (U.S.EPA, 2011), and SEARCH (ARA, 2011).

There are two important sources of variability in BC measurements: (1) differences among thermal measurement methods, and (2) the application of factors to convert optical readings to estimates of particle mass. Two widely cited comparison studies (Schmid et al. 2001; Currie et al. 2002;) show that inter-method differences from EC thermal analyses can easily differ by a factor of two, and Currie et al. (2002) found differences up to 7 times. This is true even for methods with the same designation, such as Thermal/Optical Transmittance (TOT) (Schmid et al. 2001). On top of this is the variability in the natural and methodological variability of the mass absorption coefficient (m^2/g) that converts optical measurements of aerosol absorption (Mm^{-1}) to BC. This efficiency varies based on the size distribution and form of the aerosol (see Figure 3 in Schuster et al. 2005), as well as on the methods used to determine light absorption, which differs by a factor of 2 for light transmission through Teflon or quartz fiber filters (Figure ? in Chow et al. 2010).

The Report should emphasize that optical devices do not measure carbon; they measure light absorption or attenuation at different wavelengths, then BC particle mass is estimated using

default mass absorption efficiencies set within each instrument. A column should be added to Table A1-2 to indicate the different wavelengths used, and to show the default mass absorption efficiencies used in each instrument to estimate BC. This is a major cause of uncertainty in estimates of ambient BC.

Despite the statement on page 5-3 (line 24), the mass absorption efficiency is an "issue of debate." It is known that there is no single factor that is applicable to all methods, wavelengths, particle sizes, particle compositions, shapes, and structures. Theoretical and empirical studies show that bounds can be placed on absorption efficiencies for different assumptions of the aerosol origins and compositions (Alfaro et al., 2004; Andreae et al., 2008; Chan et al., 2010; Chou et al., 2005; Chow et al., 2009; Dasgupta et al., 1991; Dillner et al., 2001; Favez et al., 2009; Fu and Sun, 2006; Fuller et al., 1999; Horvath, 1993; Jacobson, 1999; Jacobson, 2000; Jacobson, 2005; Jacobson, 2006; Liousse et al., 1993; McMeeking et al., 2005; Nordmann et al., 2009; Ogren et al., 2001; Ram and Sarin, 2009; Ramana et al., 2010; Rosen and Novakov, 1983; Schuster et al., 2005; Wagner et al., 2009; Watson et al., 2005; Widmann et al., 2005).

The suggestion (page 5-3, lines 27-28) that the ideal solution is to quantify BC in light absorption terms should be qualified, since light absorption is measured variously in situ by photoacoustic spectroscopy and as the difference between light extinction and light scattering and on filter media, or approximated by reflectance off filter media . All absorption approaches are subject to interferences from light absorbing substances other than BC. Even if using light absorption equivalent, it should be understood to be different than mass of BC (or EC). Unfortunately, from the perspective of practicality, the only ideal method might have to be particle-by-particle characterization of morphology, internal structure and composition, and optical properties as a function of relevant wavelengths.

Additional relevant studies

More than 100 BC/EC/BrC comparison studies have been published and these studies could be referenced in Chapter 5 of the Report. See Table 2 of Watson et al. (2005) and Table C-1 of Chow et al. (2006) for summaries of comparisons up to 2006. Additional comparisons among a variety of measurement methods have been published since then (Bae et al., 2007; Bae et al., 2009; Braun et al., 2007; Calvello et al., 2010; Chan et al., 2010; Cheng et al., 2011; Cheng et al., 2010; Chow et al., 2010; Corrigan et al., 2008; Cross et al., 2010; Flores-Cervantes et al., 2009; Fujita et al., 2007; Gan et al., 2010; Gilardoni et al., 2011; Hammes et al., 2007; Han et al., 2007; Hansen et al., 2010; Hopkins et al., 2007; Hsieh and Bugna, 2008; Kanaya et al., 2008; Knox et al., 2009; Krecl et al., 2007; Lack et al., 2008; Lee et al., 2007; Miyazaki et al., 2008; Moteki and Kondo, 2007; Moteki and Kondo, 2010; Muller et al., 2011; Niu and He, 2010; Nordmann et al., 2009; Paredes-Miranda et al., 2009; Poot et al., 2009; Quincey et al., 2009; Quincey, 2007; Reisinger et al., 2008; Schaap and van der Gon, 2007; Sedlacek and Lee, 2007; Slowik et al., 2007; Snyder and Schauer, 2007; Subramanian et al., 2010; Taha et al., 2007; Viana et al., 2007; Wallen et al., 2010; Wonaschutz et al., 2009; Zencak et al., 2007).

Presentation of PM2.5 Trends in the U.S.

The chapter includes a figure (Figure 5-4 on page 5-12) that shows PM2.5 composition for a number of urban areas, but the figure does not seem consistent with recent urban values (e.g., see Figure 6 from VIEWS 2010). The southern California sulfate values are too high in these pie charts and there is no rationale provided for the selection of the cities shown. It might be more relevant to use regional values from the IMPROVE network, as the broader spatial distributions represented by these data would be more relevant to climate. The Figure 5-4 key, 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? It appears that the yellow and red slices labeled “sulfate” and “nitrate” also include associated ammonium, and if the figure uses Neil Frank’s SANDWICH approach, it also probably includes some associated water. If true, or approximately so, the legend species names should be changed to “Sulfates”, “Nitrates” and “Organic Matter”, and in the caption, replace “Organic Carbon Mass” with Organic Matter”.

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Figure 6 (VIEWS, 2010). $PM_{2.5}$ Chemical components from IMPROVE measurements at Rubidoux, CA, Fresno, CA, Washington, DC, and Phoenix, AZ. Elemental carbon [ECf by IMPROVE_A (Chow et al., 2007)], ammonium nitrate (ammNO3f), ammonium sulfate (ammSO4FNO3), organic matter [OMCf, 1.8*OC (Pitchford et al., 2007)], and crustal material [SOILf, as a weighted sum of Al, Si, Ca, Mn, and Ti to account for oxides (Watson, 2002)].

4.5. Mitigation Approaches

Charge Question 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?

Chapters 6 to10 present an overview and more detailed discussion of the options for reducing BC emissions from mobile, stationary, residential, and open burning sources. In general, these chapters present fairly detailed information on technologies and control strategies that could be used to reduce BC emissions from these specific sectors. From a policy perspective that may be of interest to Congress, 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. With appropriate assistance and judicious strategies for technology development and transfer, developing countries may be able to leapfrog directly from very high emissions to very low emissions technologies, rather than follow an incremental transition path over a long time period. However, the Panel had concerns about the likelihood that certain mitigation options (e.g. improved cookstoves) would be widely implemented in the near future.

Questions did arise, however, on details related to health effects and BC-specific mitigation. These are discussed on a chapter basis below.

4.5.1 Overview of Mitigation Options (Chapter 6)

Chapter 6 presents a good overview of the current state of understanding of BC mitigation options and the Summary of Key Messages appropriately reflects the content of the chapter. The overview of the impact of trends in BC emissions and role of some key emissions management programs is helpful. However, it will be challenging at best to implement many of the control strategies described in these chapters in the developing world. The Report's discussion of mitigation options is of particular importance and is the very core of the Congressional charge for this work. The diversity of sources and the mitigation options pertaining to each source category makes it a challenging endeavor to present them in a logical and useful way for the generalist reader. The analysis shows that there is a range of costs associated with BC mitigation from sector to sector. These differences should be highlighted as a potential means of prioritizing among approaches across sectors. Also, a more developed discussion on uncertainties is important, in particular with respect to mitigation of OC versus BC (and BrC).

The discussion of climate impacts is focused mostly on changes in global mean temperature. Some implications for other endpoints are briefly mentioned but could receive further discussion. The differences in how various models treat BC external versus internal mixing should be discussed so that differences in model results from either physical, chemical or optical properties

may be evaluated more critically. In addition, the effects (positive/negative) of atmospheric brown clouds warrant more discussion since virtually nothing is discussed regarding changes in rainfall, reduced UV radiation at the surface, and other effects.

The studies of global BC emissions trends presented in the chapter 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 (reference please). The Report should discuss the potential impact of this trend on global BC emissions from all energy-related combustion sectors.

Technology Options

Most of the discussion was focused on control options available for the conventional diesel engine used in mobile applications. This is justified given that the conventional diesel engine is the largest contributor to the BC inventory for the transportation sector. The Panel suggests that the discussion of technology options include the following additional points:

- Given that diesel vehicles are typically about 30% more fuel efficient than gasoline vehicles, the marginal change in emissions when comparing diesel versus gasoline is a reduction in CO_2 emissions per unit of activity but typically an increase in black carbon emissions if the vehicle is not equipped with a filter. In such case, how much of the net benefit or disbenefit of diesel vehicles depends on BC emissions and control of such emissions? For example, if a consumer is choosing whether to purchase a vehicle, which one is more 'climate-friendly' when considering all GHG emissions?
- The report refers to on- and off-road land transport and diesels. However, 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 (i.e., engine size) other fuels (i.e., gasoline) nearly exclusively.
- The chapter should expand on low utilization rate as another factor that would make a retrofit unattractive.
- The chapter should cite the technology that shows potential for non-catalytic DPFs, emulsions and other technologies (e.g., Corbett et al. 2010b); this technology 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.

Fuels

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). In addition, there is likely to be more emphasis on biofuels and "low carbon" fuels in the future. Thus, the Report should discuss the implications of increasing use of biofuels, including the changing composition of PM and emissions of BC.

Health and Exposure

The Key Message on public health co-benefits (p. 6-1, lines 31 et seq.) refers to "reductions in directly emitted $PM_{2.5}$" as a means to substantially reduce human exposure. This discussion needs to clarify whether this refers to the benefits of reduced exposure to PM2.5, reduced exposure to directly emitted PM2.5, or reduced exposure to BC. In regions where PM2.5 is dominated by secondary aerosol, the statement as written would be misleading. Also, the Report discusses spatial aspects of global warming benefits of BC reductions, but does not acknowledge the spatial aspects of the health benefits from $PM_{2.5}$ reductions.

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? In addition, with respect to health effects in developing countries, how will better access to better health care affect the morbidity/mortality numbers? Perhaps the Report should consider a measure of "potential years of life lost" or "avoidable mortality" instead of mortality (or other measures) for evaluating health benefits from reducing BC/PM emissions in developing countries because of differences in health care (e.g., Stevens 2010).

Health benefits are based almost solely on studies for PM and a lot is known 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. However, 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. (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.

Benefits

Developed countries in general, and the U.S. in particular, already have regulations that have reduced, and will continue to reduce, PM emissions (and, as a result, BC emissions). In discussing benefits from mitigation, 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? The Report should explore the most appropriate way to put this into perspective. As presented, the discussion of 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 that shipping – particularly in areas close to the Arctic could lead to BC deposition which could lead to warming. It is not clear which is the lesser of these two sources.

The Report uses a benefit transfer approach to benefit estimation. A clear statement is needed of the benefit to be measured, the existing benefit estimates used (e.g., 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. In other words, readers should be able to reconstruct what was done to develop the transfer estimates.

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

It is valuable for the Report to include a specific internal EPA study (Anenberg et al., in preparation) 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 et al. (2009) report numerous values. It is also reasonable to expect 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.

Costs

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

To be most informative, estimates of BC reductions should be presented along with the costs of these reductions. Also, the authors should consider including a discussion of potential alternative abatement approaches because what we have right now might not be the most cost-effective way to get to reductions. (Additional discussion of cost-effectiveness evaluation is provided in Section 4.6 below.)

4.5.2 Mobile Sources (Chapter 7)

Chapter 7 includes a good overview of mitigation options for mobile sources and it is justifiably diesel-centric. To provide some balance, however, additional discussion of other mobile sources of BC (e.g., gasoline vehicles) is suggested. Also, the Report should more clearly explain the type of engines typically used in various applications because engine type can greatly impact the BC emissions. For example, 2-stroke engines typically are smaller engines (although locomotive engines are also 2-stroke) and are used for lawn and garden equipment such as handheld string trimmers. In contrast, 4-stroke engines are more widely used for lawnmowers and for larger nonroad equipment such as construction, farm, and industrial (CFI) equipment. Other factors that reduce vehicle emissions should be discussed, including policies that reduce demand for vehicle use and transportation in general (e.g., land use 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; and efficiency (reduced aerodynamic drag, idle reduction, hybrid vehicles, etc.). The Report also should present the percentage of the BC ($PM_{2.5}$) reductions that are due to current, in place, policies versus the percentage of reductions expected optimistically from potential future policies.

