Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-130-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment of carbonaceous aerosols in Xi'an, China: insights from a full year of measurements of radiocarbon and the stable isotope ^{13}C" *by* Haiyan Ni et al.

Anonymous Referee #2

Received and published: 1 May 2018

General comments This paper reports the results of source apportionment based on a 1-year campaign in China. Besides the specific results, the paper presents an interesting methodology, based on the synergic use of radioactive and stable carbon isotopes. The paper ends with an open question, but this may be the trigger to foster new research. Therefore I think the paper is worth the publication. Specific comments - Introduction, page 2 lines 16-18: "The 14C content of an aerosol sample is usually reported relative to an oxalic acid standard, and expressed as fraction modern (F14C). 14C content of the standard is related to the unperturbed atmosphere in the reference year of 1950 (Mook and van der Plicht, 1999; Reimer et al., 2004)"; please change to "The 14C content of an aerosol sample is usually reported relative to an oxalic acid

standard, and expressed as fraction modern (F14C). 14C content of the standard is related to the unperturbed atmosphere in the reference year of 1950 (Mook and van der Plicht, 1999; Reimer et al., 2004), and this is usually done with/by means of/similar a standard"; this sentence is more correct and actually describes much better the definition formula in the following line. - Introduction, page 2 lines 21-22: the assumptions on F14C are reported in a far too simplistic in several points (later on they become simple "conversion factors"). I suggest the authors to better introduce these quantities to facilitate the reader and also the writing of the following sections. The same for d13C: it is introduced in the following sections, it is true, but at this stage there are already some sentences maybe not clear to readers not too familiar with stable carbon isotopes (e.g. "signature depleted", but it is not clear with respect with which reference.) - Introduction, page 3 line 7 and table S1: actually, source signatures are not that well distinct, as they have overlaps: may the authors discuss deeper this point? - Sampling, page 3 line 28: why was the sampling time chosen to be 10 am to 10 am next day? Due to manual change? How long were the samples kept inside the sampler after sampling? - Sampling, page 4 line 4: citations missing ("previous studies" are not cited) - Stable carbon isotope (13C) analysis of OC and EC, page 4: some more details on the analysis would be welcome. Further, the title is maybe misleading, as it suggests that only 13C is measured, while I guess that also 12C is assessed for determining 13C/12C ratios. - Radiocarbon (14C) measurement of OC and EC, page 5, line 9: "Two standards with known 14C content are analyzed as quality control: an oxalic acid standard and a graphite standard.": maybe I did not understand, but I believe these standards are respectively for normalization and blank evaluation; if this is correct, they cannot be defined as "for quality control". In case further standards are measured as unknown, these can be defined "for quality control". - Radiocarbon (14C) measurement of OC and EC, page 5, lines 11-13 and 24-25: there is a repetition of the information, and actually not completely in the same way: please correct it. Further, is this contamination modern or fossil? - Source apportionment methodology using 14C: as already aforementioned, the use of the definition "conversion factors" is misleading,

as they have a physical meaning (as it is clear at page 6, line 24). Authors should introduce this concept earlier in the text, so that they can also explain the use of different values for their “conversion factors”. This would definitely make the paper easier to read. - Temporal variation of fossil and non-fossil fractions of OC and EC, page 7: levoglucosan, hopanes and picen are cited for the first time, with no reference to S2, where the measurements are described. The existence of ancillary/additional measurements deserves to be introduced as part of the methodology. - 13C signature of OC and EC, page 9, line 12: “δ13COC was in general similar to δ13CEC”: this means that the biogenic source is roughly negligible: can the author comment with finding also in relation to the radiocarbon measurement results? - I suggest moving section 4.4 straight after 4.2, as this discussion follows directly from the last sentences of 4.2. - Changes in emission sources in Xi’an, China (2008/2009 vs. 2012/2013), pages 15-16: the cited papers taken for comparison focus, respectively, on a big haze episode and on an intensive campaign (2 winter months), and not on a campaign aiming at being representative for a year, therefore I think this comparison is not very useful. Further, contributions are roughly the same within the uncertainties. - Supplement, table S1: far as I get, the reported interval for C4 plants is wide as different plants (corn, sugar cane, grass and maybe more) have different signatures: why do the authors “decrease” this range to -16.4 +- 1.4 permil? (Futher, please pay attention to number of digits, e.g. -23.4 +-1.3 and not -23.38 +-1.3)

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