

## ***Interactive comment on “Polar organic compounds in rural PM<sub>2.5</sub> aerosols from K-pusztá, Hungary, during a 2003 summer field campaign: sources and diurnal variations” by A. C. Ion et al.***

**A. C. Ion et al.**

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We would like to thank this reviewer for the time and effort spent in evaluating our paper. The comments have been helpful and have improved the paper. We have accepted the majority of the technical points and believe we have good explanations for points on which we do not concur.

Reviewer: The main text contains a large number of repetitions and unessential statements. In the Abstract, for example, the method of analysis needs not be described; the specific compounds should be named only once. The Introduction can be reduced to the first sentences of the first and second paragraph. All the relevant information is

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summarized (again) on pp. 1870-2. Once is enough.

Response: The repetitions have been removed. The method of analysis has not been mentioned in the Abstract and the specific compounds have only been named once. A large part of the Introduction dealing with the sources and source processes of the targeted compounds has been moved to Results and Discussion. However, we do not concur with the statement that the main text contains unessential statements. Without the information given on the sources and source processes of the different marker compounds presented in the Introduction and section 3.2 of the original manuscript, for example, our paper would become incomprehensible.

Reviewer: The section Experimental contains a lot of details which should be shortened wherever possible. On the other hand, some aspects of the experimental approach are missing. Why were the quartz filters baked only at 550°C?

Response: The Experimental section has been considerably shortened and reference has been made to previous work wherever possible. Baking at 550°C for 24 h was, in previous studies, found to be sufficient to remove organic material and to result in low blank values. Since reference was made to previous work, we did not consider it necessary to include these experimental details in the revised manuscript. Other recent studies on organic aerosol speciation report similar baking conditions, for example, Schkolnik et al. (2005) uses >500°C and overnight baking.

Reviewer: Why was only 1/16 of the high-volume filter used ?

Response: The following sentence has been added: "Only 1/16 of the whole filter was used because workup of larger portions led to problems in the GC/MS analysis due to co-extraction of sulfuric acid."

Reviewer: How was WSOC determined ?

Response: The following information has been added in section 3.2 of the revised manuscript: "The filters from the fine size fraction (<2.5 micrometer AD) were also

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analysed for water-soluble OC (WSOC). A filter punch of 1.0 or 1.5 cm<sup>2</sup> was placed in a 15 mL tube, 5 mL Millipore Simplicity water was added, and the tube was hand-shaken during 5 min, after which it was allowed to stand for 30 min. The sample extract was then filtered through a PVDF syringe filter (pore size 0.2  $\mu$ m) and analysed for total organic carbon (TOC), thereby correcting for the inorganic carbon, with a Shimadzu TOC-V CPH analyser. The TOC data were used as WSOC.”

Reviewer: What is OC, just the C in OM ?

Response: Yes, OC is just the C in OM. The abbreviation OC (organic carbon) was defined in the first version of the manuscript on p. 1866 (Introduction). We thought this term is quite common in organic aerosol studies.

Reviewer: It is very irritating to see that the description of the experimental details is continued in sect 3.1. This part must be integrated in sect 2.2, shortened and put into perspective, better than in the present form.

Response: The section 3.1 of the original manuscript has now been integrated into section 2.4 of the revised manuscript and shortened.

Reviewer: The section Results and Discussion should start with an overview on the quantitative details, in the form of the data in Table 1.

Response: We do not agree with this suggestion and strongly feel that a section on characterization and sources is needed to make the rest of the paper comprehensible.

Reviewer: However, the table should contain separate columns for N and D data. Table 2 must be cancelled (no problem for the reader to calculate fractions by him/herself). The text on pages 1872, 1873 and 1875 must be condensed significantly.

Response: A new Table (Table 2 in the revised manuscript) has been made with separate columns for N and D data and Table 2 (Table 3 in the revised manuscript) has been retained. We do not agree with the suggestion to cancel the latter Table. The new Table 2 contains medians and ranges, whereas the new Table 3 has averages and standard

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deviations, which the reader cannot calculate him/herself from the new Table 2. The new Table 3 enables the reader to make comparisons with similar data that have been published for other sites. Incidentally, we made an error in the calculation of the data in the original manuscript. Hence, the corrected values are given in the revised Table and the text. We do not see how it is possible to condense significantly the text on pages 1872, 1873 and 1875; as already stated above, we do not concur with the statement that the main text contains unessential statements and strongly feel that a section on characterization and sources is needed to make the rest of the paper comprehensible.

Reviewer: In its present form, the section Conclusions merely constitutes a second abstract, a waste of paper. A new section Conclusions should (i) briefly describe the improvement in understanding that has been achieved by the present study and (ii) address consequences as well as desirable future work.

Response: A new section Conclusions has been written taking into account his/her suggestions as well as general comments made by reviewer #5.

