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Interactive comment on "Black carbon physical properties and mixing state in the European megacity Paris" *by* M. Laborde et al.

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Comment by Chris Cappa and Dan Lack

In the study of Laborde et al., absorption measurements were made using an Aethelometer at 880 nm, which is a filter based measurement technique. In reporting their mass absorption coefficient (MAC) results for different air mass types (traffic, biomass burning, aged, continental), they have assumed that the scattering "correction factor," C, that is needed to turn the measurement (I/I0 of light passed through the filter) into an absolute absorption coefficient is constant. They make this assumption because they do not have data available to suggest otherwise. We suggest that this is a reasonable assumption, but that the authors do not fully consider the uncertainty introduced by it. In Section 3.3.4 they report that the observed mean mass absorption coefficient ranges

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from 7.8 mˆ2/g (traffic) to 8.8 mˆ2/g (aged) at 880 nm, with MAC values for biomass burning and continental air masses in between. They go on to compare their campaign average 880 nm value with the literature estimate of the MAC at 550 nm, extrapolating using an Angstrom exponent of 1. They find that their extrapolated MAC is 13.6 m2/g, much higher than the literature value of 7.5 +/- 1.2 mˆ2/g from Bond and Bergstrom (2006). They offer two explanations: (1) that the scattering correction factor is incorrect or (2) that the Bond and Bergstrom results were only for "fresh" emissions. However, they do not compare their "fresh" (i.e. traffic) results to Bond and Bergstrom (2006), which we suggest is the more appropriate comparison since, as Laborde et al. state, Bond and Bergstrom "reported only freshly emitted BC MAC values". Extrapolating 7.8 mˆ2/g, the "fresh" value reported in this study at 880 nm, to 550 nm gives 12.5 mˆ2/g, which is also much higher than the literature value. Therefore, this strongly suggests that the reason for the larger mean MAC during this study is due to inadequacies in the scattering correction factor, which leads to an overestimate of (and large uncertainty in) the actual absorption at 880 nm. We strongly encourage the authors to compare both their mean values and traffic values to the Bond and Bergstrom results, as has been done above.

If the scattering correction factor, C, is indeed insufficient to provide an accurate estimate of the absolute absorption, which seems to be the case, one needs to further ask the question of to what extent is C constant in time and independent of the aerosol composition? Implicit in the authors' comparison of MAC values between different air mass types is the assumption that C is air mass independent. This assumption may not be justified since filter based absorption measurement methods have previously been shown to suffer, at times, from biases that depend on the composition and abundance of non-BC particle components (Lack et al., 2008; Cappa et al., 2008). Although these particular studies cited used PSAP instruments, and not Aethelometers, it is reasonable to think that both PSAP and Aethelometer instruments would suffer from similar biases given that both are filter based. Given that the reported mean MAC values for the different air mass types differed by only ∼15%, and given the large magnitude of

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the biases observed in Lack et al. (2008), it would seem that the changes reported here in the MAC between different air mass types are well within the actual uncertainties of the measurement technique. We suggest that the impacts of such potential time/air mass-dependent biases in the scattering correction (and thus the absorption measurement) be discussed in much more detail and that the discussion and conclusions be revised accordingly. We have concerns that no firm conclusions can be made regarding the atmospheric variability of the MAC once the measurement uncertainties associated with the scattering correction are considered.

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