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/confounders would affect costs and projected reductions. Also, cost per unit reduction is assumed to be constant. If the easiest reductions have been accomplished (i.e., low hanging fruit), it is reasonable to expect that per unit marginal costs will increase to attain additional reductions; a fact that requires discussion. In the description of diesel retrofits, it is not clear what is included in the cost estimates presented in the Report. Please consider clarifying the cost estimates presented with respect to the cost of installation (new and retrofit), fuel, new vehicle inspections, regulatory compliance and possibly others. In addition, the Report should discuss the need for maintenance to preserve effectiveness of a retrofit and the associated costs of this maintenance. Filter disposal and handling of potentially hazardous waste from the filter also should be considered.

The Panel recommends that a summary table be developed to present cost and expected emission reductions for each technology/policy option. Included in the discussion should be clarification on the impact of DPFs on CO_2 or efficiency and clear definition of passive and active DPFs, catalyzed and uncatalyzed devices. As noted in comments on Chapter 6, the discussion of mobile sources includes mostly catalytic technologies, but the chapter also could explore the option of non-catalytic DPFs in some non-road sectors using higher sulfur fuels. If possible, it would be helpful to include a discussion of the costs for future BC control beyond the current regulations.

In the area of ocean going vessel emissions, the Report should reference the strong US position statements submitted to the International Maritime Organization's (IMO) Marine Environment Protection Committee (MEPC), and statements by other governmental bodies internationally (e.g., Norway; Sweden; United States, 2010)

The Report's discussion of engine standards in Europe (Euro 5 and Euro 6) should clarify that the European Particulate Measurement Programme (PMP) methodology (mentioned on p. 7-14,15 and Footnote 11) does not include a thermal denuder. The methodology simply calls for use of a volatile particle remover via thermal treatment of the exhaust samples. A thermal denuder connotes conventional usage of carbon scrubbing and this is not the case for PMP. The distinction between particle mass or particle number is subtle, but deterministically important. PMP excludes some organic compounds, 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.

Table 7-1 is a nice summary of projected mobile source emissions, but there is a substantial lack of transparency/documentation of the basis of these estimates. Basic supporting information that underlie these estimates should be given in an appendix, including, for example: vehicle age distribution by calendar year, and implied rate of turnover of vehicle fleets; emissions deterioration; fleet mix; changes in VMT or other indicators of activity. Throughout the document, appropriate specification of significant figures is needed to avoid misleading impressions of high precision of estimates.

4.5.3 Stationary Sources (Chapter 8)

Chapter 8 discusses the options for mitigating BC emission from stationary sources. 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. With respect to emission inventories, nothing in this chapter confirms that that there will be substantial domestic or international reductions in the 8% and 14% shares, respectively.

The panel identified a number of areas for improvement. In general, the chapter reads like a manual on air pollution control, without being specific enough to BC. The text should concentrate on the sources where meaningful reductions in BC can be found. Specific suggestions for improvement include the following.

- 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," as stated on page 8-7 (lines 17-20); 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.

- The Report seems to confuse two 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 has 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 (p. 8-8, lines 23-25) likely is referring to combustion efficiency (which would affect emission rates per unit of fuel consumed) rather than boiler efficiency.

- The Report (p. 8-9) suggests that fuel switching usually requires small capital investment. However, 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. Sulfur content of coal 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 SO_2 emissions per unit of energy released.

- The discussion of conversion from coal to gas or wood as an option to reduce CO_2 as well as BC emissions requires clarification. 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?

- The Report states that 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. However, similar to the statement previously made and applicable to mobile sources, 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).

A number of additional issues are presented here for consideration for additional discussion in this chapter. First, international changes will be difficult with many dispersed, small operations. Also, the Report should explore the 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 multicyclones. 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 BC emissions from U.S. power generation 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. Finally, 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.

4.5.4 Residential Heating/Cooking and Biomass Burning (Chapters 9 and 10)

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 cooking fires 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 that U.S. 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 effects of BC through deposition on snow and ice – covered surfaces.

Regarding mitigation of BC emissions from cookstoves, the discussion of 90-95% reductions in BC emissions per household seems very optimistic. A sense of a more reasonable level of penetration would be helpful. Also, nothing in this chapter confirms that that there will be substantial domestic or international reductions. Reach and effectiveness of voluntary programs has not been established. In sum, the Panel had some question about the BC reductions likely to be achieved by cookstove mitigation approaches, and notes the following:

- The BC/OC ratio for cook stove emissions is low, as stated many times in the report. Thus is it really the relevant outcome?

- Cookstove emissions are significant only in other countries and it is not clear the extent to which US policy can affect this source.

- 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.

- Some of the possible mitigation strategies appear unrealistic. For example, the ability to provide more efficient cookstoves to potentially millions of people in certain parts of Africa, India, and Asia does not seem feasible – at least not without an infrastructure that can support such a program. 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.

Chapter 10 is comprehensive and adequately describes the relatively limited options for reducing BC emissions from open burning of biomass. Table 10-1 nicely lays out the scope of the challenge, and Section 10.6 clearly discusses the challenges in implementing any of these strategies in the developing world. However, clarification of the share of anthropogenic sources of BC *vis a vis* wildfires is needed. 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). 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.

4.6. Costs and Benefits

[Leads: Boyle, Bui, Walsh]

4.6.1. The Economic Framework

The Panel urges EPA to add a chapter to the Report that summarizes the economic framework and available economic data. The economic frame includes both benefits and costs. The report should clearly state that production and control of BC and PM2.5 are joint products. The economics chapter will build on this foundation, realizing that the control of BC as a share of PM will vary by source and control approach. This recognition is crucial to avoid double counting of benefits across different policies designed to reduce BC and other PM emissions.

Comment [s6]: what does this mean? Can we say this more clearly?

The chapter should start with the endpoints to be valued from reductions in BC emissions, both environmental and human health. The discussion should link to the previous chapters in the report so that the reader will know that the control of different sources and different control strategies can have different environmental and health impacts. That is, not all PM controlled is equal in terms of the environmental and health improvements. Variation could be due to the physical characteristics of the PM controlled, the location of the PM controlled, etc.

The chapter should then move to the benefits to be measured given the health and environmental endpoints identified. Any empirical value estimates reported should be evaluated in terms of the extent that they match with the desired economic concepts of benefits to be measured. Similar consideration should be given to the definition and reporting of costs. More will be said on the issues of costs (Question 12) and benefits (Question 13) below.

The Report discusses uncertainties related to BC reductions early in the report, but these discussions do not carry through to the reporting of benefits and costs. To the extent possible, any qualitative insights that can be provided on how the uncertainties affect the measurement of economic benefits and costs would greatly improve the report.

Reporting benefits and costs in per ton units is a convenient metric, but caution is warranted. These calculations assume linear benefit and cost functions, which may or may not be appropriate, and per-unit benefits and costs may vary by source within an emission category, over time, and spatially (esp. internationally).

Finally, no mention is made of the time dimension of benefits and costs. For example, the up-front cost of diesel particulate filters (DPFs) (including costs of research and development and vehicle purchases as the diesel fleet turns over) are relatively immediate, while environmental benefits are delayed as the new trucks are purchased and DPFs come into full use. The health benefits in terms of premature deaths prevented are events 20 to 30 or more years in the future. The report is silent on the need for caution in assuming per ton costs and benefits occur in the same period, and the need to discount future benefits and costs to the present.

As noted previously, it is often unclear whether the benefits discussed in the Report are coming from existing policies or potential policies that could be put in place sometime in the future. Clarification would allow readers to think more systematically about potential costs and benefits of future actions, as well as future benefits that may accrue from existing policies.

Although the Panel understands the rationale for not including a cost-benefit analysis in the Report, it would be beneficial to organize the discussions of costs and benefits in the Report in a manner such that an informed reader could use the information provided to think more clearly about the potential ramifications of both the costs and the benefits of future policy actions. The Panel urges EPA to do this through the new chapter dedicated to the economic component of the Report and the use of summary tables on benefits and costs in this chapter.

Although the Report falls short on the charge to identify cost-effective approaches and provides an incomplete discussions of benefits, the weight of evidence from the published literature supports a finding that substantial, near-term reductions in BC emissions, in both developed and developing countries, are well-justified by expected human health benefits alone. Thus, the final report should present a compelling case for expanding and accelerating current efforts to reduce BC emissions in both developed (including the U.S.) and developing countries.

4.6.2 Costs of BC Reductions

> *Charge Question 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?*

The Report cites numerous sources of cost information for a variety of remediation technologies that seem to be appropriate. However, the Panel has more fundamental concerns about the cost information presented in the report. A key component of the congressional charge to EPA was to present "… an identification of the most cost-effective approaches (emphasis added) to reduce black carbon emissions …" The Panel does not believe the report is responsive to this element of the congressional charge.

Cost-effectiveness is a relative concept. If there is more than one approach to accomplish a reduction in BC from a given source and all approaches accomplish the same reduction in BC, then the cheapest alternative would be the cost-effective approach. The key element is that more than one approach is compared. In addition, each approach should accomplish the same reduction in BC to facilitate the cost comparison. Throughout the Report, starting with the Executive Summary (e.g., p. Ex-5, 3rd & 4th bullets), the term cost effectiveness is used inappropriately. The report presents a variety of cost numbers associated with a variety of BC mitigation approaches. However, these cost numbers are simply the costs of implementing the discussed remediation approaches, rather than comparative cost-effectiveness of multiple mitigation options.

The Panel understands the difficulty in doing true cost-effectiveness analyses for the unique approaches to mitigating BC from different sources. For each BC source there may be one logical remediation approach and only one cost estimate for implementing this remediation

approach. With this limited information it is impossible to evaluate the cost-effectiveness of remediating BC from this source or the cost effectiveness of implementing a remediation approach. An alternative cost-effectiveness analysis would be to compare the costs of remediating BC from different sources. Such an analysis would allow EPA to identify cost-effective priorities for BC remediation by major emission category and is likely the best approach given the available data. The limitation to this approach is that the different mitigation options do not accomplish the same reductions in BC. Still, if costs are expressed in per-ton units of BC remediated, then comparisons across sources are possible as long as it is reasonable to assume that costs are linear over the ranges (and remediation approaches) considered.

The Panel supports EPA's use of existing cost data, but caution is needed. For each remediation approach, EPA needs to identify the categories of cost that are appropriate, e.g., capital, operating and maintenance, replacement, regulatory, waste disposal, etc. Based on this framework, the report should note whether the available cost estimates include all elements EPA has identified and any qualitative insights EPA can provide on the reliability of each number.

Cost data are currently distributed throughout the report. The Panel recommends that cost information on the remediation approaches be organized in an economics chapter, and that a table be developed to summarize remediation approaches by BC source; a qualitative assessment of the remediation approach; the cost per ton of remediation for each approach, if available; and a qualitative assessment of the reliability of the cost numbers. When possible, the table should indicate whether the cost estimates reported are marginal or average costs and the range of BC remediation relevant to each cost estimate. Costs should be reported in current year dollars (e.g., 2010).

We realize that the summary cost table will have blank cells, but conveying this lack of knowledge is useful and supports the conclusion that research on costs should be added to Chapter 12.

Clearly, not all cost estimates are of the same quality. However, with these data spread throughout the Report and no systematic discussion of their quality, the implicit message is that all estimates are of the same quality. Assembling the cost discussion in one chapter with a summary table that includes qualitative assessments will help the reader understand where there is better cost data and weaker cost data.

Most cost data are based on U.S. information. At the end of the cost portion of the economics chapter, the Panel urges EPA to include a section that discusses what is known about cost data for international BC sources. The international discussion should not be interspersed throughout the economics chapter.

It should also be noted that there is strong evidence that actual costs tend to be lower than the *ex ante* estimates, indicating that the preliminary cost estimates tend to be higher than they actually turn out to be (references please). Further, the actual costs of controls are expected to be substantially lower in many developing countries. In part this is due to the lower labor costs in these countries compared to those in the developed world and in part due to the research and

development that has already been conducted and does not need to repeated, e.g., later adopters have lower costs than the original adopters.

4.6.3. Benefits of BC Reductions

> *Charge Question 13. Does the draft Report appropriately characterize the range and magnitude of potential benefits for both climate and public healthy that could result from reductions in BC emissions?*

As noted in the response to Charge Question 6 (Section 4.2), the Panel notes that the Report's characterization of the potential health benefits from BC emissions reductions was too cautious. Although many of the health papers cited focus on PM2.5 and not just on BC emissions, it is important to note that on a mass basis, BC is the largest component of PM emissions. There is overwhelming evidence that reductions in BC emissions will have widespread health benefits. Given the potential magnitude of these benefits, greater emphasis should be given to this result. To this end, it is critical for the Report to have a more detailed and thorough review of the existing health literature (along with a discussion of any important issues surrounding uncertainty) in Chapter 3.

