The authors gratefully acknowledge the comments by the Anonymous Referee 2.
We give detailed point-by-point answers in the text below. The referee comments are presented in italics and the corresponding replies as normal text below each comment.

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?

We checked the number of data points per bin. It has a mean±std of 91±18. There is no clear seasonal cycle in the occurrence of stratiform cloud cases and we did not find a correlation between the number of points per bin and N_{CD}. We have included some sentences in the revised version to explain this in the results (Sect. 3.1) and the number of points per bin have been added in Fig. 3 (Fig. 2 in the discussion paper).

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?

This is a good point and it would indeed be desirable to see what the effect of hygroscopicity would be on the seasonal cycle in aerosol activation. There are, however, few measurements of aerosol hygroscopicity available at Hyytiälä, all of which are for relatively short periods. Since the uncertainties in the cloud droplet retrievals are substantial, it would be hard to get a meaningful relation between hygroscopicity and aerosol activation for short periods from the available data.
In the MS we already discuss the uncertainty due to a seasonal variation in activation diameter as found by Sihto et al. (2010), due to chemical composition effects. This uncertainty is included as the error bars of Fig. 3b. Furthermore, the potential temperature gradient supports the hypothesis of updraft limitation. However, at low supersaturations related to low updraft velocities, the effect of the hygroscopicity becomes large. Therefore we cannot rule out its importance, but neither can we prove it. We have made this point more clear in the discussion by including the following statements:

p10016, line 22
'We acknowledge, however, that the effects of the chemical composition of the aerosols that serve as CCN should be studied further to clarify its role in the seasonal cycle in CCN-activation over the boreal forest. Especially under conditions of weak convection which results in low supersaturations the effect of the hygroscopicity could become important (e.g. (Dusek et al., 2006). This effect is already clear from the uncertainty due to a seasonal variation in activation diameter as found by Sihto et al. (2010). This uncertainty is included as the error bars of Fig. 3b.'

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

The term updraft velocities as we use it in our MS refers to the vertical velocity of a rising parcel that starts to condense when reaching cloud base. This velocity will then determine the super saturation reached in that air parcel and therefore the activation of the aerosols in the parcel. Measurements of this velocity are not available at this site. Measurements of vertical wind speed *w* in the surface layer are available from the Hyytiälä measurement tower. We checked whether this vertical wind speed corroborates the coupling between N_{CD} and atmospheric stability or not. We use the standard deviation of the updraft velocity (σ_w) since σ_w is a more reliable measure of vertical motions than the absolute values of the updrafts (Leaitch et al., 1996;Rosenfeld and Feingold, 2003). We found that the correlation between σ_w and N_{CD} is weak, but positive with r = 0.46. We added these findings to Sect. 3.2 and to Fig. 4 (Fig. 3 in the discussion paper, see below).

p10016, line 4.
'We also looked at the relationship between updraft velocities and N_{CD}. We use the standard deviation of the updraft velocity (σ_w) as measured at the SMEAR II station, since σ_w is a more reliable measure of vertical motions than the absolute values of the updrafts (Leaitch et al., 1996;Rosenfeld and Feingold, 2003). We found that the correlation between σ_w and N_{CD} is weak, but positive with r = 0.46. This weak correlation could be caused by the fact that we compare point measurements with spatial averages and that we use measurements in the surface layer to discuss activation at cloud base.'

