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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Scattering and absorption by aerosols during EUCAARI-LONGREX: can airborne measurements and models agree?" *by* E. J. Highwood et al.

Anonymous Referee #1

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Scattering and absorption by aerosols during EUCAARI-LONGREX: can airborne measurements and models agree?

Highwood, E. J. et al., 2011

Review

The paper presents measurements of total aerosol mass and composition (AMS/SP2) in conjunction with optical measurements (dry/wet neph. and PSAP) from flights around Europe. These data were used to calculate: extinction (from measurement and model), SSA, AOD, f(RH), and MAE (measured/modeled) all at 550nm. The main conclusion of the paper – that correctly modeling absorption requires a narrow range

of index of refraction for organics — is poorly motivated because of the dominating uncertainties in the BC and absorption measurements: in fact the uncertainty is large enough to render all associated discussion about the bias moot. The underlying thrust of the paper is technical: to demonstrate the technical control over the airborne instrumentation necessary to achieve closure. Of itself this is of value in supporting other publications associated with the dataset, however the paper does not present the technical underpinings of the comparison strongly enough to achieve this: better connection to the ground-stations is required for absolute validation.

Presentation of the basic measurements would be sufficient to justify publication as a recent overview of aerosol characteristics in the European lower troposphere , however this would require a more in-depth presentation of the of the data. Thus the manuscript requires major revision in the form of more thorough discussion and analysis of the measurements and uncertainties associated with the closure results, and from further presentation of basic observables and context in both the figures and text.

General comments: The relative impact of BC on the conclusions and measurements here is never clearly presented. There are several comments about "BC's negligible impact" and "BC is a small fraction of total aerosol" that, when coupled with the comparison of the scale of Figures 7 and 8, lead me to believe that the BC measurement doesn't influence the model/measurement comparison at any level near the uncertainties in the scattering/extinction comparison. Thus I encourage the authors to more explicitly present the relative contribution of BC to the total aerosol mass loading and AOD.

Specific points:

Title: The title suggests a narrower treatment of data than presented. Please consider modifying the title to better represent this work.

Abstract: Please include uncertainties in the values presented in the abstract. Line 12-13. This is not accurately representing the findings of this paper; it is well known

that the refractive index is a crucial parameter in modeling absorption. The point here appears to be that to achieve agreement between model and measurement, the imaginary part of the index for organic aerosol is fixed to a narrow range of values. This is a major conclusion (at 550nm) that must be better defended. Please reword to more clearly present the findings of this manuscript to the reader. Line 16 “Ratio of scattering” The last sentence seems to be directed at the editor rather than the reader.

Introduction

Line 2 of 18491: The SP2 measures all aerosol that scatters sufficient 1064 nm light, and additionally quantifies BC mass in individual particles. I suggest the authors state something in this line: “. . .as the AMS data does not detect mineral dust or sea salt aerosol, and the SP2 was used only to detect BC. . . the analysis provided here is not likely to represent regions with high loadings of these undetected materials.”

Instrumentation: It is clear that the AMS, SP2, etc. have been documented at length in other papers, however, I suggest that the authors still include a brief description of the instrument parameters of interest here, especially those that bear on the discussion of the quality of the model/meas. Closure. In this vein:

Please include an estimate of the AMS lens efficiency with a comment about how well you expect the AMS to represent total aerosol mass for the size distributions observed in the data set.

State how completely the SP2 captured total BC mass in the accumulation mode – what was the correction (if applied) for non-detetcted BC mass in this mode? This is significant for later in the paper when the bias in measured/modeled absorption is discussed. The McConnell paper (cited to support use of Rosemont inlets) appears to indicate very large (i.e. factor 3) discrepancies in extinction measured on the BAe-146 and the DC8. This raises the fundamental question about sampling of ambient aerosol, for comparison to ground sites, and also about sampling from different inlets for the internal closure experiment. Please describe inlet placement on the aircraft

if the various instruments sampled from different inlets. The Lack et al,. 2008 does not suggest correcting PSAPS as a function of organic loadings, but merely points out the scale of possible artifacts when large loadings are present. To support conjecture about the possible artifact, please present the actual organic loadings in question.

Line 19 page 18493: The wing-tip to wing-tip comparison only provides relative accuracy for two instruments. Please explicitly link to absolute uncertainty.

Line 9, page 18494: please exclude biological aerosol too.

Section 3.1: please use the appropriate number of significant figures throughout the paper, and include uncertainties in stated values.

Section 3.2: Was the high SSA in the marine-boundary layer runs also associated with higher mass extinction efficiency? Was BC/total mass different? Rather than merely narrating the figure, it appears that the authors wish to explain the observations. Hence they should present a wider range of possibilities considered.

Line 11 page 18498: why not include this broader result in the abstract for higher visability?

Section 4: Line 4, page 18499: I do not understand what is meant by “a hydrophobic effect on aerosol.”

Line 5, 18500: I assume the nephs. were operated in parallel. How was the aerosol humidified in the wet nephalometer? Are losses due to the humidification responsible for the low bias in figure 6? What was the time scale over which humidity was varied?

Figures:

For readers who skip to the figures, I suggest adding the measurement wavelength to the captions for all relevant figures.

Figure 4 and discussion: it appears that only one ground station was below any vertical profiles. Given the relatively large variability in AOD spatially, what is the value of the

comparisons to ground stations not-co-located with the profile?

Figure 5: Please expand these profiles figures to include more information. I suggest including total aerosol mass and bc mass profiles. If there is space, it would not be unreasonable to include a tracer such as CO to give more context to the reader. I understand that these are only four out of many profiles that contributed to the whole paper, but feel that a little context is better than none.

Figures 6 and 7. Please comment on the discrepancy in f(RH) measured dry (<30%) RH) in figure 6: f(RH) should be 1 at matching RH, but is 1 at $\sim$ 50% (note that the uncertainty in RH in each neph. is ony 5% for non-extreme values). Earlier in the text there was a comment about correcting wet neph. data, but it is not clear that it's appropriate here, as only dry (<30% RH) air was selected. Please clarify the source of the disagreement, and state the range of error in RH measurement in the wet and dry portions of the neph measurement.

In figure 7 it is clear that the curve for flight 374 does approach 1 at low RH, but this is not consistent with the result of figure 6. The value at 90% also does not match.

Figure 7 indicates intra-flight variability, but I suggest including a second flight in figure 6 to show this variability in the un-averaged data. Thus I suggest including flight B367 in figure 6.

Figure 8: please enlarge the font used in the figure

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18487, 2011.