Comment [s7]: there is no discussion of climate benefits in this section…

The Report should emphasize the joint benefits that will accrue from the reduction of BC emissions. By controlling BC emissions, there are both health benefits and climate benefits. Furthermore, because of the joint production of BC and other particulate matter, the control of BC emissions also will help to control releases of other fine particulate matter – which may have additional benefits

Although uncertainties in potential benefits are discussed in the earlier chapters of the Report, those uncertainties were not carried forward to the later chapters. The Panel strongly recommends that a Summary Table be constructed that lists the potential health and environmental endpoints from BC mitigation, the regional impact of their effects (local, regional, and/or global), and the monetary benefits, where possible, as well as the associated uncertainties surrounding the magnitude of these benefits. The report should clearly define how the monetary benefits are measure (value of a statistical life, avoided medical costs, a stated-preference study, etc.) and the incremental change in BC, or more appropriately PM, for which the monetary values apply.

As with costs (see response to Question 12), throughout the Report there is an implicit assumption that benefits are a linear function of BC mitigation. The Panel agrees that for industrialized countries, in particular, the U.S., Canada, and Western Europe, the assumption of linear benefits (constant marginal costs/benefits) may be reasonable. For developing countries, however, this assumption is more problematic and deserves careful consideration.

Benefits associated with BC emissions reductions, particularly those in developing countries, will depend upon the feasibility of implementing effective mitigation strategies. As noted in Section 4.5 above, The Report would benefit from a more thorough investigation of the feasibility of the mitigation strategies to better inform the reader about the likely realization of the benefits.

Although a great deal of attention is placed on the differential effects that BC emissions may have both seasonally and spatially, the corresponding discussion is not developed in the discussion of benefits in the Report. These differences should be made more transparent.

Because the Report makes use of benefit measures that are provided in the literature, it is critical to specify exactly what benefits are being measured and discussed. It is also important to note that measurements that are typically used in industrialized countries (e.g. value of a statistical life) may make less sense when used in developing countries. Problems of measurement may be further exacerbated when potential benefits come in the future and must be discounted to the present.

4.7. <u>Metrics for BC Climate Effects</u>

Charge Question 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 CO_2, and to compare among BC mitigation alternatives?

[Lead: Fuglestvedt, Russell]

Chapter 11 gives a good overview of metrics and related issues. It contains important information but the discussions needs to be more linked to possible climate targets or purposes of BC mitigation strategies. The adequacy of metrics depends on the overall purpose, which is not clearly stated beyond a certain focus on rate and short term warming. As discussed below, an evaluation of adequacy of metrics must be put into context (see report from IPCC Expert meeting on metrics, section 4.1.1.—reference please). In this regard, it may be useful to provide the specific mandate from Congress, along with the strategy EPA is taking to respond to the charge. The Panel suggests that the material in Chapter 11 be divided into two parts; one part discussing concepts and perspectives (to precede the current Chapter 2) and one later on applications in the context of mitigation and policy making. Further, the Committee recommends improvements to the graphics used in the chapter, and suggests a number of potential graphics below.

Dependence on Climate Goals

The report should state clearly for policymakers that the utility of particular metrics will depend upon the goals of climate policy. Even though not intended to recommend a specific policy or set of policies, the Report should further discuss possible climate policy goals (e.g., climate stabilization, reducing short-term warming, reducing the rate of warming), and how the various goals would impact the choice of metrics and mitigation strategies.

Within this context, the Report would also benefit from a more focused and structured discussion of the 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.*" which 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 likely motivation for BC controls, this should be followed up throughout the report. It will have impacts on the use of metrics and potentially also on the identification of cost-effective multi-component mitigation strategies.

As shown in several papers (Manne and Richels, 2001; Shine et al., 2007; Manning and Raisinger, 2011), the global warming potential (GWP) concept is not suited for a policy with

stabilization as the overall goal. The global temperature change potential, GTP(t), concept presented by Shine et al. (2007) is one alternative in this context; see discussion below.

The Report is unclear about whether metrics are sought to choose among various BC reduction alternatives, or across components. Attempts to assess the effectiveness of mitigation options for multiple categories of climate forcers (i.e., 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. Better alternatives include a multi-basket approach (e.g., Rypdal et al. 2005), or a single basket with a metric that is a function of time (e.g., Manne & Richels 2001) or the GTP(t) from Shine et al. 2007).

Further, it is difficult to force BC into the current climate policy framework (i.e., the Kyoto Protocol's multi-component basket) if we are concerned about more than long term change. However, if one is concerned about more than the long term temperature, then an additional target could be formulated (e.g., a short term target or rate of change target). As stated by Berntsen et al. (2010):

> Our discussion assumes a climate policy with one long-term target. It has been suggested that an additional short-term (Fuglestvedt et al. 2000; Rypdal et al. 2005; Jackson 2009) or mid-term target (O'Neill et al. 2010) can be introduced to the climate policy as an interim goal leading toward the long-term target. The motivation to set up such an interim target may also be as a means to avoid crossing the tipping elements (Lenton et al. 2008) or to curb environmental side-effects (e.g. adverse human impacts of BC emissions). When such an additional target is introduced, a possibility to develop strategies and policies that could in a consistent manner employ different timescales for metric calculations opens up.

The report would also benefit from a discussion of the scale (i.e., national, regional, and/or global) of possible BC policies.

Scope of metrics

The importance of regionality and spatial variations is stressed, but the Report should distinguish more clearly between the regionality of the *driver* (i.e., emissions) and the *response.* For BC, the location of emissions is important to the magnitude and pattern of response. Note however, that the regional patterns of the radiative forcing (RF) – which is the main end point used in the Report– are different than the patterns of the temperature response. The chapter could also be more explicit about the importance of time horizon with respect to rate of change and short- versus long-term effects.

Obviously, one purpose of metrics is to put effects of various climate forcers on a common scale; i.e. to compare effects of BC reductions to effects of reducing other components. This use of metrics is consistent with the Congressional charge. However, the chapter also discusses the use of metrics to compare across sources (which are different due to different location of emissions). If direct comparison with CO_2 is not the objective, it would be useful to group the components in different baskets according to their lifetimes/adjustment times (Fuglestvedt et al. 2000; Rypdal et al. 2005; Jackson 2009). To summarize, it is sometimes unclear whether the report is searching

for metrics to compare alternative for reducing BC or to compare BC reductions with CO_2 reductions.

Application of Metrics

The differences between BC and CO_2 are stressed throughout the report, but the *implications* of these differences could be given more attention. A figure showing the RF and dT responses to pulses as well as sustained constant emissions of BC and CO_2 would illustrate the differences in temporal behavior of these components (see below).

The presentation of RF 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 (as in Fig. 2-16 from Unger et al., 2010). Figure 2-7 in the Report (reproduced below) shows the current RF relative to pre-industrial times.

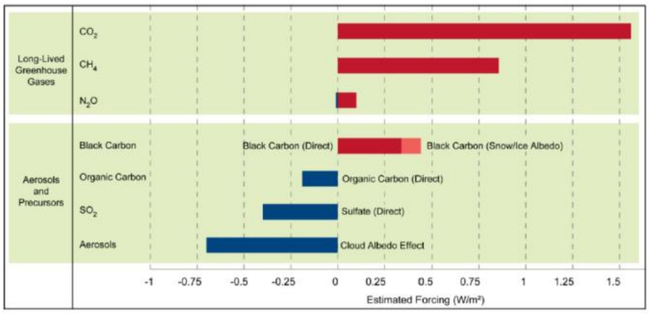


Figure 2-7. Components of Global Average Radiative Forcing for Emissions of Principal Gases, Aerosols, and Aerosol Precursors.

These data, which represent the instantaneous values for (typically) year 2000, could be placed in context using a graphic that illustrates these levels relative to pre-industrial times. (e.g., Fig XX below…Source for this graphic?)

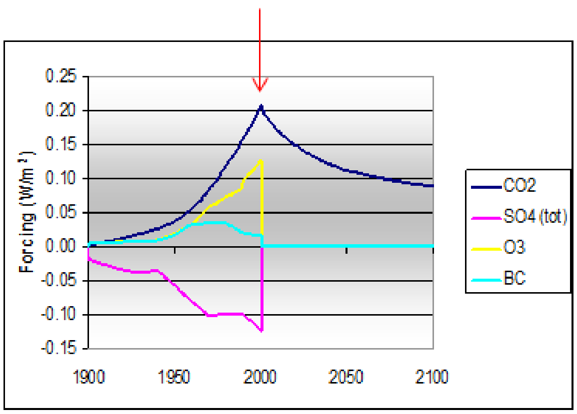


Figure XX. Forcing history due to emissions of CO_2, SO_2, BC and ozone pre-cursors from the transport sector up to 2000 with zero emissions after this time. (Reprinted with permission..)

This figure also shows that the perturbation of CO_2 is very long-lived, while forcings from O_3, black carbon (BC) and sulphate (SO_4) die out quickly after the emissions stop. One should also keep in mind the very different behavior the agents show after the chosen year due to the very different lifetimes; in other words, Figure 2-7 in the Report does not say anything about the future role of the various RF agents.

The different purposes of these backward and forward looking perspectives (for attribution/understanding vs. policy making, respectively) should be explained. The report would benefit from more emphasis on forward looking perspectives since the motivation is mitigation and policy-making; e.g., as in Figure 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 the extent of the cuts (% or mass) in BC and methane emissions that are assumed in the calculations behind Figure 6.4.).

RF for 20 and 100 years after emission - for sustained constant emissions – is used as a metric in Figure 2-16. In order to avoid confusion, it should be explained that this is approximately the same as using integrated RF for one-year pulse emissions; which is in line with using AGWPs for evaluation of emissions.

Various metrics are discussed in Chapter 11, 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 x $M(H)_i$; and implications of this. See, for example, Figure 7 from Fuglestvedt et al. (2010). Alternatively, a figure like Figure ig. 2.22 from IPCC AR4 WG1, see below (or an update) would be useful.

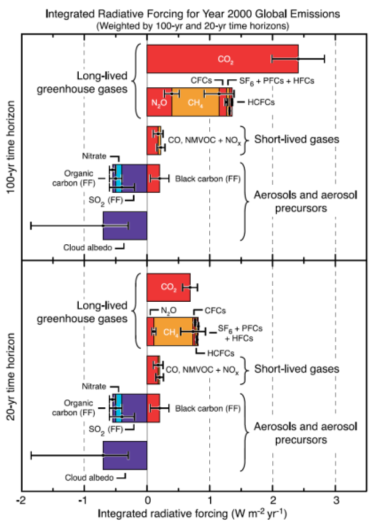
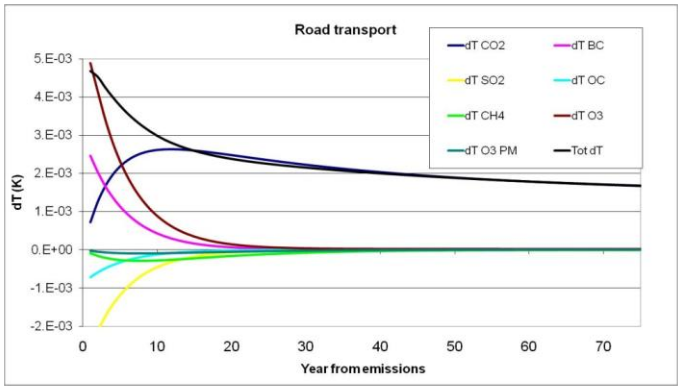


Figure 2.22 from IPCC AR4 WP1. *Integrated RF of year 2000 emissions over two time horizons (20 and 100 years). The fi gure gives an indication of the future climate impact of current emissions. The values for aerosols and aerosol precursors are essentially equal for the two time horizons. It should be noted that the RFs of short-lived gases and aerosol depend critically on both when and where they are emitted; the values given in the fi gure apply only to total global annual emissions. For organic carbon and BC, both fossil fuel (FF) and biomass burning emissions are included. The uncertainty estimates are based on the uncertainties in emission sources, lifetime and radiative effi ciency estimates.* ***(NB Note wrong unit. Should be W m^{-2} yr).***

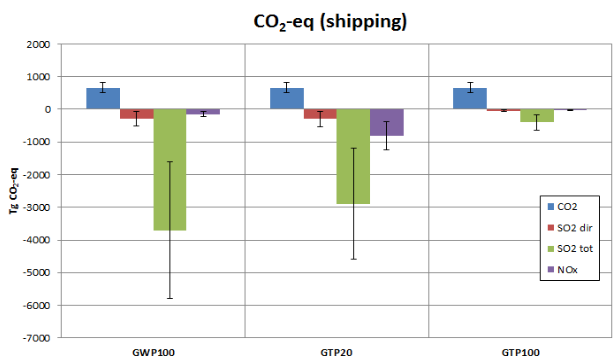
The effects of BC mitigation on short term warming and rate of warming (assuming ranges for the magnitudes of the various effects of BC) could also be illustrated by a figure.

Metrics can be used to illustrate the very different temporal behavior of BC and CO_2. The figure below (from Berntsen and Fuglestvedt, PNAS 2008) is an example showing the development in temperature effect of one year of global emission from road transport. The effect of BC is strong but short-lived, while the effect of CO_2 is more long-lived. For sustained emissions the effect of BC would be larger at an approximately stable level since new emissions are added to the atmosphere each year. The effect of CO_2, on the other hand, would accumulate over time. Such illustrations would help to convey the very different behavior of these two components.



