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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cn to ccn relationships and cloud microphysical properties in different air masses at a free tropospheric site" *by* R. Dupuy et al.

Anonymous Referee #3

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Review of the manuscript "CN to CCN relationship and cloud microphysical properties in different air masses at a free tropospheric site" by R. Dupuy, P. Laj., and K. Sellegri.

The MS presents data on CCN/CN ratios which were deduced from CN measurements and cloud droplet spectra obtained from FSSP data on the Puy de Dome mountain site in France. The MS contains relevant material on an important field of atmospheric research and lies within the scope of ACP. Before being acceptable for publication, major revisions are required which are related to the presentation of material and the drawn conclusions as well as to the use of language.

GENERAL COMMENTS

1. The used language requires careful revision, since numerous typos and missing words occur.

2. The presented material is part of a long-term effort with some results being already published. Therefore, the reader is referred to other publications also for essential information on experimental details and applied analytical methods. In the present form, the paper is no stand-alone publication. The authors should give some space to a description of the background of the study, to applied methods concerning data analysis and air mass classification, and to key results on aerosol characteristics etc. published in other papers.

3. The procedure of the data analysis is not clear to the reader. It would help when the author present some time series of key data for one cloud event in the Measurements section to show what they have been done. Also a brief description of applied analytical methods is necessary. The reference to Sellegri et al. (2003a, b) is not sufficient.

4. A description of the air classification scheme is definitely required, since otherwise the reader cannot get a clear picture which criteria have been used. The classification is a crucial step in the data analysis. A trajectory plot for the different classes would also help.

5. The bulk chemical composition has to be reported for different size classes. The information must be available since the authors use impactor data. Bulk mass is dominated by coarse mode particles while the CCN fraction is dominated in terms of number by the accumulation mode aerosol. Currently it is not clear whether the large organic fraction is concentrated in the coarse mode or in the accumulation mode, although this knowledge is crucial for the drawn conclusions.

6. Figure 2 and the conclusions drawn from it are totally unclear to me. The authors discuss supersaturation but present no data. They should have relative humidity mea-

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Interactive Comment

surements available to justify the assumptions.

7. Figure 3 and drawn conclusions are debatable. Fig. 3 should show the effective volume as a function of the LWC per particle. The authors interpret the different slopes of regression lines in terms of the shape parameter k. Are the differences in the regression lines statistically significant between the data sets? To my impression the difference is not between air mass types but is a function of LWC per particle. At least, the authors should give some value and range of uncertainly for k, i.e. please report the parameters of the regression lines.

8. Section 4 and Fig. 4 contain the data analysis results. However, the conclusions drawn from Fig. 4 are not obvious to me. I cannot see a statistically significant difference in the scavenged fraction between air masses MM and CL. Only the mean of air mass PL deviates significantly from the others. Particularly the difference between MM and CL is not obvious. Please discuss the statistical significance before interpreting the small difference. The authors should also show aerosol size spectra for all different cases because they argue with differences between air masses without showing them.

9. A more general question is, whether the authors can really measure the CCN fraction, because CCN activated aerosol and scavenged aerosol mean different things. The authors can measure aerosol scavenging without being able to decide which fraction of the scavenged aerosol was activated during cloud formation and which fraction was scavenged during cloud ageing. A more precise use of terms would help. The answer to this question affects the entire section on the Impact on cloud microphysics. The entire section 5 requires careful revision, since currently the drawn conclusions are not clear. The same statement holds for the Conclusions section.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 879, 2006.