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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Using measurements for evaluation of black carbon modeling" *by* S. Gilardoni et al.

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1 General Comments

This paper addresses the difficult topic of validating modeled BC concentrations with measurements. This topic is difficult, in part, because we do not have great confidence in our ability to routinely measure BC at the present time. Additionally, most of the available BC measurements are obtained at the surface, so we are stuck with a spatial sampling mismatch (i.e., surface measurements are "points," and models provide concentrations in a grid). Additionally, the surface is a difficult region for models, as the boundary layer dynamics are more difficult to parameterize than the rest of the atmosphere. Unfortunately, the authors weren't able to offer any significant improvements to the present situation.

The paper is not easy to follow, in general. It is often difficult to see where the author is leading the reader. I suggest "lead-in" paragraphs at the beginning of each section, and "lead-in" sentences at the beginning of each paragraph. It is very important for the reader to know where he/she is going, as this tremendously aids comprehension. Otherwise, the reader is forced to backtrack for clarification, and most customers are not patient enough to do this for the length of a paper. Passive voice adds to the confusion; it is better to use an active voice.

The authors spend quite a bit of time in the intro explaining the problems associated with measuring black carbon, but then they do not solve or even address these problems in the paper. The intro is supposed to launch the rest of the paper, and placing this info in the intro leads the reader to believe that this paper will address this issue. I would move the first few paragraphs of the intro to the Method section, and begin the paper with what is presently on line 19 of page 4 ("BC modeling is a crucial component of. . . "). This focuses the reader on the true topic of the paper, which is an attempt at validation of modeled BC.

I strongly recommend against the term "equivalent BC." If the authors insist on using this terminology, they need to define "equivalent." The term "BC" already denotes an optical measurement, so what value does the adjective "equivalent" add to the definition?

The 6x4 degree grid is very large for comparisons to surface sites, and the authors arguments to justify the comparisons are not very strong. For instance, the authors state that Fig 2a indicates that the EBC trend at Alert is similar to other polar sites (which is true), but the magnitude of the monthly means can vary by a factor of 2-3 between sites. Also, on page 17, line 23, the authors even attribute the model/measurement discrepancy to grid size. Since grid size is always a potential factor in these comparisons, how can one possibly validate the model in these large grid cells?

The authors talk about the variability of measured values for σ^* (Figure 3 even includes

two assumptions for σ^*), but they do not talk about the modeled values for σ^*. Some comments about the modeled σ^* are important, as this directly effects the radiation field. The authors need to think about how internal mixing affects the specific absorption; internal mixtures with high BC fractions will have much lower specific absorption than internal mixtures with low BC fractions.

2 Specific Comments

There is something wrong with Eq 3 or Eq 4; these equations are nearly identical, with the exception of the '$*$'. Accepting both of these equations to be true requires $\sigma = \sigma^*$ and $C \times R = 1$. Also, I have never seen a form of the Lambert-Beer (or Bouguer) law that looks like Eq 3 — where is the attenuation? Is B_{abs} dimensionless, like B_{ATN}? No, that won't work. Is EBC a mass concentration or a number concentration? What are the dimensions of C and R? I suppose I could sort all of this out if I worked hard enough, but most readers don't want to work that hard (especially with the inconsistency of Eqs 3 & 4). This muddling of the equations causes additional confusion in the next paragraph. Eq 3 seems to indicate that σ is the intrinsic specific absorption of BC (i.e., without instrument artifacts), but the remainder of the paper discusses σ^*.

Are the aethalometer measurements "wet" or "dry?"

Starting on page 9, line 24, the authors state that the measured value of σ^* is 20 m2g-1, but that they follow the lead of Malm et al. (1994) and use 10m2g-1. Why? How is this justified?

Page 15, line 20: Trinidad Head should be a much cleaner location than Bondville, and is likely to have smaller fractions of BC in the internally mixed component. Lower BC fractions result in higher specific absorptions, which should also be mentioned in this paper. Also, the authors state that "the uncertainty of black carbon optical properties compromises the use of EBC measurements for model evaluation" at Trinidad Head

(page 15, line 23), yet they continue to use this site for the remainder of the paper.

Table 1 indicates that the authors are using different wavelengths for determining the BC at the different surface sites, sometimes as low as 370 nm; why not use the 880 nm wavelength for all of the analyses? This would minimize or eliminate the influence of dust and OC. Also, the authors indicate an aethalometer wavelength of 820 nm at Alert, but this is not one of the standard or alternative wavelengths listed in the aethelometer manual. Is this a custom-built instrument, or a typo?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11315, 2010.