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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mass concentrations of black carbon measured by four instruments in the middle of Central East China in June 2006" *by* Y. Kanaya et al.

Anonymous Referee #3

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General statement

This paper compares different measurements of black carbon at a single site in China. The measurements presented are valuable because measurement methods are known to yield different results even when applied to the same sample. Collocated measurements made under different conditions can provide a better understanding of how much and why the instruments differ. At present, we do not have enough studies of such concurrent measurements, so this paper is a useful contribution.

The paper has a nice level of detail. I found that most of my questions about instrumental methods were answered shortly after they occurred to me. The organization is

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good. I recommend publication after some revisions which I consider minor, although they could entail some substantial work.

Comments

I like the authors' statement, that large uncertainty is identified in the measurement, but that measurement is still useful to constrain the emission rate. However, I think that statement should be qualified. This single location doesn't constrain the emissions from all of China. Furthermore a comparison between measurement and model would also contain some model errors. I suggest that authors re-write that statement to more clearly identify what can be understood.

Some of the figures, especially the time series, are difficult to see. I am not sure what can be done about this. Possibly in Fig 3, the legends could be removed from inside the panels, and the high points could be cut off. Same comment for Fig 9.

There is a large number of acronyms in this paper. Although these are very well explained by the authors, this reader found it difficult to remember what each one stood for and had to search for the explanatory paragraphs many times. I suggest a table that contains the acronym, the instrument used to measure the quantity, and the principle of measurement.

Page 14958, line 19: suggest "explanations" instead of "possibilities"

Page 14959, line 19: "most significant region"– for what? What fraction of emissions occurs here?

Page 14961, line 2: "measurements with less uncertainty"– I suggest that this approach doesn't reduce uncertainty, rather just helps understand it

Page 14962, line 28, spell out SUS

Page 14963, description of heating PSAP sample. I understand the purpose of this heating-removing substance that could be coated on to the particles. But I do not

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understand it, physically. Certainly the PSAP sample is not made at 400 C, so a re-cooling occurs. Are we certain that all the volatile material evaporates or decomposes and never returns to the particle phase?

Page 14963, line 14, and 14964, line 28. Mass absorption cross section is given for PSAP and aethalometer. Authors should discuss how uncertain this factor is: it might affect whether comparison with other instruments looks high or low.

Page 14963, line 26. "IMPROVE program." IMPROVE program uses reflectance rather than transmittance for optical correction. Some literature suggests that this difference is important. Authors should make clear that the use of transmittance is unlike IMPROVE and hence, the comparison may not be exactly like the full IMPROVE method. The comparison given here is valuable, however, because it shows the effect of 550C temperature without the confounding factor of the different optical correction.

Page 14964, line 9, "change in transmittance... used to determine BC concentrations optically." What cross section was used and how did this compare with the other cross sections?

Section 3.2. NIOSH vs IMPROVE. This is a nice comparison, and I appreciate the contribution. However, it seems that there could be some confounding factors. The period when the NIOSH program operated has much different concentrations than the later period, when the IMPROVE operated. If the response depends on loading or concentration, this comparison is invalid. Further, the slope of the NIOSH line could be very much affected by the high points. I suggest that the authors also include regression statistics for the NIOSH and Opt-EC when limiting the Opt-EC concentration to 3 ug/m3.

Page 14968, lines 17-21. There is a relatively better agreement between MAAP and IMPROVE. However the disagreement (50%) is quite high and I don't think the 10% better agreement with the IMPROVE is important in light of this large disagreement.

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Section 3.5, differences (starting on line 12). These are really three explanations: the lensing, the cancellation by MAAP, and the split point. The MAAP instrument is supposed to cancel scattering but it does not account for the lensing effect, which is actually an increase of absorption.

Page 14970, line 15. Interesting observation of volatility observed with low BC/EC ratio.

Page 14970, line 23-26. This explanation is puzzling. Authors have suggested that thermal EC measured by IMPROVE and by NIOSH are different. This suggests that OC/EC ratio for these two should be slightly different. Then, how could MAAP_BC to EC ratio be the same for the two different measurements of EC? Also, I do not understand how this observation could be used to determine that the optical instruments are overestimating. Some clarification is needed.

Page 14971, lines 10-11. "Aging does not affect the determination of the split point between OC and EC." How can you be sure of that statement? I agree that an aging effect is a reasonable explanation, but I don't think that the other explanation is eliminated.

Page 14971, last paragraph (continues to 14972). This is a nice collection of studies, but I think the discussion should be sharpened. Also, not discussed, is the possible change in conversion of optical signal to mass (absorption cross-section in m2/gram). This is mentioned in the end of conclusions but should be discussed earlier. I think possibly this discussion would be more readable if the studies were summarized in a table showing instruments (for both BC and EC), ratio observed, and hypothesis advanced by the authors.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 14957, 2008.

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