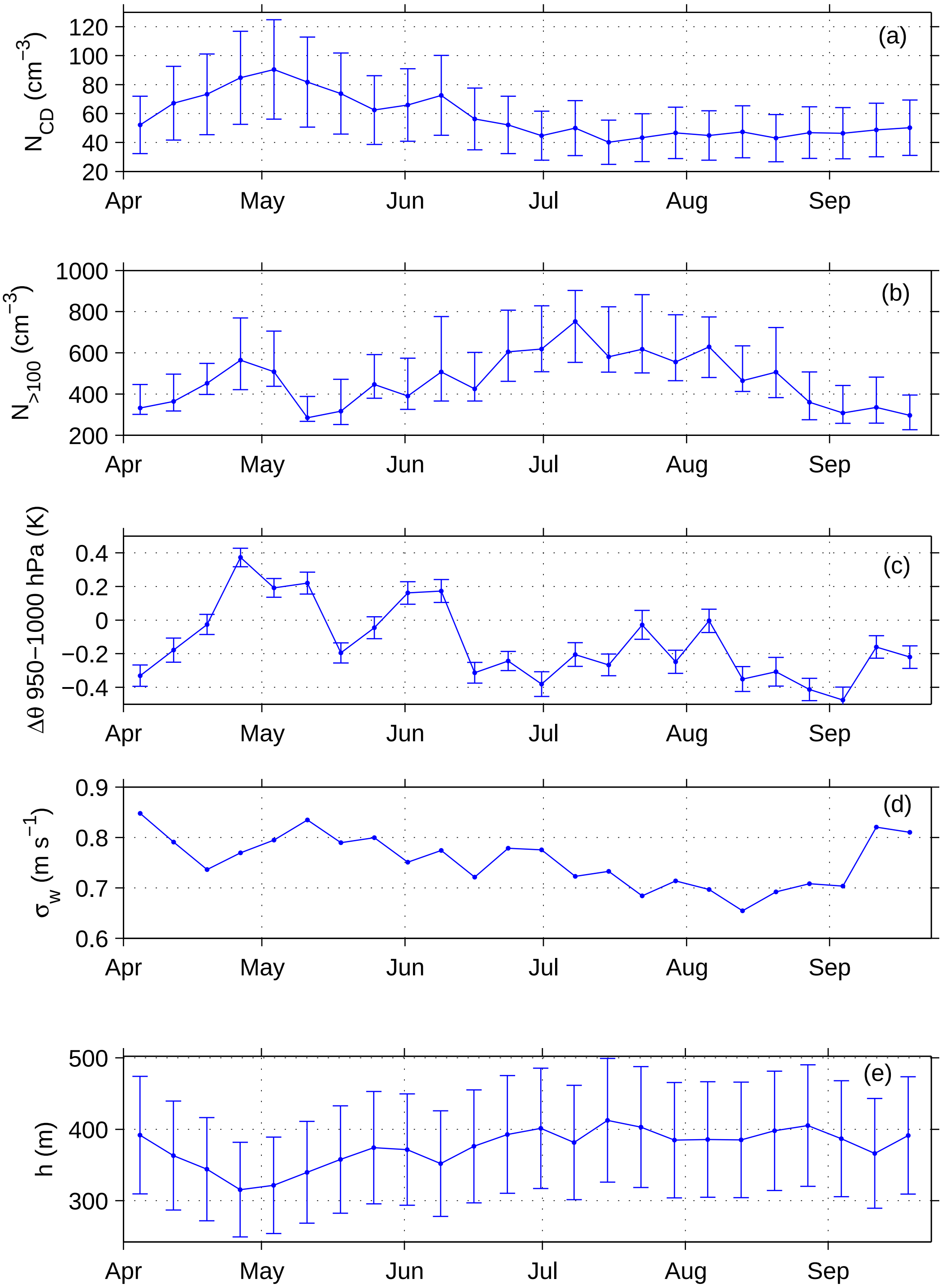

Fig. 4. Median seasonal cycle over 2000–2008 in **(a)** cloud droplet number concentration N_{CD}, **(b)** surface observations of CCN-proxy concentrations $N_{>100}$, **(c)** potential temperature

difference between the 1000 and 950 hPa-level $\Delta\theta_{1000-950}$, **(d)** standard deviation of the vertical wind speed σ_w and **(e)** cloud depth *h*. The errorbars in N_{CD} and *h* indicate the uncertainty as calculated in Sect. 2.4. The errorbars in $N_{>100}$ indicate the concentrations of aerosols larger than 80nm ($N_{>80}$, upper limit) and larger than 120nm ($N_{>120}$, lower limit), respectively, to account for the seasonal variation in critical diameter for CCN-activity of aerosols at Hyytiälä (Sihto et al., 2010). Errorbars in $\Delta\theta_{1000-950}$ designate the standard error. Meaning of datapoints as in Fig. 3.

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

We have indeed selected a low supersaturation because in stratiform clouds, lower supersaturations are expected than in convective clouds. However, the exact supersaturation for the clouds in this study is not known. The chosen supersaturation is similar to the supersaturation of 0.25% as used by Boers et al. (2006) for stratocumulus under weak convective conditions.

p.10013, line 14:
*"*The chosen supersaturation is similar to the supersaturation of 0.25% as used by B06 under weak convective conditions."

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?

