

Interactive comment on “Functional group analysis by H NMR/chemical derivatization for the characterization of organic aerosol from the SMOCC field campaign” by E. Tagliavini et al.

Anonymous Referee #2

Received and published: 15 October 2005

This work expands on previous studies by these authors. This time, they are focusing on the characterization of the water soluble (WS) fraction of the organic component in atmospheric aerosol particles. The unique approach here is not to analyze specific organic compounds, but rather to identify the amount of the major functional groups present in the sample. This analysis provides an overall view of the chemical composition of the particles. In this study, the method was applied to smoke particles, and this is the first time that it has been applied to such matrix. In this study, the authors also introduce a new method to overcome problems encountered in previous studies: by derivatization of carboxylic groups they can, for the first time, determine the carboxylic

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

content of the WSOC. Another unique aspect of the paper is the comparison of the functional group content as a function of aerosol size.

The manuscript contains many interesting observations and conclusions, some of them are puzzling, such as the contrast with other chemical analysis methods that observe differences in the chemical composition between day and night, and the conclusion that the coarse fraction contains very high portion of polyols. Another interesting observation is that the fine mode of fresh smoke particles is substantially less oxygenated than larger ones. These observations call for more research on the chemical composition of smoke particles, and for more detailed and well-performed intercomparison between this new method and the more traditional ones. After reading the manuscript, I am not sure how quantitative the method is, as details about calibrations, limits of detection and sensitivity are missing, and should certainly be filled in. The authors should dwell on this and compare to other commonly used methods, especially when they talk about specific compounds such as polyols and levoglucosan. Finally, the issue of “missing carbon”, especially at highly loaded samples may point out to some problems in sampling or extraction.

The manuscript fits well the interests of ACP's readership. It could benefit from a ~20% reduction in length.

Some minor points that the authors may want to address are:

1. page 9455, line 15: how was the identity of the polyols determined? What is the detection limit for these and other compounds by the method? How does it compare to other methods?
2. page 9456 line 32: what does “relatively less’ means? How can this method be more quantitative? How was the sensitivity of the method calibrated?
3. page 9458, line 6: The estimation for the contribution of the of the fine particles should take also into account the mass loading in each one of the fractions, and not

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

only the relative volume of air passed through the filters.

4. page 9459, line 1: the assignment to polyols seems hasty. Can the authors substantiate it?

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9447, 2005.

ACPD

5, S3235–S3237, 2005

Interactive
Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper