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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evolution of multispectral aerosol optical properties in a biogenically-influenced urban environment during the CARES campaign" *by* M. Gyawali et al.

Anonymous Referee #2

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I believe that in this first response, the authors have failed to directly answer a number of Reviewer #1's (R1) questions.

First, R1 specifically states "There is no discussion on the uncertainty in PASS measurements and given a range of approximately 20% uncertainty in other similar measurements, this difference of 10% seems fairly unremarkable and within the uncertainty range of the instrument." Given this comment, I would have thought that the authors response might make some concrete statements about instrument uncertainty, which, as R1 notes, is not discussed. Unfortunately, the authors seem to avoid this question, instead giving some answer about the economic downturn with only a cursory men-

tion of uncertainties that does not answer the question. The authors absolutely did not present their "results in the context of instrument uncertainties." The instrumental description provides no discussion of absolute uncertainties on any of the measurements. Further, the error bars that are provided are not the appropriate error bars for consideration. For example, (and as I've noted in my review) the uncertainty determined from a linear fit to a plot of absorption vs. [rBC] is NOT the appropriate uncertainty to consider as this only says something about the precision of the measurements and the level to which the data conform to a linear fit. It provides no information about the absolute uncertainties involved and is not appropriate when measurements from different instruments are being compared between different sites. The minimum percent uncertainty in the MAC is actually the square root of the sum of the squares of the percent uncertainties for each of the measurements involved, which I imagine is actually wavelength dependent and may even be different at the two sites since two sets of independent instruments were used. I therefore strongly disagree with the authors contention that they "presented their results in the context of instrument uncertainties."

Second, the authors miss the entire point of R1's question regarding the use of different wavelength pairs for calculating AEA at T1 and T0. As the authors note just a few responses above, "The AEA of course does depend on wavelength choice." Given this, comparing AEA values that are derived at different wavelengths, and then aiming to draw conclusions based on differences between these AEA values, is not justifiable. Further, the authors must propagate their uncertainties to determine an absolute uncertainty in the AEA values. I will note that if the authors were simply looking at the variability in the AEA as measured at one site by one set of instruments, then it may be possible to identify differences/changes in the AEA that are smaller than the absolute propagated uncertainty because the measurements can then be looked at in a relative sense (which then depends on instrument precision and not accuracy). However, because they are comparing between multiple sites with independent instruments absolute uncertainties cannot be ignored and fit uncertainties are, for the most part, not a useful metric.

ACPD

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I make these additional comments here in the hopes that the authors are able to provide more direct answers to my questions and concerns raised in my review, as opposed to what I view as very cursory responses provided here for many of R1's most important points.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7113, 2013.