The fact that our retrievals of cloud depth *h* on seasonal time scale are uncertain is a consequence of the uncertainty in some input parameters of the cloud model that are poorly constrained (subadiabatic fraction, adiabatic lapse rate of liquid water content mixing ratio, see also Sect. 2.4 on the uncertainty analysis and Fig. 1). Although the relative error that we calculate for *h* is smaller than that of N_{CD}, the larger numbers of *h* result in larger absolute errors. Note, however, that in another study good agreement is found between retrieved and observed cloud depth by Roebeling et al. (2008).

Cloud depth influences the cloud optical thickness and therewith cloud albedo. The effect of aerosols, however, on *h* is less well understood than that on N_{CD}. This means that also the effect of the latter on the cloud albedo is better understood, which was also discussed by Boers et al. (2006).
The following explanation has been added in the text:

p.10013, line 4:
'This large uncertainty is a result of the uncertainty in several input parameters of the cloud model, that vary on seasonal time scales, a point which was also reported by Boers et al. (2006). Roebeling et al. (2008), however, found good agreement between retrieved *h* from the cloud mdoel and *h* as observed by ground based observations. A changing *h* due to aerosol effects could in principle affect the cloud albedo, but this effect is not well understood and therefore we will not further discuss it.'

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?

This is an interesting point which is related to comment 7 of referee 1 (*Further discussion is needed about the differences between the activation ratio definitions. Perhaps the authors can show what would be the activation ratios in the cases where they have data about aerosol concentration, CCN concentration and calculation of the cloud droplet concentrations.*) and therefore we give a combined answer to these comments:

p. 10015, line 18:
'To illustrate the different activated fractions, we have calculated $F_{act(1)}$ and $F_{act(3)}$ for the period that we have data for N_A, N_{CCN} and N_{CD}, i.e. July to September 2008. Fig. 8 shows that N_{CCN} increases with increasing N_A. $F_{act(1)}$, which is the ratio of these, does not have a clear pattern over this period, but when looking at a longer period, Sihto et al. (2010) found a seasonal cycle in $F_{act(1)}$ at this site. The behaviour of $F_{act(3)}$ for this period is similar to that of the whole measurement period, showing little sensitivity of N_{CD} to $N_{CCN0.2}$. How $F_{act(2)}$ would behave, can be illustrated by the following limiting cases: 1) if CCN-activation is transport limited, meaning that few CCN are transported from the surface to cloud base, we would expect a high $F_{act(2)}$, since few CCN reach cloud base, but those that do are activated. 2) If CCN-activation is limited by the activation itself, many CCN reach cloud base, but few are activated, resulting in a low $F_{act(2)}$. In reality, these 2 effects will be combined, but based on our results we cannot make a separation between them.'