Reviewer: As to the figures, the reviewer has the impression that the authors spent far too little time on data evaluation and optimum data presentation. The information contained in several of the figures is hard, sometimes impossible to read. More specifically, the following comments and suggestions should be considered: Fig. 1. To be cancelled; reference should be made to other papers where the molecular structure has already been shown.

Response: We as well as the other two reviewers do not find it a good idea to cancel this figure and, therefore, the figure has been retained.

Reviewer: Fig. 2. The labels should be enlarged by about 30% to make them easily readable in print (all figures one-column wide). Fructose and glucose should either not be mentioned specifically (there are other unlabelled small peaks), or should also be labelled by number. Which are the three peaks for fructose ?

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Response: A new Figure 2 has been made taking into account the above suggestions. The three peaks for fructose elute closely to each other and we admit that this cannot be seen in the chromatogram. Therefore, we have preferred not to mention fructose and glucose specifically.

Reviewer: Fig. 3. All labels, notably those for N and D at the abscissa would be unreadable in print (one column wide figure). Dates at the abscissa should only be shown in two-or three-day intervals. The figure caption should state that points reflect alternating N/D data. To illustrate the relevant differences in N/D behaviour only one example for a photosensitive compound should be shown, e.g. mannitol (use x instead of \* to indicate multiplication factor).

Response: A new Figure 3 has been made following most of the above recommendations. Since this Figure contains a wealth of information and is quite complex, a specific request is made to the printer to display it over a width of 2 columns. Further, we like to point out that mannitol is not a photosensitive compound but a marker of fungal fragments, which should be regarded as primary organic bioaerosol. A sentence has been added to clarify the distinction between photosensitive compounds such as isoprene oxidation products and fungal bioaerosol markers (section 3.2): “It is pointed out that the latter compounds, unlike the isoprene oxidation products, are primary aerosol components that are associated with fungal fragments.”

Reviewer: Fig. 4. “%XYZ” is not a physical quantity; an appropriate notation would be XYZ Fraction of OC(%). Why fraction of OC and not mass concentration? Remove upper inset. The caption should state that the line is the result of a linear regression.

Response: A new Figure 4 has been made using mass concentrations.

Reviewer: Figs. 5 and 6. These two figures must be redesigned completely to make the data transparent and to identify a correlation with meteorological parameters. The differences between D and N become much more evident if they were identified by different symbols and if each set of N and D data is connected by a separate line. Data

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for two typical examples (compounds) plus one for levoglucosan should be shown in individual panels, log scale (same scale in decades/cm). Data for other compounds should be described by comparison to these graphs. In doing so, it will become clear that most of the time the D/N ratios were large, but that there were several days/nights on which the ratios were around or even below unity. Interpretation of the data in terms of efficient photochemical oxidation makes sense only if the times with high D/N ratio exhibited much higher intensity of sunshine than those on which D/N was around unity. The correlation with meteorological parameters must be included in a revised manuscript. Again, the authors should provide a good argument for presenting the data for the organic compounds as fractions of OC and not mass concentrations.

Response: New Figures 5 and 6 have been made following most of the above recommendations. Further, as these Figures contain a wealth of information and are quite complex, a specific request is made to the printer to display them over a width of 2 columns. As stated in the revised version, we have a good argument for presenting the data for the organic compounds as fractions of OC and not as atmospheric concentrations. The following sentence has been added in section 3.2: “The percentage carbon contributions to the fine OC are thought to provide better a insight into the time-dependant formation of the targeted compounds than the atmospheric concentration data, since the latter depend on meteorological conditions and are, e.g., affected by day/night variations in the mixing height of the tropospheric boundary layer.”

Reviewer: Fig. 7. Scatter plot for malic acid vs. OC. Using the terms “OC” and “Malic acid” to indicate mass concentrations (MC) is laboratory slang. More importantly, the statement (p. 1874) that “malic acid can serve as a reasonably good general indicator compound for the organic carbon mass in the PM<sub>2.5</sub> aerosol” cannot be accepted. First of all, ... The authors should either spend more time on data evaluation to identify true correlations or should limit themselves to merely presenting the data without adding unjustified interpretations.

Response: The statement “malic acid can serve as a reasonably good general indicator

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compound for the organic carbon mass in the PM<sub>2.5</sub> aerosol” has been dropped and Figure 7 has been removed. We have appreciated the efforts of the reviewer to perform a detailed evaluation of the malic acid and OC mass concentration data which show that MC(OC) varies roughly as the square root of MC(MA). As indicated by the reviewer, a firm interpretation about this correlation cannot be made, and, therefore, we have preferred to limit ourselves to presenting the data without adding interpretations and to mention in the section Conclusions that further research is warranted on this issue.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1863, 2005.

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