Global mean temperature (Kelvin) change after 1 year of emissions from the major transportation modes given per component and for the total net effect.

Metrics can also be used to illustrate the propagation of uncertainties in emission estimates (see example for effects of shipping emissions below—reference for this figure?).



As discussed in chapter 11, there is currently no single metric (e.g., GWP) that is widely accepted by the science and research community for comparison of climate impacts of different components. However, it is important to differentiate between various types applications of metrics: 1) Policy targets and international agreements and 2) Assessment and illustration of effects. Obviously, it is much more difficult to agree upon a metric for the first application, while for assessments and illustration of effects, the various metrics can provide insight to the nature of the different contributions from various components, such as BC, CO_2 and methane.

In chapter 2 the RF numbers for the direct effects and albedo effects are presented on graphs. It could be noted that the efficacies of these mechanisms are probably very different.

Possible Metrics

In the report, the OC/BC ratio is presented as a metric. The Panel recognizes that this ratio is a useful indicator but recommends that it not be referred to as a metric since this is not based on any climate response (such as RF or dT). The Report discusses problems related to metrics for short-lived components and presents two new alternatives, STRE and SFP**,** and the Panel suggests a third:

- ***Surface Temperature Response per unit continuous emission*** (STRE) from Jacobson (2010): This metric is similar to the GTP for sustained emissions (GTPs) presented by Shine et al. (2005). The implicit assumption on sustained emissions (i.e., future behavior and emissions) should be kept in mind for applications in policymaking. Pulse-based metrics can be used for annual emissions and may also be used as building blocks for cases with sustained emissions or for scenarios.

- The ***Specific Forcing Pulse*** (SFP) from Bond et al. (2011) is similar to the Absolute Global Warming Potential (AGWP) ,with the important difference that selected regions for RF response are chosen rather than global mean RF. The SFP metric uses a different unit (GJ/g) which may seem confusing, so it is quite important that this metric be well explained. It is also worth noting that the regional patterns in RF does not indicate regional pattern in temperature response.

- The **GTP concept** also could be discussed with time horizon as function of proximity to the target year, as suggested by Shine et al. (2007). Figure 5 in Shine et al. (2007) shows the GTP for BC for various scenarios, target years and climate sensitivities. While the GWP value remains constant over time, the GTP values are low in the beginning but increases towards the target year. In other words, the contribution from BC to warming in the target year – *relative to* CO_2 – increases over time. This says nothing about the total absolute reduction needed to stay below the temperature ceiling. See Berntsen et al. 2010, for a discussion of reductions in BC vs CO_2 for a situation with a temperature ceiling.

As a general comment, the Panel recommends that Table 11-1 indicate how transparent the metric formulations are; i.e., simple analytical formulation vs. a complex numerical model.

Additional Relevant Literature

Berntsen et al. (2010) discusses whether a mitigation strategy directed towards BC may hamper CO2 abatement, and Rypdal et al. (2005) and Jackson (2009) discuss how short-lived components can be included in climate policies. Additional papers on the choice and application of metrics include Tanaka et al. (2010) and Manning and Resinger (2011). Studies that could be given more emphasis include Rypdal et al. (2009), which gives GWP and GTP values for various regions of emissions, and Shine et al. (2007), which presents the GTP(t) concept and obtains results (with a physical and transparent metric) that are similar to the results from Manne and Richels (2001), who use an economic model framework.

4.8. Research Priorities

Charge Question 15. Does the draft report appropriately identify the highest research needs regarding BC?

[Leads: Mauzerall, Poirot]

Overall, Chapter 12 provides a succinct summary of most of the key conclusions and research needs outlined in the report. However, it is lacking in several areas outlined below. In addition, it is written in a technocratic style and would be improved by careful editing and improved formatting and structure which would make it more accessible and interesting to a non-expert.

Conclusions

As the EPA report is intended to serve as a review and synthesis of available scientific and technical information on BC, it is important that the findings of the UNEP report on BC and ozone, which will be released in mid-June, be discussed. Ideally findings in the EPA and UNEP report will be consistent as this will add credibility to both reports. If there are areas where findings differ it would be useful to provide an explanation. The draft summary of the UNEP report is available here: http://www.unep.org/dewa/Portals/67/pdf/Black_Carbon.pdf

The conclusions in the EPA report are cautious. In particular, the conclusions stop short of summarizing the benefits of specific mitigation options. The UNEP report identifies measures that improve climate and air quality and have a large emission reduction potential. It would be useful if the EPA report took a similar approach and identified BC mitigation technologies/methodologies that appear feasible and cost effective. Commenting on whether they are the same or different from those in the UNEP report would be instructive. It would be particularly useful to rank mitigation options in some way that took account of their feasibility, cost, health and climate benefits. This would facilitate the identification of useful future mitigation policies.

Although including research recommendations is valuable, it is important to provide conclusions which provide clear information on what we know should be done now (i.e., that has clear benefits) that does not require waiting for further research to justify action.

Research Recommendations

The research recommendations touch on many important topics but are too vague in places. In addition, they should be put in priority order, not in the order they appear in the report. Clarity on what should be done first and what can wait is needed. What do we need to know to help EPA determine regulatory priorities? More information on the scale of the effort should be included – should it be domestic? Regional? International?

Specific suggestions for improvement include the following:

Topic #2. "Continued investigation of basic microphysical and atmospheric processes…" There is substantial uncertainty regarding the effect of BC on cloud condensation nuclei and hence on the indirect effect of clouds. Research in this area should be explicitly included in this topic as there is now speculation that indirect effects of BC may significantly decrease the positive direct radiative forcing of BC. In addition, the title of this topic should be improved.

Of key importance is research on which BC mitigation strategies are most cost-effective and beneficial for public health and climate. This is buried in the research recommendations #5 and needs to be a topic on its own. Research needs include:

1. more information on costs and benefits of mitigation by sector.
2. R&D on improved mitigation strategies for some sectors.
3. a way to compare numbers across sectors.
4. information on the toxicity of BC – specific impacts on health versus those of other particulates
5. research on the climate impacts of BC reaching specific climate sensitive regions.

Topic #6. Climate/health metrics.
It would be useful to have a metric which combined the climate and health impacts of BC into a single metric as mentioned in this section. This climate/health metric could then be used to compare mitigation strategies across sectors and regions. Research into such a metric would be valuable and could facilitate national, regional and global mitigation strategies for BC. It's better to start with a goal (e.g., long term climate stabilization or short term/rate of warming or integration of health and climate effects?) then choose a metric. Then decide if the metric is , to compare across all emission sources? Across components – all or separated into groups? Single basket of all warming agents or multiple baskets for different types of agents or comparison between different emission sources of the same agent?

As noted previously, it is unclear whether the Report is searching for metrics for

- choosing among various BC reduction alternatives or
- across components with positive radiative forcing (i.e,. CO2, BC, CH4, etc.)

This should be clarified.

REFERENCES

[merge the 2 lists of references]

Alfaro, S.C.; Lafon, S.; Rajot, J.L.; Formenti, P.; Gaudichet, A.; Maille, M. (2004). Iron oxides and light absorption by pure desert dust: An experimental study. J. Geophys. Res. - Atmospheres, 109(D8)

Andreae, M.O.; Schmid, O.; Yang, H.; Chand, D.; Yu, J.Z.; Zeng, L.M.; Zhang, Y.H. (2008). Optical properties and chemical composition of the atmospheric aerosol in urban Guangzhou, China. Atmos. Environ., 42(25): 6335-6350.

ARA (2011). Introduction to the SouthEastern Aerosol Research and Characterization Study experiment (SEARCH). prepared by Atmospheric Research & Analysis, Inc., Plano, TX, http://www.atmospheric-research.com/studies/SEARCH/index.html

Arola, A.; Schuster, G.; Myhre, G.; Kazadzis, S.; Dey, S.; Tripathi, S.N. (2011). Inferring absorbing organic carbon content from AERONET data. Atmos. Chem. Phys., 11(1): 215-225.

Bachmann, J.D. (2007). Will the circle be unbroken: A history of the US national ambient air quality standards-2007 Critical Review. J. Air Waste Manage. Assoc., 57(6): 652-697.

Bae, M.S.; Hong, C.S.; Kim, Y.J.; Han, J.S.; Moon, K.J.; Kondo, Y.; Komazaki, Y.; Miyazaki, Y. (2007). Intercomparison of two different thermal-optical elemental carbons and optical black carbon during ABC-EAREX2005. Atmos. Environ., 41(13): 2791-2803.

Bae, M.S.; Schauer, J.J.; Turner, J.R.; Hopke, P.K. (2009). Seasonal variations of elemental carbon in urban aerosols as measured by two common thermal-optical carbon methods. Sci. Total Environ., 407(18): 5176-5183.

Bahadur, R.; Feng, Y.; Russell, L.M.; Ramanathan, V. (2011). Impact of California's air pollution laws on black carbon and their implications for direct radiative forcing. Atmos. Environ., 45(5): 1162-1167.

Bond, T.C.; Anderson, T.L.; Campbell, D.E. (1999). Calibration and intercomparison of filter-based measurements of visible light absorption by aerosols. Aerosol Sci. Technol., 30(6): 582-600.

Braun, A.; Mun, B.S.; Huggins, F.E.; Huffman, G.P. (2007). Carbon speciation of diesel exhaust and urban particulate matter NIST standard reference materials with C(1s) NEXAFS spectroscopy. Environ. Sci. Technol., 41(1): 173-178.

Burnet, P.G.; McCrillis, R.C.; Morgan, S.J. (1988). Performance of certified woodstoves under field conditions. In Transactions, PM10: Implementation of Standards, Mathai, C. V., Stonefield, D. H., Eds.; Air Pollution Control Association: Pittsburgh, PA, 664-672

Butler, A.T. (1988). Control of woodstoves by state regulation as a fine particulate control strategy. In Transactions, PM10: Implementation of Standards, Mathai, C. V., Stonefield, D. H., Eds.; Air Pollution Control Association: Pittsburgh, PA, 654-663

Calvello, M.; Esposito, F.; Pavese, G.; Serio, C. (2010). Physical and optical properties of atmospheric aerosols by in-situ and radiometric measurements. Atmos. Chem. Phys., 10(5): 2195-2208.

Campbell, D.E.; Copeland, S.; Cahill, T.A. (1995). Measurement of aerosol absorption coefficient from Teflon filters using integrating plate and integrating sphere techniques. Aerosol Sci. Technol., 22(3): 287-292.

Cao, J.J.; Lee, S.C.; Chow, J.C.; Watson, J.G.; Ho, K.F.; Zhang, R.J.; Jin, Z.D.; Shen, Z.X.; Chen, G.C.; Kang, Y.M.; Zou, S.C.; Zhang, L.Z.; Qi, S.H.; Dai, M.H.; Cheng, Y.; Hu, K. (2007). Spatial and seasonal distributions of carbonaceous aerosols over China. J. Geophys. Res. -Atmospheres, 112(D22S11): 1-9. doi:10.1029/2006JD008205.

Chan, T.W.; Huang, L.; Leaitch, W.R.; Sharma, S.; Brook, J.R.; Slowik, J.G.; Abbatt, J.P.D.; Brickell, P.C.; Liggio, J.; Li, S.M.; Moosmüller, H. (2010). Observations of OM/OC and specific attenuation coefficients (SAC) in ambient fine PM at a rural site in central Ontario, Canada. Atmos. Chem. Phys., 10(5): 2393-2411.

Chen, L.-W.A.; Chow, J.C.; Watson, J.G.; Moosmüller, H.; Arnott, W.P. (2004). Modeling reflectance and transmittance of quartz-fiber filter samples containing elemental carbon particles: Implications for thermal/optical analysis. J. Aerosol Sci., 35(6): 765-780. doi: 10.1016/j.jaerosci.2003.12.005.

Cheng, J.Y.W.; Chan, C.K.; Lau, A.P.S. (2011). Quantification of airborne elemental carbon by digital imaging. Aerosol Sci. Technol., 45(5): 581-586.

Cheng, Y.; He, K.B.; Duan, F.K.; Zheng, M.; Ma, Y.L.; Tan, J.H.; Du, Z.Y. (2010). Improved measurement of carbonaceous aerosol: evaluation of the sampling artifacts and inter-comparison of the thermal-optical analysis methods. Atmos. Chem. Phys., 10(17): 8533-8548.

Chou, C.C.K.; Chen, W.N.; Chang, S.Y.; Chen, T.K.; Huang, S.H. (2005). Specific absorption cross-section and elemental carbon content of urban aerosols. Geophys. Res. Lett., 32(L21808): 1-4. doi:10.1029/2005GL024301.

