

Interactive comment on “Vertical profiles of black carbon measured by a micro-aethalometer in summer in the North China Plain” by L. Ran et al.

Anonymous Referee #2

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This work reports vertical profiles of absorption during summer 2014 at a semirural area in the North China Plain. Profiles up to 1 km a.g.l. were measured by a single wavelength absorption monitor onboard a tethered balloon. Even though the area where the flights took place is reported as semi rural the measured concentrations are high and comparable to many polluted urban areas, as the authors state in the abstract (p1 l23-24). However in the manuscript no comparison is found between the surrounded areas and the area where the flights took place. Under such high concentrations, a validation in my point of view is required (see also my comment below).

In addition, the authors do not report the diurnal and, most important, temporal cycle of absorbing aerosol in that area. The reader is left questioned whether the reported concentrations correspond to the maximum, minimum or average concentrations in that area.

C1

Even though the vertical profile analysis done in this work is, in my point of view, complete and comprehensible with assistive figures, it is not complemented by any discussion on the surrounding area and the particulate temporal variations. I also found disappointing that instead of showing a map of the measurement area, a reference is used instead. The manuscript must be complete by itself.

Additionally, the authors report a multiple scattering optical enhancement factor C equal to 2.52 using a mass attenuation cross-section equal to 12.5 m² g⁻¹. I strongly support enhancing that part, as it holds the greatest interest. The authors should give more details on how the C factor was calculated and provide, as a minimum, a graphical representation of the results. As an example, Ferrero et al. (2011) reports C equal to 2.05 instead. The AE-51 cannot operate on a 24/7 basis. It was never designed to do so. Therefore, the authors should provide more details on how they conducted the comparison.

On top of that, I recommend the authors to read Hyvarinen et al., (2013, doi:10.5194/amt-6-81-2013). In that work MAAP was reported to underestimate BC concentrations even when the sample was measured onto fresh sample-spots. Even though MAAP and AE51 use different methods to derive BC mass (or absorption) the sampling strategy remains the same; sample is accumulated on a sample spot. Even though the face velocity of AE51 is a factor of 3 lower than that of MAAP, it does not exclude that the effect described in Hyvarinen et al., (2013) does not take place here. The comparison performed here, but discussed too briefly, can shed a light on this. It will also add value to the vertical profiles shown in this work.

My last remark concerns the smoothing algorithm. I understand that on a single wavelength monitor a smoothing algorithm is primarily used to remove outliers and make measurements more presentable. Under this perspective the authors provide adequate information on the smoothing process. However, multi-wavelength miniature absorption instruments are on the way and a proper smoothing algorithm is essential in calculating the angstrom exponent, as an example. Therefore I strongly encourage the

C2

authors to provide a comparison of unsmoothed dataset against those of the proposed algorithm and of Hagler et al. (2011).

Minor comments p1, L23-25. Please specify which polluted urban areas you are referring to.

p3, L5-10. Please add an image of the area, instead of a reference. Currently this work is somewhat incomplete.

p3, L19. what exactly does that statement means. Can you specify the conditions under which the AE-51 cannot operate. I am a bit surprised as this instrument has been used in drones moving with km h⁻¹ speed.

p5, L9. please provide more details on how the artifacts were addressed. Start by briefly mentioning what artifacts you are referring to. This is not obvious to the reader. The manuscript should be complete.

p6, L5-6. how does this affect the measurements. What is the diurnal variation in this area?

p6, L29. I suspect a typo at Larzridis, 2011

On page 6 the authors mention two methods from estimating the boundary layer height. Please discuss the differences in PBL height from these two methods.

p7, L16-17. Were these frequent vertical profiles complemented by ground measurements somewhere close by?

p8, L1-2. Was there any measurements performed the previous night. What was the result?

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