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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 #1

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This paper report results of vertical profiles of black carbon aerosol collected in the North China Plain. The topic and the reported measurements are very important as vertical profile data of BC a globally scarse if compared with the high amount of ground-based observation. Thus the topic of this paper is of fundamental importance. It is suitable to be published on ACP after the authors raised the following points.

MAIN POINTS: Abstract (page 1 lines 12-20): the developement of the mixing layer is qualitatively described. Moreover it is reported that the mixing layer usually developed from 0.2 km up to 1 km (i.e. sunny days) and followed by a "collapse" during the evening. In a such situation a residual layer usually forms above the NBL making the concentration measured above the mixing layer not representative of a clean free troposphere. Please discuss also the possible importance of the residual layer formation on your measurements along the entire manuscript.

Section 2.2.1: the developed smoothing algorithm appears very promising. However, a deeper discussion here is called for. Especially it is necessary to compare the smoothing results with that can be obtained by the ONA (Hagler et al. (2011)) application. I strongly suggest to introduce a new picture to show the effect of the two data treatment on the raw collected BC data along vertical profiles. The reason for a such request comes from the fact that the Hagler at al. algorithm is based on the physical behaviour of the measured ATN in the Aethalometer, while the new smoothing algorithm reported in this paper appear only statistically based and somehow affected by the operator (i.e. "(6) Repeat step (1)-(5) for m times to obtain acceptable smoothed data"). Concerning the last point in brackets: have you defined a criteria for the "acceptable smoothed data"? How much is the threshold? How much is the loss in terms of vertical resolution of the data after the smoothing? I think the smoothing algorithm should be also discussed more quantitatively than did until now.

Section 2.2.2, page 5, line 8: "Details of the correction scheme developed for tackling with artifacts of AE-31 were described in Ran et al. (2016)". Note that Ran et al. (2016) is just a submitted paper. In the reference list the journal to which Ran et al. paper was submitted is missing. Please add it. Moreover, as the AE31 data could significantly change in function of the chosen correction function it is necessary to resume here at least the main points of the correction scheme adopted in Ran et al. as the paper is not yet available to the scientific community. With respect to this, depending on the chosen correction scheme (i.e. C factors for each wavelength of the AE31), the obtained angstrom exponent should change introducing an error on the retrieved σMAAP,880nm. A quantitative assessment of the variability of σMAAP,880nm depending on the chosen correction scheme for the AE31 is called for. Moreover, I strongly recommend an analysis of the error propagation of σMAAP,880nm on the obtained C for the AE51. As a matter of fact the C factor of 2.52 is reported here without any statistical treatment of its uncertainty. Finally, no reference was made to the C value of 2.05 $\pm$ 0.03 for the AE51 reported in Ferrero et al. (2011a). It should very interesting to discuss the difference on the two C values in terms of the chemical composition of

the aerosol in the NCP with respect to the Europe.

Page 5, lines 12-13: "Measured σAE-51,880nm (ATN<10) and calculated σMAAP,880nm were linearly fitted with a correlation coefficient of 0.96 in a significant level (P<0.001), yielding a C value of 2.52": Please add the picture of this correlation.

Page 6, line 13, equation 6: "Hm was calculated from a sigmoid function that could well characterize typical daytime profile of mBC:". From this sentence it appears that Hm was calculated using equation 6. However, equation 6 requires as input both the mixing layer and the entrainment layer. This point is not clearly defined and needs to be specified. I also suggest to add a graphical example of the mixing layer determination using the sigmoid function. Finally a question: as you have both the potential temperature and wind profiles at disposal, have you ever thought to analyse the mixing layer also using the Richardson number approach?

MINOR POINTS: Page 7, lines 3-4: "the normalized height (HNor), which was calculated from h/Hm-1". In Ferrero et al. (2014) this analysis is explained. Add this reference at the end of the sentence.

Figure 2b: at Hnor=0 BC data are characterized by free troposphere concentration levels. I was a bit surprised about it. I expected that around Hnor=0 there was at least the end of the exponential deacrease of concentration starting from ground values. Could you comment it?

Page 3, lines 24-25 and equation 1: "to estimate aerosol absorption coefficients at the wavelength of 880 nm following". . . Please note that σAE-51,880nm is the attenuation coefficient and not the absorption coefficient as reported in many papers (i.e. starting from Weingartner et al. (2003)). Please correct the paper for this point.

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