Chow, J.C.; Watson, J.G.; Chen, L.-W.A.; Arnott, W.P.; Moosmüller, H.; Fung, K.K. (2004). Equivalence of elemental carbon by Thermal/Optical Reflectance and Transmittance with different temperature protocols. Environ. Sci. Technol., 38(16): 4414-4422.

Chow, J.C.; Watson, J.G.; Chen, L.-W.A.; Chang, M.C.O.; Robinson, N.F.; Trimble, D.; Kohl, S.D. (2007). The IMPROVE_A temperature protocol for thermal/optical carbon analysis: Maintaining consistency with a long-term database. J. Air Waste Manage. Assoc., 57(9): 1014-1023.

Chow, J.C.; Watson, J.G.; Crow, D.; Lowenthal, D.H.; Merrifield, T.M. (2001). Comparison of IMPROVE and NIOSH carbon measurements. Aerosol Sci. Technol., 34(1): 23-34.

Chow, J.C.; Watson, J.G.; Doraiswamy, P.; Chen, L.W.A.; Sodeman, D.A.; Lowenthal, D.H.; Park, K.; Arnott, W.P.; Motallebi, N. (2009). Aerosol light absorption, black carbon, and elemental carbon at the Fresno Supersite, California. Atmos. Res., 93(4): 874-887.

Chow, J.C.; Watson, J.G.; Doraiswamy, P.; Chen, L.-W.A.; Sodeman, D.A.; Ho, S.S.H.; Kohl, S.D.; Trimble, D.L.; Voepel, H.; Fung, K.K. (2006). Climate change - Characterization of black carbon and organic carbon air pollution emissions and evaluation of measurement methods. Phase I: Method intercomparison. Volume I: Interim report. Report Number DRI Contract No. 04-307; prepared by Desert Research Institute, Reno, NV, for California Air Resources Board and the California Environmental Protection Agency, Sacramento, CA;

Chow, J.C.; Watson, J.G.; Green, M.C.; Frank, N.H. (2010). Filter light attenuation as a surrogate for elemental carbon. J. Air Waste Manage. Assoc., 60(11): 1365-1375.

Chow, J.C.; Watson, J.G.; Pritchett, L.C.; Pierson, W.R.; Frazier, C.A.; Purcell, R.G. (1993). The DRI Thermal/Optical Reflectance carbon analysis system: Description, evaluation and applications in U.S. air quality studies. Atmos. Environ., 27A(8): 1185-1201.

Collaud, C.M.; Weingartner, E.; Apituley, A.; Ceburnis, D.; Fierz-Schmidhauser, R.; Flentje, H.; Henzing, J.S.; Jennings, S.G.; Moerman, M.; Petzold, A.; Schmid, O.; Baltensperger, U. (2010). Minimizing light absorption measurement artifacts of the aethalometer: Evaluation of five correction algorithms. Atmos. Meas. Tech., 3(2): 457-474.

Corrigan, C.E.; Roberts, G.C.; Ramana, M.V.; Kim, D.; Ramanathan, V. (2008). Capturing vertical profiles of aerosols and black carbon over the Indian Ocean using autonomous unmanned aerial vehicles. Atmos. Chem. Phys., 8(3): 737-747.

Cross, E.S.; Onasch, T.B.; Ahern, A.; Wrobel, W.; Slowik, J.G.; Olfert, J.; Lack, D.A.; Massoli, P.; Cappa, C.D.; Schwarz, J.P.; Spackman, J.R.; Fahey, D.W.; Sedlacek, A.; Trimborn, A.; Jayne, J.T.; Freedman, A.; Williams, L.R.; Ng, N.L.; Mazzoleni, C.; Dubey, M.; Brem, B.; Kok, G.L.; Subramanian, R.; Freitag, S.; Clarke, A.; Thornhill, D.; Marr, L.C.; Kolb, C.E.; Worsnop, D.R.; Davidovits, P. (2010). Soot particle studies instrument intercomparison -Project overview. Aerosol Sci. Technol., 44(8): 592-611.

Currie, L.A.; Benner, B.A., Jr.; Cachier, H.; Cary, R.; Chow, J.C.; Druffel, E.R.M.; Eglinton, T.I.; Gustafsson, Ö.; Hartmann, P.C.; Hedges, J.I.; Kessler, J.D.; Kirchstetter, T.W.; Klinedinst, D.B.; Klouda, G.A.; Marolf, J.V.; Masiello, C.A.; Novakov, T.; Pearson, A.; Prentice, K.M.; Puxbaum, H.; Quinn, J.G.; Reddy, C.M.; Schmid, H.; Slater, J.F.; Watson, J.G.; Wise, S.A. (2002). A critical evaluation of interlaboratory data on total, elemental, and isotopic carbon in the carbonaceous particle reference material, NIST SRM 1649a. J. Res. National Bureau Standards, 107(3): 279-298.

Dasgupta, D.; Demichelis, F.; Pirri, C.F.; Tagliaferro, A. (1991). Pi-bands and gap states from optical absorption and electron spin resonance studies on amorphous carbon and amorphous hydrogenated carbon films. Physical Review B, 43(3): 2131-2135.

Dey, S.; Tripathi, S.N.; Singh, R.P.; Holben, B.N. (2006). Retrieval of black carbon and specific absorption over Kanpur city, northern India during 2001-2003 using AERONET data. Atmos. Environ., 40(3): 445-456. ISI:000235325300004.

Dillner, A.M.; Stein, C.; Larson, S.M.; Hitzenberger, R. (2001). Measuring the mass extinction efficiency of elemental carbon in rural aerosol. Aerosol Sci. Technol., 35(6): 1009-1021.

Favez, O.; Alfaro, S.C.; Sciare, J.; Cachier, H.; Abdelwahab, M.M. (2009). Ambient measurements of light-absorption by agricultural waste burning organic aerosols. J. Aerosol Sci., 40(7): 613-620.

Flores-Cervantes, D.X.; Reddy, C.M.; Gschwend, P.M. (2009). Inferring black carbon concentrations in particulate organic matter by observing pyrene fluorescence losses. Environ. Sci. Technol., 43(13): 4864-4870.

Fu, Q.; Sun, W.B. (2006). Apparent optical properties of spherical particles in absorbing medium. J. Quant. Spectrosc. Radiat. Transfer, 100(1-3): 137-142. ISI:000236892800014.

Fujita, E.M.; Campbell, D.E.; Arnott, W.P.; Chow, J.C.; Zielinska, B. (2007). Evaluations of the chemical mass balance method for determining contributions of gasoline and diesel exhaust to ambient carbonaceous aerosols. J. Air Waste Manage. Assoc., 57(6): 721-740.

Fuller, K.A.; Malm, W.C.; Kreidenweis, S.M. (1999). Effects of mixing on extinction by carbonaceous particles. J. Geophys. Res., 104(D13): 15941-15954.

Gan, T.H.; Hanhela, P.; Mazurek, W.; Gillett, R. (2010). Characteristics of submarine engine diesel particulates in the maritime environment. J. Aerosol Sci., 41(1): 23-35.

Gilardoni, S.; Vignati, E.; Wilson, J. (2011). Using measurements for evaluation of black carbon modeling. Atmos. Chem. Phys., 11(2): 439-455.

Hammes, K.; Schmidt, M.W.I.; Smernik, R.J.; Currie, L.A.; Ball, W.P.; Nguyen, T.H.; Louchouarn, P.; Houel, S.; Gustafsson, O.; Elmquist, M.; Cornelissen, G.; Skjemstad, J.O.; Masiello, C.A.; Song, J.; Peng, P.; Mitra, S.; Dunn, J.C.; Hatcher, P.G.; Hockaday, W.C.; Smith, D.M.; Hartkopf-Froeder, C.; Boehmer, A.; Luer, B.; Huebert, B.J.; Amelung, W.; Brodowski, S.; Huang, L.; Zhang, W.; Gschwend, P.M.; Flores-Cervantes, D.X.; Largeau, C.; Rouzaud, J.N.; Rumpel, C.; Guggenberger, G.; Kaiser, K.; Rodionov, A.; Gonzalez-Vila, F.J.; Gonzalez-Perez, J.A.; de la Rosa, J.M.; Manning, D.A.C.; Lopez-Capel, E.; Ding, L. (2007). Comparison of quantification methods to measure fire-derived (black/elemental) carbon in soils and sediments using reference materials from soil, water, sediment and the atmosphere. Global Biogeochemical Cycles, 21(3)

Han, Y.M.; Cao, J.J.; An, Z.; Chow, J.C.; Watson, J.G.; Jin, Z.D.; Fung, K.K.; Liu, S. (2007). Evaluation of the thermal/optical reflectance method for quantification of elemental carbon in sediments. Chemosphere, 69(4): 526-533.

Hansen, J.C.; Woolwine, W.R.; Bates, B.L.; Clark, J.M.; Kuprov, R.Y.; Mukherjee, P.; Murray, J.A.; Simmons, M.A.; Waite, M.F.; Eatough, N.L.; Eatough, D.J.; Long, R.; Grover, B.D. (2010). Semicontinuous PM2.5 and PM10 mass and composition measurements in Lindon, Utah, during winter 2007. J. Air Waste Manage. Assoc., 60(3): 346-355.

Hopkins, R.J.; Tivanski, A.V.; Marten, B.D.; Gilles, M.K. (2007). Chemical bonding and structure of black carbon reference materials and individual carbonaceous atmospheric aerosols. J. Aerosol Sci., 38(6): 573-591.

Horvath, H. (1993). Atmospheric light absorption - A review. Atmos. Environ., 27A(3): 293-317.

Hough, M.L.; Tombleson, B.; Wolgamott, M. (1988). Oregon approach to reducing residential woodsmoke as part of the PM10 strategy. In Transactions, PM10: Implementation of Standards, Mathai, C. V., Stonefield, D. H., Eds.; Air Pollution Control Association: Pittsburgh, PA, 646-653

Hsieh, Y.P.; Bugna, G.C. (2008). Analysis of black carbon in sediments and soils using multi-element scanning thermal analysis (MESTA). Organic Geochemistry, 39(11): 1562-1571.

IMPROVE (2011). Interagency Monitoring of Protected Visual Environments. prepared by National Park Service, Ft. Collins, CO, http://vista.cira.colostate.edu/IMPROVE

Jacobson, M.Z. (1999). Isolating nitrated and aromatic aerosols and nitrated aromatic gases as sources of ultraviolet light absorption. J. Geophys. Res., 104(D3): 3527-3542.

Jacobson, M.Z. (2000). A physically-based treatment of elemental carbon optics: Implications for global direct forcing of aerosols. Geophys. Res. Lett., 27: 217-220.

Jacobson, M.Z. (2005). A refined method of parameterizing absorption coefficients among multiple gases simultaneously from line-by-line data. Journal of the Atmospheric Sciences, 62(2): 506-517.

Jacobson, M.Z. (2006). Effects of externally-through-internally-mixed soot inclusions within clouds and precipitation on global climate. Journal of Physical Chemistry A, 110(21): 6860-6873.

Jimenez, J.; Claiborn, C.S.; Larson, T.; Gould, T.; Kirchstetter, T.W.; Gundel, L. (2007). Loading effect correction for real-time aethalometer measurements of fresh diesel soot. J. Air Waste Manage. Assoc., 57(7): 868-873.

Kanaya, Y.; Komazaki, Y.; Pochanart, P.; Liu, Y.; Akimoto, H.; Gao, J.; Wang, T.; Wang, Z. (2008). Mass concentrations of black carbon measured by four instruments in the middle of Central East China in June 2006. Atmos. Chem. Phys., 8(24): 7637-7649.

Kirchstetter, T.W.; Agular, J.; Tonse, S.; Fairley, D.; Novakov, T. (2008). Black carbon concentrations and diesel vehicle emission factors derived from coefficient of haze measurements in California: 1967-2003. Atmos. Environ., 42(3): 480-491.

Knox, A.; Evans, G.J.; Brook, J.R.; Yao, X.; Jeong, C.H.; Godri, K.J.; Sabaliauskas, K.; Slowik, J.G. (2009). Mass absorption cross section of ambient black carbon aerosol in relation to chemical age. Aerosol Sci. Technol., 43(6): 522-532.

Koven, C.D.; Fung, I. (2006). Inferring dust composition from wavelength-dependent absorption in Aerosol Robotic Network (AERONET) data. J. Geophys. Res. -Atmospheres, 111(D14)

Krecl, P.; Strom, J.; Johansson, C. (2007). Carbon content of atmospheric aerosols in a residential area during the wood combustion season in Sweden. Atmos. Environ., 41(33): 6974-6985.

Lack, D.A.; Cappa, C.D.; Covert, D.S.; Baynard, T.; Massoli, P.; Sierau, B.; Bates, T.S.; Quinn, P.K.; Lovejoy, E.R.; Ravishankara, A.R. (2008). Bias in filter-based aerosol light absorption measurements due to organic aerosol loading: Evidence from ambient measurements. Aerosol Sci. Technol., 42(12): 1033-1041.

