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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 #1

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This paper takes measurements of CCN/CN ratios, inferred from FSSP and CN counter data, obtained in cloud at a mountain site in France. It catagorizes the data by where the airmass came from and by cloud liquid water content, and attempts to describe differences based on measurements of aerosol composition.

Primary concerns include the following: The paper is not well written, has numerous typos, grammatical and stylistic errors, and in some situations paragraphs are unreadable. Second, CCN composition cannot be inferred from bulk mass measurements since bulk "whole air" aerosol mass is controlled by supermicron aerosol whereas CCN

are controlled by submicron aerosols. Third, and most importantly, the CCN/CN ratio is determined by particle size and composition, but foremost by cloud supersaturation. CCN/CN will be unity if the supersaturation is sufficiently high, and zero if it is too low. No measurements of supersaturation or updraft velocity are provided, and considerable variability would be expected because the cloud forcing is orographic, and the catagorized airmasses are coming from different directions and at different speeds. In the absence of any control for cloud dynamics, it is not clear that any useful scientific information can be inferred from this study.

1. p. 882 l. 9: An independent measurement of the effective radius can be provided by the PVM probe, through the combined LWC and PSA channels of the instrument. Since both are used, how did the PVM and FSSP compare? This would be useful to know, in order to infer confidence in the results.

2. p. 882 l 12: What is the RJI inlet?

3. p. 882 l 25: What is the size range of the aerosol analyzed using the gravimetric technique? Is there any size threshold cutoff?

4. p. 883 l 7: It is two easy to incorrectly read CL as “clean” next to PL for “polluted”. I suggest CT instead (or better yet just use the words polluted, continental, and marine throughout)

5. p. 883 l 15: It needs to be clarified whether much information can be derived from the mass measurements regarding cloud activation. CCN are controlled by particles in the 0.1 to 1 μm size range, whereas mass is controlled by particles > 1 μm, which tend to form due to entirely different mechanisms. For example, the organic matter observed could simply be spring-time pollens, for example.

6. p. 883, l 26 and 27. Words such as “as expected”, or “surprisingly” should not be used without some words of explanation as to why.

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7. p. 884 l 3: What were the “large variety of clouds measured in this study”?

8. p. 884 l 13: What references therein?

9. p. 885 l 2: What LWC probe was used here? It should be the PVM. If the FSSP probe was used, then it is not accurate to infer high correlation, since both N and LWC from the FSSP are correlated through $LWC = 4\pi\rho N r^3$.

10. p. 885 l 4: It is not clearly explained why low variability in k implies adiabaticity. Could not turbulence and entrainment produce a size distribution that has a fairly uniform value of k?

11. p. 885 par 2: The LWC classification scheme is very confusing. $dLWC/dz$ is a strong function of temperature, and because the samples include a transition season at mid-latitudes, should be expected to be highly variable. Moreover “thin clouds” and “sampled near cloud base” are not the same thing. “medium clouds” could be quite thick if $dLWC/dz$ is low. Perhaps “low”, “medium”, and “high” would be better?

12. p. 884, par 3: It is not clear why polluted airmasses should have a lower CCN fraction. The sentence “A large fraction of the PL particles are smaller than the activation diameter leading to a limited F_{N_p}”. Data should be provided to show this is the case. It should not be “expected” because in highly polluted airmasses nucleation mode particles tend to grow rapidly to accumulation mode sizes (> 50 nm) through condensation and coagulation.

13. p. 885 and 886. I am not familiar with the Puy de Dome, however I suspect that like most other topography it is not perfectly symmetric. Air that approaches it from different directions (as for the three cases described here) will experience differing degrees of orographic lifting. Since vertical velocity is the primary controlling factor for cloud supersaturation, and supersaturation is the primary controlling factor for the CCN/CN ratio (F_{Np}), I don’t see how much can be inferred

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from differences in the data without controlling for the updraft velocity at cloud base (where the nucleation occurs).

14. p. 886 l. 10: This is confirmed... Please provide details and a reference.

15. p. 886 l. 15: No reason is given for why the second hypothesis is most probable.

16. p. 886 l. 20: What exactly is meant by “remoteness of the airmass”. With respect to what? Can a reference be provided?

17. p. 887 l 21: The difference in F_{Np}... This is not shown in the paper, nor is it referenced. The significance level for the statistical test is not given.

18. p. 888 l 3. “a parameterisation” What is this parameterisation? It is not a particularly meaningful statement to the reader, who would need to look up Raga and Jones to infer what this parameterisation is.

19. p. 888 l 4. What does 2 μm refer to? If this is a difference it is not clear what the quantity is.

20. p. 888 l 9. The last sentence is confusing, and the argument does not clearly follow.

21. p. 888 par 2. I cannot make sense of this paragraph, even after reading several times.

22. Last paragraph. The last paragraph makes no sense in English.

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