

Interactive comment on “Assessment of wood burning versus fossil fuel contribution to wintertime black carbon and carbon monoxide concentrations in Athens, Greece” by Athina-Cerise Kalogridis et al.

Anonymous Referee #2

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General The manuscript attributes the concentration-contribution of wood burning to air pollution in Athens and with that handles an important health related subject that needs attention. For the source apportionment of black carbon, the well-known technique based on wavelength dependence of the aerosol light absorption coefficient is used after application of necessary compensations. For CO apportionment two models are described. Model 1 relies on known emission ratios of NO_x and CO and assumed similar atmospheric lifetimes. This model is not trusted by the authors and according to the authors this linear model always over-estimates the wood burning contribution.

C1

The results from model 1 are not researched enough to give recommendations to other scientific studies. It remains unclear why this model is included in this paper. A short discussion in the introduction could be enough. Model 2 for CO apportionment is based on multiple linear regression between CO and BC. This technique is well-known but in the conclusion section it is written that the method is new; or is the application to CO in combination to wood burning new? If new this fact should receive more attention earlier in the paper.

The manuscript is not pushing scientific boundaries, but contains important numbers, e.g. 30% of BC is wood burning related.

The authors were a bit sloppy on the correct references.

Details Abstract: ‘30% to the observed eBC and the CO concentrations (. . .)’ this doesn’t read well and is confusing for CO.

Section 2.2.1 For compensation parameter f values given in Drinovec et al. (2015) were used. But Drinovec et al. describe that filter loading effects change with location and time. The values of Drinovec differ a lot from Sandradewi et al (2008) (reference in manuscript but not listed in References, make sure to find the correct paper) and Zotter et al (2017). The latter paper, that is known to the authors confirms the importance of proper compensation. I would like to see a more worked out compensation correction.

About the instruments At DEM (AE33) at NOA(AE42). I understand that AE33 and AE31 are compared. At page 3 line29: ‘and data from AE31 aethalometer withto AE-42, which operates continuously in parallel with the AE33 at DEM station. So what instruments are compared and where is the AE-31 located?’

The R-squared of 0.79 is not very convincing for aethalometers, I would like to see the plot. The intercept (what is the unit? Inverse Mm or ng/m³?) of 0.15 is interesting.

I’m not sure, if it is interesting that ‘BC is historically defined from aethalometer measurements at 880nm’. The important message should be that the whole spectral de-

C2

pendence approach depends on fixation somewhere. This is done at 880nm because it is believed that at that wavelength the MAC for wood burning and fossil fuel combustion is very close. Otherwise the DEC MAC cannot be applied at NOA. The whole fractioning is based on the wavelength dependence that is somewhere fixed (Equation 10). The reader should be convinced of the choice that is supported by literature.

It is written '(MAC)... (determined from the comparison with concurrent measurements at DEM of elemental carbon)'. A bit later a reference to Diapouli et al. (2014) is included. Does this paper include the 7.5 m² g⁻¹?

The angstrom exponent for absorption is measured why do the authors assume an exponent of 1.0 in line 6 (p4).?

P4 line21 Reference to Sandradewi (please include the correct one in references). Sandradewi discussed different Angstrom exponents depending on the chosen wavelengths. This wavelength dependence should be discussed in light of the choices given in line 32, or refer to other studies that use same wavelengths. The 470 nm channel was broken in that Sandradewi study, why does this study start at 470 nm (line 32)

P5 top para. Exponents 0.9 (traffic) and 2.0 (wood) 'were used, based on the range of values.. reported'. The value of 2.0 is disqualified by Zotter et al., 2017, because it leads to differences with radiocarbon results. The exponents are crucial to the method, 'based on' should be worked out.

P6 line 20 'the hypothesis of negligible photochemical chemistry is validated.' Where is it validated please include reference(s). Negligible what does that mean, negligible for the scale considered in this study? Or is the assumption that BC and CO have similar lifetimes? This para needs to be worked out to convince the reader that model2 is superior to model 1.

Page 7 line 9 'eBC in PM2.5' but at NOA a PM10 sampling head is installed, right?

Page7 line 11. Apparently the 1.25 percentile of a dataset can be used for background.

C3

Really would like to read that paper. Please include Kondo et al., 2006 in the references.

P7L14 'relatively short lifetime of BC' please compare to P6L20 P7L34 how is the relative standard deviation defined in this case. P7L39 '0.184' please include units if appropriate

P8L4 the value 0.00137 is 0.7% of the best estimate 0.184. This is very small compared to values in Table 2. Please include discussion. Why is 0.00137 an useable value and why is the resulting uncertainty of 25% for the emission ratio 'rather reasonable' (not scientific terminology)

P8L13 background or intercept values are 109 and 147 how do these differences related to 'cannot differ significantly' line 1 of this page?

P8L14 background concentrations of CO with a reference to Goldstein and Schade (2000), this work contains some informations on background but not on CO. How should the reader interpret the reference, please modify.

P8L14etc The resulting background concentration are in very good agreement with ... 1.25 percentile. Really want to learn more. For me it sounds like abracadabra.

P8L41 'using a best fit line' If this is a fit how was the data selected? This was not clear from the references literature.

P9Line4 informs us that the ratio is larger than ... please explain

P9L9 'values found in the literature' please include references.

P9L10 2-3% where should I look to see the supporting material.

Table 4 Regression Slope between model 1 and model 2: what model outcomes are regressed? Are we looking at CO_{wb}/totalCO?

Typos-suggestions P3 line26 'this purpose' → for loading compensation P4 L9 ratios

C4

were P4 line 25 λ is bold in equation P4 eq 5 λ_1 should be λ_2 in denominator. P7L40 last ff should be sub. P8L28 diurnal variabilities ...are P8L33 Comparison of A and B Figure 7 caption or axis titles are wrong for right bottom figure

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