Lee, H.M.; Okuyama, K.; Mizohata, A.; Kim, T.O.; Koyama, H. (2007). Fabrication of reference filter for measurements of EC (elemental carbon) and OC (organic carbon) in aerosol particles. Aerosol Sci. Technol., 41(3): 284-294.

Liousse, C.; Cachier, H.; Jennings, S.G. (1993). Optical and thermal measurements of black carbon aerosol content in different environments: Variation of the specific attenuation cross-section, sigma (□). Atmos. Environ., 27A(8): 1203-1211.

Mallet, M.; Roger, J.C.; Despiau, S.; Putaud, J.P.; Dubovik, O. (2004). A study of the mixing state of black carbon in urban zone. J. Geophys. Res., 109(D4): D04202. doi:10.1029/2003JD003940.

McMeeking, G.R.; Kreidenweis, S.M.; Carrico, C.M.; Collett, J.L., Jr.; Day, D.E.; Malm, W.C. (2005). Observations of smoke-influenced aerosol during the Yosemite Aerosol Characterization Study: 2. Aerosol scattering and absorbing properties. J. Geophys. Res. -Atmospheres, 110(D18)

Minoura, H.; Takahashi, K.; Chow, J.C.; Watson, J.G. (2006). Multi-year trend in fine and coarse particle mass, carbon, and ions in downtown Tokyo, Japan. Atmos. Environ., 40(14): 2478-2487.

Miyazaki, Y.; Kondo, Y.; Sahu, L.K.; Imaru, J.; Fukushima, N.; Kano, M. (2008). Performance of a newly designed continuous soot monitoring system (COSMOS). J. Environ. Monit., 10(10): 1195-1201.

Moteki, N.; Kondo, Y. (2007). Effects of mixing state on black carbon measurements by laser-induced incandescence. Aerosol Sci. Technol., 41(4): 398-417.

Moteki, N.; Kondo, Y. (2010). Dependence of laser-induced incandescence on physical properties of black carbon aerosols: Measurements and theoretical interpretation. Aerosol Sci. Technol., 44(8): 663-675.

Muller, T.; Henzing, J.S.; de Leeuw, G.; Wiedensohler, A.; Alastuey, A.; Angelov, H.; Bizjak, M.; Coen, M.C.; Engstrom, J.E.; Gruening, C.; Hillamo, R.; Hoffer, A.; Imre, K.; Ivanow, P.; Jennings, G.; Sun, J.Y.; Kalivitis, N.; Karlsson, H.; Komppula, M.; Laj, P.; Li, S.M.; Lunder, C.; Marinoni, A.; dos Santos, S.M.; Moerman, M.; Nowak, A.; Ogren, J.A.; Petzold, A.; Pichon, J.M.; Rodriquez, S.; Sharma, S.; Sheridan, P.J.; Teinila, K.; Tuch, T.; Viana, M.; Virkkula, A.; Weingartner, E.; Wilhelm, R.; Wang, Y.Q. (2011). Characterization and intercomparison of aerosol absorption photometers: result of two intercomparison workshops. Atmospheric Measurement Techniques, 4(2): 245-268.

Murphy, D.M.; Chow, J.C.; Leibensperger, E.M.; Malm, W.C.; Pitchford, M.L.; Schichtel, B.A.; Watson, J.G.; White, W.H. (2011). Decreases in elemental carbon and fine particle mass in the United States. Atmos. Chem. Phys. Discuss., 11: 2057-2076.

Myhre, G.; Berglen, T.F.; Johnsrud, M.; Hoyle, C.R.; Berntsen, T.K.; Christopher, S.A.; Fahey, D.W.; Isaksen, I.S.A.; Jones, T.A.; Kahn, R.A.; Loeb, N.; Quinn, P.; Remer, L.; Schwarz, J.P.; Yttri, K.E. (2009). Modelled radiative forcing of the direct aerosol effect with multi-observation evaluation. Atmos. Chem. Phys., 9(4): 1365-1392.

NIOSH (1999). Method 5040 Issue 3 (Interim): Elemental carbon (diesel exhaust). In NIOSH Manual of Analytical Methods, 4th; National Institute of Occupational Safety and Health: Cincinnati, OH. http://www.cdc.gov/niosh/pdfs/5040.pdf

Niu, C.; He, F. (2010). Comparison of indoor, outdoor, and personal EC and OC levels in Tianjin, China. In Proceedings, Leapfrogging Opportunities for Air Quality Improvement, Chow, J. C., Watson, J. G., Cao, J. J., Eds.; Air & Waste Management Association: Pittsburgh, PA, 631-635

Nordmann, S.; Birmili, W.; Weinhold, K.; Wiedensohler, A.; Mertes, S.; Muller, K.; Gnauk, T.; Herrmann, H.; Pitz, M.; Cyrys, J.; Flentje, H.; Ries, L.; Wirtz, K. (2009). Atmospheric aerosol measurements in the German Ultrafine Aerosol Network (GUAN) Part 2: Comparison of measurements techniques for graphitic, light-absorbing, and elemental carbon, and non-volatile particle volume under field conditions. Gefahrstoffe Reinhaltung der Luft, 69(11-12): 469-474.

Ogren, J.A.; Sheridan, P.J.; Andrews, E.; Jefferson, A.; Heintzenberg, J.; Mertes, S. (2001). Measurements of the light absorption efficiency of graphitic carbon in Indian Ocean aerosols. J. Aerosol Sci., 32(1001): 677-688.

Paredes-Miranda, G.; Arnott, W.P.; Jimenez, J.L.; Aiken, A.C.; Gaffney, J.S.; Marley, N.A. (2009). Primary and secondary contributions to aerosol light scattering and absorption in Mexico City during the MILAGRO 2006 campaign. Atmos. Chem. Phys., 9(11): 3721-3730.

Park, S.S.; Hansen, A.D.A.; Cho, S.Y. (2010). Measurement of real time black carbon for investigating spot loading effects of Aethalometer data. Atmos. Environ., 44(11): 1449-1455.

Peterson, M.R.; Richards, M.H. (2002). Thermal-optical-transmittance analysis for organic, elemental, carbonate, total carbon, and OCX2 in PM2.5 by the EPA/NIOSH method. In Proceedings, Symposium on Air Quality Measurement Methods and Technology-2002, Winegar, E. D., Tropp, R. J., Eds.; Air & Waste Management Association: Pittsburgh, PA, 83-1-83-19

Pitchford, M.L.; Malm, W.C.; Schichtel, B.A.; Kumar, N.K.; Lowenthal, D.H.; Hand, J.L. (2007). Revised algorithm for estimating light extinction from IMPROVE particle speciation data. J. Air Waste Manage. Assoc., 57(11): 1326-1336.

Poot, A.; Quik, J.T.K.; Veld, H.; Koelmans, A.A. (2009). Quantification methods of black carbon: Comparison of Rock-Eval analysis with traditional methods. Journal of Chromatography A, 1216(3): 613-622.
Quincey, P.; Butterfield, D.; Green, D.; Coyle, M.; Cape, C.N. (2009). An evaluation of measurement methods for organic, elemental and black carbon in ambient air monitoring sites. Atmos. Environ., 43(32): 5085-5091.
Quincey, P.G. (2007). A relationship between Black Smoke index and black carbon concentration. Atmos. Environ., 41: 7964-7968.
Ram, K.; Sarin, M.M. (2009). Absorption coefficient and site-specific mass absorption efficiency of elemental carbon in aerosols over urban, rural, and high-altitude sites in India. Environ. Sci. Technol., 43(21): 8233-8239.
Ramana, M.V.; Ramanathan, V.; Feng, Y.; Yoon, S.C.; Kim, S.W.; Carmichael, G.R.; Schauer, J.J. (2010). Nature Geoscience 3(8): 542-545.
Reisinger, P.; Wonaschutz, A.; Hitzenberger, R.; Petzold, A.; Bauer, H.; Jankowski, N.; Puxbaum, H.; Chi, X.; Maenhaut, W. (2008). Intercomparison of measurement techniques for black or elemental carbon under urban background conditions in wintertime: Influence of biomass combustion. Environ. Sci. Technol., 42(3): 884-889.
Rosen, H.; Novakov, T. (1983). Optical transmission through aerosol deposits on diffusely reflective filters: A method for measuring the absorbing component of aerosol particles. Appl. Opt., 22(9): 1265-1267.
Sakerin, S.M.; Smirnov, A.; Kabanov, D.M.; Pol'kin, V.V.; Panchenko, M.V.; Holben, B.N.; Kopelevich, O.V. (2007). Aerosol optical and microphysical properties over the Atlantic Ocean during the 19th cruise of the Research Vessel Akademik Sergey Vavilov. J. Geophys. Res. -Atmospheres, 112(D10)
Sato, M.; Hansen, J.; Koch, D.; Lacis, A.; Ruedy, R.; Dubovik, O.; Holben, B.; Chin, M.; Novakov, T. (2003). Global atmospheric black carbon inferred from AERONET. Proceedings of the National Academy of Sciences of the United States of America, 100(11): 6319-6324.
Schaap, M.; van der Gon, H.A.C.D. (2007). On the variability of black smoke and carbonaceous aerosols in the Netherlands. Atmos. Environ., 41(28): 5908-5920.
Schmid, H.P.; Laskus, L.; Abraham, H.J.; Baltensperger, U.; Lavanchy, V.M.H.; Bizjak, M.; Burba, P.; Cachier, H.; Crow, D.; Chow, J.C.; Gnauk, T.; Even, A.; ten Brink, H.M.; Giesen, K.P.; Hitzenberger, R.; Hueglin, C.; Maenhaut, W.; Pio, C.A.; Puttock, J.; Putaud, J.P.; Toom-Sauntry, D.; Puxbaum, H. (2001). Results of the "Carbon Conference" international aerosol carbon round robin test: Stage 1. Atmos. Environ., 35(12): 2111-2121.
Schuster, G.L.; Dubovik, O.; Holben, B.N.; Clothiaux, E.E. (2005). Inferring black carbon content and specific absorption from Aerosol Robotic Network (AERONET) aerosol retrievals. J. Geophys. Res. -Atmospheres, 110(D10)ISI:000227066300001.
Sedlacek, A.J.; Lee, J. (2007). Photothermal interferometric aerosol absorption spectrometry. Aerosol Sci. Technol., 41(12): 1089-1101.
Slowik, J.G.; Cross, E.S.; Han, J.H.; Davidovits, P.; Onasch, T.B.; Jayne, J.T.; Williams, L.R.; Canagaratna, M.R.; Worsnop, D.R.; Chakrabarty, R.K.; Moosmüller, H.; Arnott, W.P.; Schwarz, J.P.; Gao, R.S.; Fahey, D.W.; Kok, G.L.; Petzold, A. (2007). An inter-comparison of instruments measuring black carbon content of soot particles. Aerosol Sci. Technol., 41(3): 295-314.

Snyder, D.C.; Schauer, J.J. (2007). An inter-comparison of two black carbon aerosol instruments and a semi-continuous elemental carbon instrument in the urban environment. Aerosol Sci. Technol., 41(5): 463-474.

Subramanian, R.; Kok, G.L.; Baumgardner, D.; Clarke, A.; Shinozuka, Y.; Campos, T.L.; Heizer, C.G.; Stephens, B.B.; de Foy, B.; Voss, P.B.; Zaveri, R.A. (2010). Black carbon over Mexico: The effect of atmospheric transport on mixing state, mass absorption cross-section, and BC/CO ratios. Atmos. Chem. Phys., 10(1): 219-237.

Taha, G.; Box, G.P.; Cohen, D.A.; Stelcer, E. (2007). Black carbon measurement using laser integrating plate method. Aerosol Sci. Technol., 41(3): 266-276.

U.S.EPA (2003). Guidance for tracking progress under the Regional Haze Rule. Report Number EPA-454/B-03-004; prepared by U.S. Environmental Protection Agency, Reserach Triangle Park, NC, http://www.epa.gov/ttn/oarpg/t1/memoranda/rh_tpurhr_gd.pdf

U.S.EPA (2011). Chemical speciation. prepared by U.S. Environmental Protection Agency, Research Triangle Park, NC, http://www.epa.gov/ttn/amtic/speciepg.html

Viana, M.; Maenhaut, W.; ten Brink, H.M.; Chi, X.; Weijers, E.; Querol, X.; Alastuey, A.; Mikuska, P.; Vecera, Z. (2007). Comparative analysis of organic and elemental carbon concentrations in carbonaceous aerosols in three European cities. Atmos. Environ., 41(28): 5972-5983.

