

Interactive comment on “Fossil fuel combustion, biomass burning and biogenic sources of fine carbonaceous aerosol in the Carpathian Basin” by Imre Salma et al.

Anonymous Referee #4

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Interactive comment on a paper by: Imre Salma, Anikó Vasanits-Zsigrai, Attila Machon, Tamás Varga, István Major, Virág Gergely, and Mihály Molnár: Fossil fuel combustion, biomass burning and biogenic sources of fine carbonaceous aerosol in the Carpathian Basin

General comments: The paper deals with source attribution of carbonaceous aerosols in the Carpathian basin using chemical analysis of PM_{2.5} samples obtained using high-volume samplers. Besides standard analytes as organic carbon (OC), elemental carbon (EC), water-soluble carbon (WSOC), and few chemical elements, wood combustion markers as levoglucosan (LVG), mannosan, and galactosan were analysed

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together with radiocarbon ^{14}C . The authors use their pragmatic coupled radiocarbon-LVG marker method (Salma et al. 2017) to attribute OC and EC to fossil fuel, biomass burning and biogenic origin fractions in Carpathian basin based on parallel, one week sampling period, at three sites in each season. Although the topic is important and data are new and valuable within the mentioned area, there are several major issues that should be answered before publishing the paper.

First, the naming of sampling periods (winter, spring, summer and autumn) suggests that the data are representative for these seasons. This is not true as sampling was performed during 14 (or even 7) consecutive days during each season and only 7 overlapping days were fully analysed for ^{14}C . These sampling periods are too short to be representative for a season and therefore, months, when data were taken, are more proper for naming of the sampling periods. For the same reason, the authors should more concentrate on differences between the sites and less on “seasonal” characterisation and differences. More detailed weather characteristics for each sampling period can explain more various type of events that change differences among the sites.

Second, median values presented e.g. in the Table 2 or 4 can be representative for only part of the data especially if two types of atmospheric mixing were present during short sampling period. Therefore, either medians with high and low percentiles or averages with standard deviations should be presented together.

In addition, particle number concentrations paragraph (lines 437-442) is completely out of topic of the paper, it should be omitted together with related references.

Finally, the combination of OM/OC conversion factors used by authors is not logical and is not based on current scientific knowledge and must be corrected. Therefore, most of the calculations must be corrected.

Specific comments: Line 251-253 - The authors use conversion factor for city centre 1.6 and for suburban and rural cities 1.4. This is taken opposite way than it is usual. While both lower values 1.4 and used value 1.6 can be accepted for places with fresh traffic

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aerosols – city centre the value 1.4 used for urban and rural background is unacceptably low. Some seasonal dependence of this factor can be also expected. Actually, cited work of Turpin and Lim 2001 says in its abstract: "This investigation suggests that 1.4 is the lowest reasonable estimate for the organic molecular weight per carbon weight for an urban aerosol and that 1.4 does not accurately represent the average organic molecular weight per carbon weight for a non-urban aerosol. Based on the current evaluation, ratios of 1.6 +/- 0.2 for urban aerosols and 2.1 +/- 0.2 for non-urban aerosols appear to be more accurate" Therefore, the calculation for suburban and rural sites have to be recalculated with higher conversion ratio OM/OC (at least also 1.6).

Specific comments Line 265 – It should stay "Their" instead of "They" Line 291 -293 – Measure of photochemical activity is not ozone concentration itself. Line 305 – WSOC vs SOA relation can be biased by biomass burning emissions. Therefore, the sentence needs correction. Compare also with lines 385 and 388. Line 378 - "minimum in summer" can be omitted as it is mentioned again in the next sentence. Lines 437-442 – It is out of topic, remove the paragraph Lines 456 – 457 – The sentence should be corrected, the results do not justify fully such sentence. Line 488 – OC/EC ratios can be influenced also by other effects, therefore less strong opinion would be more proper Line 547 – correlation coefficients are significant or insignificant based on given statistical criteria. Correct the sentence. Line 548 – "linear" relationship of OCFF with NO was seen for suburban site only (corr. coef. 0.93) while for city centre was only 0.39. Therefore, the sentence needs correction or clarification. Line 551 – the last sentence should be removed or corrected. The correlations can support results but not approve them. Line 569-570 – the differences in share of OC/BIO are negligible in comparison with their uncertainty, therefore, no tendency can be retrieved from the data. Correct the sentence accordingly. Lines 662-664 - again OM/OC conversion factors – correct as mentioned above.

Graph 8 – if authors want to show differences in OC shares during their sampling periods they should stop call them seasonal differences, as their sampling periods

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cannot fully represent seasons. Moreover, the lines in graphs are not representative for the data giving sometimes unrealistic impression about the data. Redo the graph

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