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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Black carbon concentrations and sources in the marine boundary layer of the tropical Atlantic Ocean using four methodologies" *by* K. Pohl et al.

Anonymous Referee #1

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This paper present measurements atmospheric black carbon within the Atlantic marine boundary layer, using a variety of offline measurement techniques. Two of the techniques are quite commonly used within the atmospheric science community but the others less so. By focusing on the comparison of the four techniques, the paper risks being more suited to AMT, however there are also atmospherically-relevant conclusions, regarding the spatial variability of the black carbon and also potential insights into the composition of black carbon that can be gained by comparing the techniques. The novelty of the technique and the fact that the measurements take place in data-poor regions make this relevant to ACP.

I do have a few comments, but I would overall consider this publishable if they are addressed. I therefore recommend that this is published subject to minor revisions.

General comments:

I found the terminology used within the paper a little hard to get used to, as not all of it was consistent with what is generally used within the atmospheric science community. Furthermore, there is a general move in the community to consign the term 'BC' to that which is measured using only the optical methods; most people would refer to that measured by evolved gas analysis as 'EC'. For these reasons, I would strongly urge the authors to modify their terminology to make as consistent as possible with the recommendations presented by Petzold et al. (2013, doi:10.5194/acp-13-8365-2013). I would certainly be in favour of denoting each measured quantity with a subscript to indicate which technique produced it.

As it currently stands, the abstract and conclusions seem to focus mainly on the technical aspects of the paper, i.e. the comparison of the four analysis methods. For the sake of making the paper more relevant to ACP, it would be useful to focus more on the atmospheric implications, specifically the spatial variabilities and the trends in the disagreements between measurements that may indicate a change in the physical nature of the BC.

While I do not dispute that the trends in the measured data are probably a reflection of a change in the physical properties, I do not think that the explanation relating to what the authors term 'charcoal' is adequately supported. Firstly, as the authors admit, large particles of incompletely-combusted fuel are not expected to participate in long range transport, owing to their faster settling velocity. But perhaps more importantly to the paper, other potential explanations for the deviations between the measurements are not explored. These could include (but it probably not limited to) changes in the degree of graphitisation, the presence of metals (e.g. potassium) interfering with the analysis, surface functionalization of the soot and the presence of macromolecular

organics (e.g. PAHs, humic-like substances). In addition, the optical transmittance measurement could be further confounded by variations in the mass attenuation cross section, the presence of 'brown' carbon and scattering artefacts on the filter. While the presence of charcoal could be an explanation, this should be presented as a speculative hypothesis rather than a supported conclusion if the other explanations cannot be discounted. The discussion should also seek to include other potential explanations, again if only in a speculative manner. In particular, I would like to see the possibility of the humic-like substances generally present in biomass burning aerosols being responsible to be discounted.

There are a number of instances within this paper where the authors do not demonstrate a good knowledge of the literature from the recent decade or so, in particular with regards global aerosol modelling. I have pointed out a few of these in the specific comments, but given the considerable advances in the field over the previous 10-15 years, I suggest the authors strive generally to provide more up-to-date references.

Specific comments:

Page 29787, line 10: A more recent review of atmospheric black carbon instrumentation should ideally be cited, as there have been major advances in some fields in recent years, in particular laser induced incandescence (LII).

Page 29791, line 25: The authors should clarify this. Are they saying glass fibre filters were used in error? Could this have affected any of the other measurements? Do the authors not have some other way of verifying what material was used for the filter?

Page 29793, line 1: According to this, a precombusted sand blank produced no signal using the CTO-375 method, but earlier in the section, it is stated that this technique had nonzero blanks. Does this mean that the other blanks referred to were not precombusted? If so, this is a problem because the filters used during sampling were precombusted, so the blanks should have been subjected to the same treatment. Or is it that the sand blank produced no signal over the normal blank? Whichever way, this

should be clarified.

Page 29793, line 9 (and elsewhere): Using a reference from 1996 for modelled organic concentrations does not seem appropriate, as in-use models of secondary organic aerosols have changed beyond all recognition in the intervening time. The authors should obtain a more up-to-date estimate.

Page 29793, line 8: Rather than use a qualitative description of the colour, what was the quantitative Angstrom exponent by the optical transmission method?

Page 29795, line 21: There is a wealth of material in the literature on BC in South America, in particular looking at biomass burning in Brazil (c.f. the work of P. Artaxo and coworkers). This is unless they are referring to the region of the Atlantic, in which case they should specifically refer to this.

Page 29795, line 24: Again, the use of out-of-date modelling studies does not seem relevant, as model representations of BC have greatly improved in recent years. The authors should strive to use a more up-to-date estimate.

Figure 1: This would be clearer if the four groupings could be labelled. It would also be useful if the sample locations could be labelled with the sample numbers. If necessary, a second figure should be used.

Technical corrections:

Page 29788: The description of the thermal-optical technique is not correct. The initial heating to 800°C is performed in an oxygen-free atmosphere to remove any organic carbon. The sample is then cooled and then a second temperature ramp is performed in the presence of oxygen to burn off the EC. The EC is typically oxidised at much lower temperatures. The authors should refer to Birch and Cary (1996) for details and revise accordingly.

Page 29790, line 21: Please clarify what is meant by 'inorganic carbon'. I'm guessing the authors mean carbonate, but many would consider 'inorganic' to include the

elemental carbon in soot with this definition.

Page 29797, line 23: For the sake of consistency, the supplementary material should be referred to as such, rather than ‘aux information’.

Figure 1: Given that there is very little useful information in the higher latitudes, this figure would be more efficiently presented in a rectilinear projection (the current one appears to be a Mercator).

Figure 2: Please label the axes and denote the sub-figures with a, b, etc.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29785, 2013.