VIEWS (2010). Visibility Information Exchange Web System. prepared by Colorado State University, Ft. Collins, CO, http://vista.cira.colostate.edu/views/

VIEWS (2011). Visibility Information Exchange Web System. prepared by Colorado State University, Ft. Collins, CO, http://vista.cira.colostate.edu/views/

Wagner, R.; Linke, C.; Naumann, K.H.; Schnaiter, M.; Vragel, M.; Gangl, M.; Horvath, H. (2009). A review of optical measurements at the aerosol and cloud chamber AIDA. J. Quant. Spectrosc. Radiat. Transfer, 110(11): 930-949.

Wallen, A.; Liden, G.; Hansson, H.C. (2010). Measured elemental carbon by thermo-optical transmittance analysis in water-soluble extracts from diesel exhaust, woodsmoke, and ambient particulate samples. Journal of Occupational and Environmental Hygiene, 7(1): 35-45.

Watson, J.G. (2002). Visibility: Science and regulation - 2002 Critical Review. J. Air Waste Manage. Assoc., 52(6): 628-713.

Watson, J.G.; Chow, J.C.; Chen, L.-W.A. (2005). Summary of organic and elemental carbon/black carbon analysis methods and intercomparisons. AAQR, 5(1): 65-102. http://aaqr.org/.

Widmann, J.F.; Duchez, J.; Yang, J.C.; Conny, J.M.; Mulholland, G.W. (2005). Measurement of the optical extinction coefficient of combustion-generated aerosol. J. Aerosol Sci., 36(2): 283-289.

Wonaschutz, A.; Hitzenberger, R.; Bauer, H.; Pouresmaeil, P.; Klatzer, B.; Caseiro, A.; Puxbaum, H. (2009). Application of the integrating sphere method to separate the contributions of brown and black carbon in atmospheric aerosols. Environ. Sci. Technol., 43(4): 1141-1146.

Zencak, Z.; Elmquist, M.; Gustafsson, O. (2007). Quantification and radiocarbon source apportionment of black carbon in atmospheric aerosols using the CTO-375 method. Atmos. Environ., 41: 7895-7906.

Ackerman, A.S., O.B. Toon, D.E. Stevens, A.J. Heymsfield, V. Ramanathan, and E.J. Welton, 2000. Reduction of tropical cloudiness by soot. *Science* 288:1042-1047, doi:10.1126/science.288.5468.1042.

Alley, R. B. J. Marotzke, W. D. Nordhaus, J. T. Overpeck, D. M. Peteet, R. A. Pielke Jr., R. T. Pierrehumbert, P. B. Rhines,T. F. Stocker, L. D. Talley, and J. M. Wallace. 2003. Abrupt Climate Change. *Science* 299:2005-2010.

Amiro, B.D., A. Cantin, M.D. Flannigan, and W.J. de Groot. 2009. Future emissions from Canadian boreal forest fires. Can. J. For. Res. 39: 383-395.

Arctic Council. 2009. Arctic Marine Shipping Assessment 2009 Report. Arctic Council: Tromsa, Norway, 194p.

Bell M.L., F. Dominici, K. Ebisu, S.L. Zeger and J.M. Samet. 2007. Spatial and temporal variation in $PM_{2.5}$ chemical composition in the United States for health effects studies. *Environmental Health Perspectives* 115(7):989-995.

Bell M.L., D.L. Davis, L.A. Cifuentes, A.J. Krupnick, R.D. Morgenstern and G.D. Thurston. 2008. Ancillary human health benefits of improved air quality resulting from climate change mitigation. *Environmental Health* 7:41.

Berntsen, T. and J.S. Fuglestvedt. 2008. Global temperature responses to current emissions from the transport sectors. *Proceedings of the National Academy of Sciences (PNAS)* 105(49):19154-19159.

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

Betts, A. K. and M. A. F. Silva Dias. 2010. Progress in understanding land-surface-atmosphere coupling from LBA research. *J. Adv. Model. Earth Syst*., Vol. 2, Art. #6, doi:10.3894/JAMES.2010.2.6

Canadell JG, Le Quéré C, Raupach MR, Field CB, Buitenhuis ET, Ciais P, Conway TJ, Gillett NP, Houghton RA, Marland G. 2007. Contributions to accelerating atmospheric CO2 growth from economic activity, carbon intensity, and efficiency of natural sinks. Proc Natl Acad Sci U S A. Nov 20;104(47):18866-70. Epub 2007 Oct 25.

Chow, J.C., J.G. Watson, L.C. Pritchett, W.R. Pierson, C.A. Frazier, and R.G. Purcell. 1993. The DRI Thermal/Optical Reflectance carbon analysis system: Description, evaluation and applications in U.S. air quality studies. *Atmos. Environ*. 27A(8):1185-1201.

Chow, J.C., J.G. Watson, P. Doraiswamy, L.-W.A. Chen, D.A. Sodeman, S.S.H. Ho, R.J. Tropp, S.D. Kohl, D.L. Trimble, and K.K. Fung. 2006. Climate change - Characterization of

black carbon and organic carbon air pollution emissions and evaluation of measurement methods, Phase I. Report Number DRI 04-307; prepared by Desert Research Institute, Reno, NV, for California Air Resources Board, Sacramento, CA; http://www.arb.ca.gov/research/apr/past/04-307_v1.pdf

Chow, J.C., J.G. Watson, L.-W. A. Chen, M.C.O. Chang, N.F. Robinson, D. Trimble and S.D. Kohl. 2007. The IMPROVE_A temperature protocol for thermal/optical carbon analysis: Maintaining consistency with a long-term database. *J. Air Waste Manage. Assoc*. 57(9): 1014-1023.

Chow, J.C., J.G. Watson, J. Robles, X.L. Wang, L.-W.A. Chen, D.L. Trimble, S.D. Kohl, S.D., R.J. Tropp, K.K. Fung. 2011. Quality control and quality assurance for thermal/optical analysis of aerosol samples for organic and elemental carbon. *Analytical and Bioanalytical Chemistry*, in press.

Cohan, D. S., J. Xu, R. Greenwald, M. H. Bergin, and W. L. Chameides. 2002. Impact of atmospheric aerosol light scattering and absorption on terrestrial net primary productivity, *Global Biogeochem. Cycles* 16(4):1090, doi:10.1029/2001GB001441.

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

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

Fuglestvedt, J.S., K.P. Shine, T. Berntsena, J. Cook, D.S. Lee, A. Stenke, R.B. Skeie, G.J.M. Velders and I.A. Waitz. 2010. Transport Impacts on Atmosphere and Climate: Metrics. *Atmospheric Environment* 44:4648-4677.

Ganten et al. Lancet 2010

Greenwald, R., M.H. Bergin, J. Xu, D. Cohan, G. Hoogenboom, W.L. Chameides. 2006. The influence of aerosols on crop production: A study using the CERES crop model, *Agricultural Systems*, 89: 2-3, 390-413, doi: 10.1016/j.agsy.2005.10.004.

Hansen, J., M. Sato, and R. Ruedy. 1997. Radiative forcing and climate response, *J. Geophys. Res.* 102:683106864, doi:10.1029/96JD03436. (CHECK)

Huang et al. American Journal of Preventative Medicine 2011

IPCC, 2007: Climate Change 2007: Impacts, Adaptation and Vulnerability. Contribution of Working Group II to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, M.L. Parry, O.F. Canziani, J.P. Palutikof, P.J. van der Linden and C.E. Hanson, Eds., Cambridge University Press, Cambridge, UK, 976pp.

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

Jacobson, M.Z. 2006. Effects of absorption by soot inclusions within clouds and precipitation on global climate. J. Phys. Chem. 110:6860-6873.

Jacobson, M.Z. 2010. Short-term effects of controlling fossil-fuel soot, biofuel soot and gases, and methane on climate, Arctic ice, and air pollution health. J. Geophys. Res. 115, D14209, doi:10.1029/2009JD013795.

Kozlowski, Theodore T. and Theodor Keller. 1966. Food Relations of Woody Plants. The Botanical Review 32 (4): 293-382.

Laird, D. A. 2008. The charcoal vision: A win-win-win scenario for simultaneously producing bioenergy, permanently sequestering carbon, while improving soil and water quality. Agron. J. 100:178Y181.

Lehmann, J., J. Gaunt, and M. Rondon. 2006. Bio-char sequestration in terrestrial ecosystemsYA review. Mitigation and Adaptation Strategies for Global Change. 11:395Y419.

Loh P., J. Sugerman-Brozan, S. Wiggins, D. Noiles and C. Archibald. 2002. From asthma to AirBeat: community-driven monitoring of fine particles and black carbon in Roxbury, Massachusetts. *Environ Health Perspect* 110(suppl 2):297-301.

Liu, Yongqiang, John Stanturf, and Scott Goodrick. 2010. Trends in global wildfire potential in a changing climate. Forest Ecol. Manag. 259:685-697.Andreae, M.O. and A. Gelencser. 2006. Black carbon or brown carbon? The nature of light-absorbing carbonaceous aerosols. Atmos. Chem. Phys. 6: 3131-3148.Kriegler, E., J. W. Hall, H. Held, R. Dawson and H. J. Schellnhuber. 2009. Imprecise probability assessment of tipping points in the climate system. Proceedings of the National Academy of Sciences 106 (13):5041-5046.

Li et al. Science of the Total Environment 2011,

Manning, M. and A. Reisinger, 2011: Broader perspectives for comparing different greenhouse gases. *Proceedings of the Royal Society A*, http://rsta.royalsocietypublishing.org/cgi/reprint/369/1943/1891?ijkey=5Dizkisc8u2XNal&keytype=ref

Matsui, T., A. Beltrán-Przekurat, D. Niyogi, R. A. Pielke Sr., and M. Coughenour. 2008. Aerosol light scattering effect on terrestrial plant productivity and energy fluxes over the eastern United States. J. Geophys. Res., 113, D14S14, doi:10.1029/2007JD009658.

Moosmüller, H. et al. 2009. Aerosol light absorption and its measurement: A review. Journal of Quantitative Spectroscopy & Radiative Transfer 110:844–878.

Comment [S8]: Get full reference

Norway, Sweden and the United States. 2010. 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.

Oliveira, P. H. F., Artaxo, P., Pires, C., De Lucca, S., Procopio, A., Holben, B., Schafer, J., Cardoso, L. F., Wofsy, S. C. and Rocha, H. R. 2007. The effects of biomass burning aerosols and clouds on the CO2 flux in Amazonia. *Tellus B* 59:338–349. doi: 10.1111/j.1600-0889.2007.00270.x

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

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

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.

Schmid, H.P., L. Laskus, H.J. Abraham, H.J., U. Baltensperger, V.M.H. Lavanchy, M. Bizjak, P. Burba, H. Cachier, D. Crow, J.C. Chow, T. Gnauk, A. Even, H.M. ten Brink, K.P. Giesen, R. Hitzenberger, C. Hueglin, W. Maenhaut, C.A. Pio, J. Puttock, J.P. Putaud, D. Toom-Sauntry, and H. Puxbaum. 2001. Results of the "Carbon Conference" international aerosol carbon round robin test: Stage 1. *Atmos. Environ*. 35(12): 2111-2121.

Shine K.P., T.K. Berntsen J.S. Fuglestvedt, R.B. Skeie and N. Stuber. 2007. Comparing the climate effect of emissions of short- and long-lived climate agents. *Phil. Trans. Royal. Soc., A: Mathematical Physical and Engineering Sciences* 365:1903-1914.

Stevens, G. 2010. Avoidable mortality—a tool for policy evaluation in developing countries? *Eur J Public Health* 20(3): 241-242. doi: 10.1093/eurpub/ckq051?).

Sun, H.L., L. Biedermann and T.C. Bond. 2007. Color of brown carbon: A model for ultraviolet and visible light absorption by organic carbon aerosol. *Geophys. Res. Lett.* 34(17) GET PAGE NUMBERS

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

Ten Hoeve, J.E., L.A. Remer, and M.Z. Jacobson. 2011. 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.

Thurston GD, K. Ito, T. Mar, W.F. Christensen, D.J. Eatough, R.C. Henry, et al. 2005. Workgroup report: workshop on source apportionment of particulate matter health

effects—intercomparison of results and implications. *Environ Health Perspect* 113:1768–1774.

U.S. Department of Transportation (U.S. DOT). 2010. *Transportation's Role in Reducing U.S. Greenhouse Gas Emissions: Report to Congress*. Available at http://ntl.bts.gov/lib/32000/32700/32779/DOT_Climate_Change_Report_-_April_2010_-_Volume_1_and_2.pdf (accessed May 2011).

U.S. Environmental Protection Agency (U.S. EPA). *Integrated Science Assessment for Particulate Matter (Final Report).* U.S. Environmental Protection Agency, Washington, DC, EPA/600/R-08/139F, 2009.

U.S. Environmental Protection Agency (U.S. EPA). *The Benefits and Costs of the Clean Air Act from 1990 to 2020.* U.S. Environmental Protection Agency, Office of Air and Radiation, Washington, DC. Available at http://www.epa.gov/cleanairactbenefits/feb11/fullreport.pdf.

