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***Interactive comment on* “Estimating seasonal variations in cloud droplet number concentration over the boreal forest from satellite observations” by R. H. H. Janssen et al.**

Anonymous Referee #2

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This paper studies the seasonal variations of cloud condensation nuclei (CCN) and cloud droplet number concentrations for low-level stratiform clouds over boreal forests in southern Finland. The investigation is based on 9 years (2000–2008) of MODIS satellite data as well as ground-based data from the SMEAR II measurement station in Hyytiälä, Finland. The satellite retrievals were used for days with a solar zenith angle of less than 60°. Therefore, the studied cycle was constrained to a period between April and September, which coincides with the growing season of the boreal forest. A cloud model was used to calculate the cloud droplet concentration and cloud physical thickness from the observational data. These calculations are followed by a detailed uncertainty analysis for each variable. The authors conclude that the cloud droplet number

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concentration is insensitive to aerosol concentrations at the surface. Additionally, it is stated that a low cloud droplet number concentration is related to stable atmospheric conditions, which leads to the conclusion that activation of aerosol particles to cloud droplets is limited by convection in boreal forests.

In general this paper is well written and presents interesting results, which matches the scope of Atmospheric Chemistry and Physics. I recommend this manuscript for publication, if the authors can address my comments below.

1) It would be interesting to get an idea of the occurrence of low-level stratiform clouds in this area. How many events are taken for each data point, e.g. are more stratiform clouds found in the spring time?

2) Are there informations available about the hygroscopicity of the aerosol particles? Is there an annual cycle, which could also influence the activation of aerosol particles?

3) Are there also measurements of the updraft velocities available at this measurement site, which can support the findings of Fig. 3 c)?

4) Why was a supersaturation of 0.2 % chosen? Is this a typical value for these kind of clouds?

5) The discussion of the calculation for cloud thickness (p.10013) is very short. What are the reasons for the large uncertainty? What could be learned from a changing cloud depth?

6) How would the results change for Fig. 5 b), when the different definitions of the activated fraction (Fig. 6) were used? Can the authors discuss briefly what they would expect?

7) A map with the location of the measurement site and an indication of the $2^{\circ} \times 2^{\circ}$ box could be helpful.

Some minor remarks:

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- 1) p.10003, line 28: a comma between “surface data” and “an introduction” is missing
- 2) p.10004, line 11: what is meant with “both”?
- 3) Figure 3: Meaning of error bars for the cloud depth is missing.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9999, 2011.

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