

Interactive comment on “Rural continental aerosol properties and processes observed during the Hohenpeissenberg Aerosol Characterization Experiment (HAZE2002)” by N. Hock et al.

Anonymous Referee #2

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The paper describes the chemical composition of atmospheric aerosol in a rural region in Germany, during the HAZE 2002 experiment. Results were obtained from a large variety of instruments, including Aerosol Mass Spectrometer (AMS), gas chromatography - ion trap mass spectrometer, Condensation Particle Counter (CPC), and Scanning Mobility Particle Sizer (SMPS). The results of the campaign, as well as the data analysis are presented clearly. The conclusions are effectively supported by the discussion of the experimental results. Overall the paper is well organized into sections that concisely illustrate several issues related to the composition, properties and formation of aerosols. In order to investigate the OM/OC ratio during this study, the authors compare the OM measured by AMS on submicron aerosol to the OC determined

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on PM_{2.5} samples. The authors should clarify in detail which are the assumptions required to make significant the comparison of different fraction of the suspended particulate matter; they should also estimate the relative error for the OM/OC ratio reported. As concern the source of oxidized organic aerosol, the conclusions are not consistent with the results and discussion section. Paragraph 3.4.1 points out that highly oxidized organic aerosol might be an indicator of aged aerosol or biogenic emissions. In the same paragraph the authors claim that the aerosol in the Hohenpeissenberg area was dominated by local biogenic emissions (page 8634), as shown by the AMS fragmentation pattern. In the conclusion paragraph the oxidized organics are then attributed to photochemically aged aerosol. The assessment of ternary nucleation seems weak. Is this the only possible explanation? What evidence rules out organics in new particle formation? Is the measured value nucleation or growth? The authors should consider the findings of other groups, especially in more urban influenced areas (see McMurry on St. Louis, Pandis on Pittsburgh, Russell on Boston, or even O'dowd on Ireland). The paper shares a major weakness of several similar papers that it cites primarily AMS literature and fails to acknowledge earlier findings on OM/OC, preferential scavenging, organic aging and sources. Since these are presented as major new findings of this work, the failure to cite the precedents is a serious flaw that reflects poorly on otherwise interesting work, and poorly on a journal that would publish it without proper referencing.

In details: There are numerous typos in spelling and even some missing periods. Due to time constraints, I am unable to list all of them here but I trust the authors will endeavor to correct them. Page 8620, line 18. Additional references are needed to explain the debate about CCN activation of aerosol particles. Page 8621, line 25. Replace mn with nm. Page 8621, line 26. Correction of verb tens; replace has been with was. Page 8624. Figure 1 caption should not be repeated in the body paragraph. Page 8625, line 17. The fact that EC is a small fraction of PM₁ does not justify the approximation of EC in submicron aerosol with EC from PM_{2.5} measurements. Page 8625, line 24. HVS sampler should be replaced with HVS. Page 8626 paragraph 3.2. The

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criteria that are used to identify and distinguish the three time periods described in the paragraph are not clear to the reader. Page 8641, line 14 - 21. The paragraph should be move in the first part of the conclusion section to follow that same order used in the results and discussion section. Page 8641, line 27. Missing period between “size range” and “Since”.

Caption of Figure 1, line 4. Remove “and”. Replace QAMS with Q-AMS.

Figure 1, 3, 9, 10, 12, 13. Dates on horizontal axis are reported using the European notation (day/month/year). Such a notation might be ambiguous. Figure 6. The mean values are reported with different symbols in each panel.

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