Yamasoe, M, C. von Rando, A. Manz, J. Schafe, T. Eck, and B. Holben. 2005. Effect of smoke on the transmissivity of photosynthetically active radiation inside the canopy. *Atmos. Chem. Phys. Discuss*. 5:5909–5934.

APPENDIX A: Charge to the Panel

&&&&.

APPENDIX B: Technical Corrections

The Panel's advice and responses to the charge questions are contained in the body of this report. However, in the course of the review, the following technical errors were noted in the materials provided by the Agency. This is not intended to be an exhaustive list.

Executive Summary:
Chaper 2: Black Carbon Effects on Climate

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

Chapter 4: Emissions of Black Carbon

Source characterization

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.

Section 4.1: Lines 9-10, and elsewhere such as Section 4.3.2.2, Page 4-13, Line 18.The report needs some clarification regarding diesel engines; the ratio and mass of BC and OC varies by source. Rephrase to say: "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)."

Section 4.1, 14-17: Some conditional statements do not seem accurate or related (e.g., what makes the OC/BC ratio a determinant of the candidacy for mitigation? There were other specific comments regarding the scientific understanding of the formation of OC (OM) and its treatment in climate models.

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.

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. Table 4-2 is formatted poorly for the main report, and might be moved to an appendix.

Section 4.3.1, Page 4-2, Lines 28-29: Text should also provide information relevant to vertical distribution of emissions (implied but not explained in Section 4.5 on page 4-34, line 17). Where

possible, better discussion of size distributions could be part of the discussion of uncertainty in models.

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." 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).

Page 4-4, Figure 4-1. Correct 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.

Geographical Characterization

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.

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)).

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 40th parallel. Why do our northerly citizens drive their cars more but use less electricity and home heating fuel?

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

Section 4.1: Lines 9-10: add text to emphasize that OC:BC ratios vary by combustion type AND by fuel type, jointly; i.e., "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).* Also add "typically" to read: "Diesel sources *typically* have a low OC/BC ratiosince not all diesel engine sources have low OC/BC ratios (heavy-fueled diesels).

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*,

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.

Technology assessment and descriptions

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.

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).

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.

This is "counter" to conventional wisdom and perhaps counter to corporate memory within parts of EPA, so it bears noting for your readers. 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.

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.

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?

Accuracy and Uncertainties descriptions

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.*

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 and agricultural burning – to get a sense of what's potentially controllable and what's not.

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.

Table 4-5 and Table 4-5: 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".

Section 4.4, Page 4-31, Lines 11-13 (last paragraph): The discussion of uncertainty must also (somewhere) include discussion of model uncertainty – probably in multiple places including here (and Section 4.5). The model(s) greatly underestimate BC amount and optical properties compared to observations, especially in Asia. There is a deficiency in models regarding treatment of emission sources, sizes, optical representation or distribution from source to boundary layer and above.

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). There are now available higher resolution, more complete Arctic inventories 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."

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

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.

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

Editorial

Section 4.3.1, line 31: delete "how" in the sentence for grammar.

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.

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

Reinforced by Levy comment: -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.

Reinforced by Poirot comment: 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.

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.

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.

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)

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

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?
Page 4-9 and global: "data" are plural, so replace "this data" with "these data" and similar throughout report.
Table 4-5 and Table 4-5: Tables are NOT tables but pasted pictures. Replace and format.
Table 4-6 is NOT a table. Replace and reformat.
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. <u>Global replace</u> 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.
Table 4-3 is NOT a table. Replace and format.
Table 4-8 is NOT a table. Replace and format.
Figure 4-9, bottom panel: the Energy Power pie slice should be green. It is currently Red IN ERROR.
Ayala comment: Pg 4-26, line 13, 4th IPCC assessment (IPCC, 2001) is not listed in bibliography. 2001, should be the 3rd instead of 4th?
Ayala comment: 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.
Ayala comment: 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.
Hansen comments: 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.
Hansen comments: 4-5, Figure 4-2. Explain the percentage ranges in the parentheses. Reference footnote 4 in the caption.

> Reinforced by Poirot comment: 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 PM2.5 emissions in the EPA inventory is composed of fugitive dust, but only a small fraction of this dust shows up in ambient PM2.5 measurements. BC and OC contribute XX% and YY% of the non-dust US PM2.5 emissions.

Hansen comments: 4-8,9 to 12. This really needs to include the heteroatoms in OM as "unidentified."
Hansen comments: 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.
Hansen comments: 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?

Hansen comments: 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.

Hansen comments: 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.

Hansen comments: 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.

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

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

Chapter 5: Observational Data for Black Carbon

- 5-3, 3-4: The explanation of the difference between OC and EC is too brief given the variety of temperature protocols used; and the term "phase" is ambiguous here. Be a bit more precise as to what is meant by phase. An updated table, such as Table 1 from Watson et al. (2005),should be added to Appendix 1 to demonstrate the range of thermal methods used and the variables that might be important. Table B-1 of Chow et al. (2006) provides a more detailed explanation of the most commonly applied methods and the studies in which they have been used. The difference between the TOT values in Schmid et al. (2001) shows how the treatment of the different variables can affect the EC concentration.
- 5-3,11. Change to "EC to estimate PM light absorption through an appropriately selected mass absorption efficiency." The U.S. default value for this is efficiency is 10 m2/g, as codified in implementation of the Regional Haze Rule (Pitchford et al., 2007; U.S.EPA, 2003; Watson, 2002), as derived from comparisons of Teflon filter transmittance measurements (Campbell et al., 1995) with corresponding quartz filter EC measurements (Chow et al., 1993; 2007). The mass absorption efficiency is a key concept relating EC/BC to its absorbing properties, and clarifications like this need to be made throughout the document.
- 5-3,16. "light transmitted through or reflected from a filter…"
- 5-3,20. "Several studies" would be better replaced with references to the studies. A search on "several studies" should be done for the entire document to be more specific.
- 5-3,21-23. There are several types of filter transmission methods and corrections (Bond et al., 1999; Chen et al., 2004; Collaud et al., 2010; Jimenez et al., 2007; Park et al., 2010). This might be better dealt with in Appendix 1 as it seems more detailed than the rest of the discussion.
- 5-3, 32-34. "absorption" not "adsorption." There's no reference for the ±30% difference, and the weight of evidence shows that it can be much larger. Watson et al. (2005) observe that one can (and that many authors do) cherry-pick among comparison studies to demonstrate good or poor comparability between collocated measurements.

- 5-3, last paragraph: Should add that 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.
- 5-2: Footnotes in Table 5-1 are missing.
- 5-4,4-5. Delete the first sentence. Not clear what is meant by "limited."
- 5-4,12-13. What is the criteria for inclusion of a network? It should include the public availability of the information.
- 5-5. It would be worthwhile to have a method code of some type (i.e., thermal protocol, aethalometer, PSAP, etc.) in the legend next to the network name and defined in the caption.
- 5-6,3-4. IMPROVE started in 1987 and CSN started in 2001. VIEWS (2011) has information on the start and end dates for each site.
- 5-6, 9: Please clarify what this set of 45 monitors refers to here. If aethalometers and PSAPs are meant, at least refer to them as "continuous" or "semi-continuous".
- 5-6, Footnote 1. The STN method used in CSN is well-described by Peterson and Richards (Peterson and Richards, 2002) while the IMPROVE and its relation to the STN methods are described by Chow et al. (Chow et al., 1993; 2001; 2004; 2007). There is no description of the temperature protocol in NIOSH (1999), just an idealized figure from which one must estimate temperatures by assuming a vertical axis label and measuring graph heights with a ruler.
- 5-6, footnote 4: why not the IMPROVE data "after 2005" rather than "2005-2007". Did the method change again in 2008?
- 5-6, footnote 5: This note is cryptic. The explanation in Chapter 4 does not discuss this in the same way as suggested in the note.
- 5-7, Check alignment of labels and values of row of "global background" information in Table 5-2.
- 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 Quincey (2007) 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 collocated aethalometer.
- 5-11, Figure 5-3: 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 smoke stain 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 µg/m3, plus, the key did not reproduce clearly, so is mostly unreadable
- 5-11, 10: It is interesting to note that the actual gradients may have been even sharper than those revealed by the 150 monitors.
- 5-12, footnote 10: Has LAC been defined?
- 5-13, Fig 5-5a: Scales on axes are not legible
- Isn't using cleaner fuels part of fuel switching?
- 5-13, 13 to 15. The comparison in Figure 5-5b is 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. Can this be explained?

- 5-14, 17-18: The reference should be to the Bahadur et al. (2011). However, perhaps this last sentence should be deleted, 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".
- 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. Note also that although there is an indication that there is an increase due to change of instrumentation, then in 5-17, 10 there is a note about a potential negative inflection due to instrument change. Appendix A1-8-17/22 suggests there should be little change. Need to make clear and consistent.
- 5-16, 1 and Figure 5-8: It is not clear here whether it means BC concentrations in "San Francisco Bay" or in air above the "San Francisco Bay Area". I assume that it means Bay Area, but it should be clarified. Also, the reported units on the Y scale (mg m-3) should probably be µg m-3.
- 5-17, Figure 5-9: Aethalometer should be capitalized in caption. Clarify whether these data have been corrected for various artifacts, such as identified by Virkkula et al. (2006) and Cohen et al. (2010). The caption should explain the averaging basis for the red line.
- 5-17, 10-11. The lack of trends being observed is not due to the change from TOT to TOR EC analysis at CSN sites. TOR nearly always measures more EC than TOT, mostly owing to the charring of adsorbed organic vapors within the filter that are detected by the transmitted light but not by the reflected light. This charred portion evolves after the EC in the PM deposit on the filter surface, thereby enhancing the value of the pyrolysis correction (Chen et al., 2004; Chow et al., 2004).
- 5-18, 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.
- 5-18, 13-15: The rates of decrease given for Alert (-2.1 ng/m3 yr) and for Zeppelin (-1.4 ng/m3 yr) do not agree with the slopes of the lines in Figure 5-11, which are -3.5 ng/m3 yr and -5 ng/m3 yr, respectively. Also, please clarify how 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, it is difficult to interpret what is written and, secondly, what is the point of plotting horizontal lines on a time series that shows a downward trend?
- 5-19, 10-16. Some of the sun photometer work specific to black carbon should be cited (Arola et al., 2011; Dey et al., 2006; Koven and Fung, 2006; Mallet et al., 2004; Myhre et al., 2009; Sakerin et al., 2007; Sato et al., 2003; Schuster et al., 2005). Appendix 1 needs a brief explanation of how BC and mass absorption efficiencies are derived from spectral data, the assumptions involved, and some of the uncertainties.
- 5-19, line 18: The sentence should be clarified. Satellites can be very helpful in identifying spatial patterns, but it's questionable how well they can quantify surface BC concentrations.

- 5-20, 11: 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. The pattern of the points frankly looks like a shotgun blast. For a precise description of the agreement, why not give the correlation coefficient and coefficient of variation?
- 5-21, 9: What is the date of the Russell citation?
- 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, 20-21: The ratio is approximately 6, not 10, during the summer in Figure 5-14. 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?
- 5-24, 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.
- 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, Fig 5-17: Please clarify what the blue bars at top of figure represent relative to black ones or how to interpret.
- 5-28, 18- 1:. 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."
- 5-30, Fig. 5-19 doesn't 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.
- 5-32, 3-4 and Figure 5-21: It is 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.
- 5-31, 4-5: There is no such Figure 5-22. The text should refer to Fig 5-20.
- 5-31, Figure 5-20 and caption: The "gray shaded region (between the black and blue dotted lines)" in the top figure is indiscernible.
- 5-32, Figure 5-21 caption: More explanation should be given of what the box and stem plots represent.

Chapter 6: Mitigation Overview

Chapter 7: Mitigation Approaches for Mobile Sources

Chapter 8: Mitigation Approaches for Stationary Sources

Chapter 9: Mitigation Approaches for Residential Heating and Cooling

Chapter 10:

Chapter 11: Metrics for Comparing BC Impacts to Impacts of Other Climate Forcers

- Page 11-11, line 27: IPPC → IPCC
- The correct full name of the GTP is Global Temperature change Potential. “change” is missing.
- Fuglestvedt is misspelled in several places; e.g. page 11-9, line 2 and 7.
- Page 11-4, Table 11-1 Explain "energy" as the climate impact in a Table footnote.
- Page 11-4, line 22: Is STRE developed specifically for short lived climate forcers?
- Page 11-5, line 18: More references could be added here.

  Page 11-9, line 14: “equivalent” may be deleted.
- Page 11-14, line 29: The title could be improved. The whole chapter discusses and incorporates considerations of lifetime.
- Page 11-14, line 35-36: This may be illustrated by the use of GTP(t).
- Page 11-15, line 18-19: References could be added here; e.g. Rypdal et al. 2005; Jackson 2009, Fuglestvedt et al. 2000.