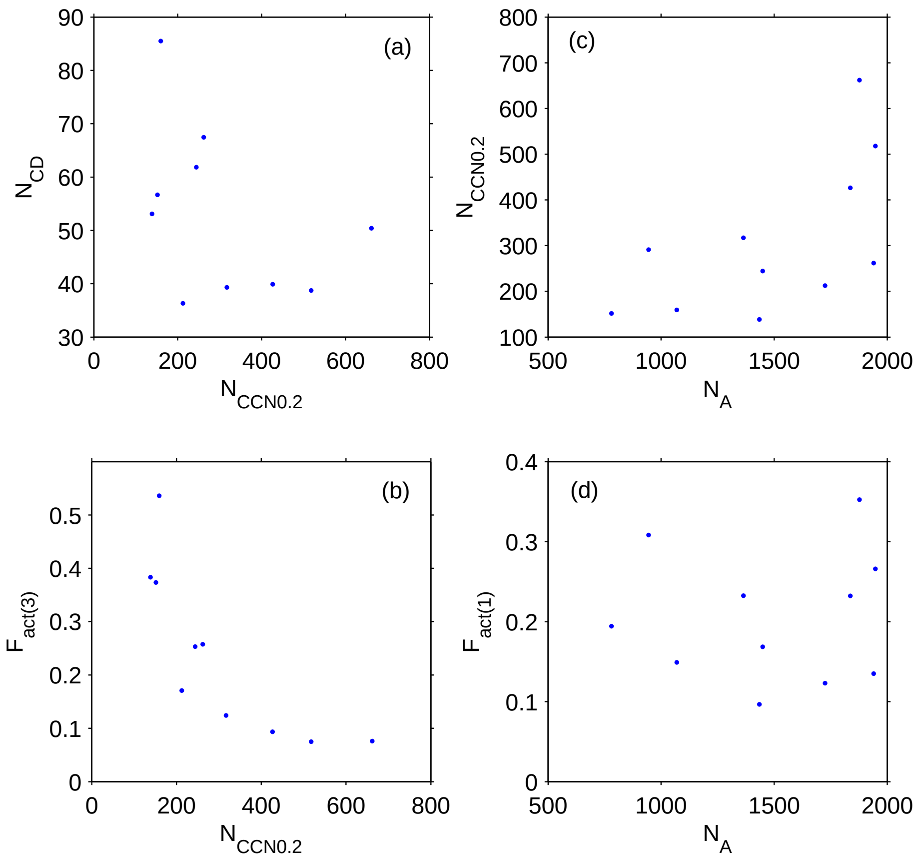

Fig. 8. The activated fraction F_{act} for the period July-September 2008, for which there are data available of aerosol concentration N_A, CCN-concentration at 0.2% supersaturation $N_{CCN0.2}$ and cloud droplet concentration N_{CD}. **(a)** Comparison of $N_{CCN0.2}$ and N_{CD}, **(b)** $F_{act(3)}$,

defined as the ratio of N_{CD} and $N_{CCN0.2}$, **(c)** comparison of N_A and $N_{CCN0.2}$ and **(d)** $F_{act(1)}$, defined as the ratio of $N_{CCN0.2}$ and N_A.

7) A map with the location of the measurement site and an indication of the 2_x2_ box could be helpful.

A map of the research area indicating the location of Hyytiälä and the 2x2^o box is included in the paper as Figure 1 (see below).

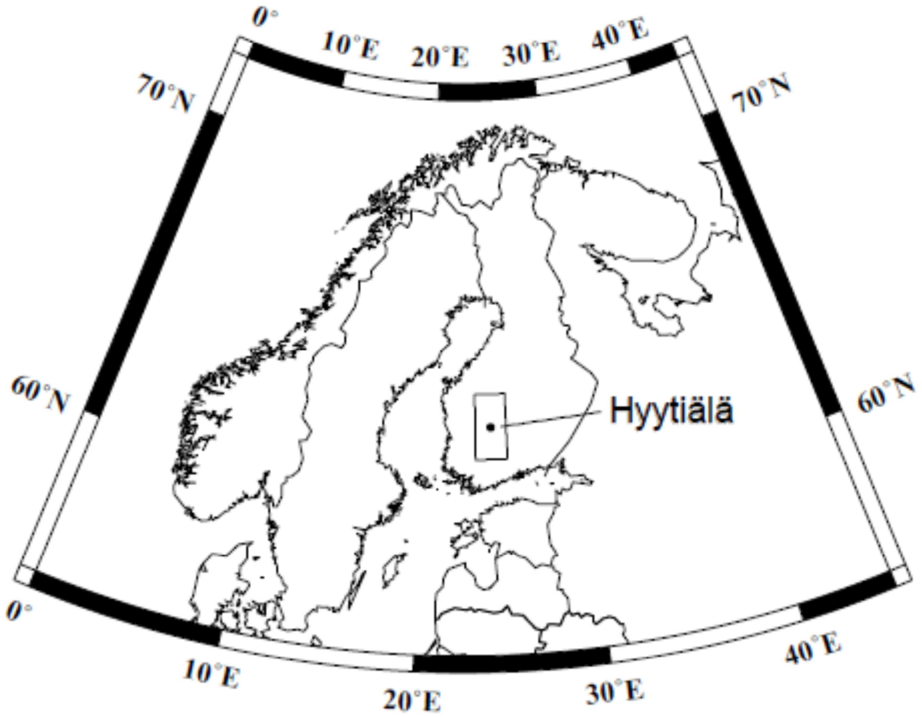

Fig. 1. Map indicating the location of the SMEAR II field station at Hyytiälä, Finland and the 2 x 2^o latitude-longitude box over which the MODIS and ECMWF-data are averaged.

Some minor remarks:

1) *p.10003, line 28: a comma between "surface data" and "an introduction" is missing*

A comma is placed between "surface data" and "an introduction".

2) *p.10004, line 11: what is meant with "both"?*

Since the listing consist of three elements (satellite, aerosol and meteorological data), "both" is removed from this sentence.

3) *Figure 3: Meaning of error bars for the cloud depth is missing.*

Explaining text added in figure caption: "The errorbars in N_{CD} and *h* indicate the uncertainty as calculated in Sect. 2.4."

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