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Interactive Comment

Interactive comment on "Investigation of the effective peak supersaturation for liquid-phase clouds at the high-alpine site Jungfraujoch, Switzerland (3580 m a.s.l.)" *by* E. Hammer et al.

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The authors would like to thank Referee 2 for the helpful and thoughtful comments and suggestions. All comments of reviewer #2 are addressed below. The reviewer's comments are in italics and our responses in plain text.

General Comments:

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This study uses aerosol particle and cloud data collected at Jungfraujoch to estimate the maximum supersaturations in liquid clouds impacting the mountain site. The subject is difficult, but the particle analysis is carefully done and the results will be a valuable addition to Atmospheric Physics and Chemistry. I think that the differences associated with clouds approaching from the steep mountain side versus the more slowly rising slope is a very nice demonstration of the effects of updraft speed, even if the approach produces an upper estimate. Some mostly minor comments follow.

Specific comments:

1) Referring to Fig 3b, on line 6 below equation 2 you say that the activated fraction approaches unity if there is no entrainment. However, if you only measure the total aerosol and the interstitial aerosol at any given time, is it still not possible that you see the activated fraction approach unity regardless of whether the cloud parcel sampled has been changed by entrainment?

Actually, we are referring to entrainment after particles have been activated. For clarification, we changed the sentence in section 3.3 as follows: "Small particles remain interstitial, while with increasing diameter the activated fraction approaches a value of approximately 1 (if no entrainment occurs after particle activation in the vicinity of the JFJ and no ice particles are present).

2) While "shallow-layer" clouds may be closer to adiabatic, mixing is typically strong within cumulus. If the mixing is homogeneous, then presumably your approach is relatively unaffected, but if the mixing is initially heterogeneous, could it be a significant factor in the lower peak supersaturations shown in Fig. 9? Another way to look at this

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would be to limit your data points to cases for which the measured LWC is near the adiabatic value determined from your cloud base estimate. Would that approach help with understanding the scatter in your Figure 9?

This is a very interesting idea. Unfortunately, we are not able to detect if there initially was heterogeneous mixing occurring or not. The cloud base was estimated using the measured LWC and thus we cannot retrieve the cloud base with an independent measurement as you suggested. Nevertheless, we added a discussion of this possibility in section 4.5.2 (last paragraph) as a further possible explanation for the scatter of Figure 9: "Particularly for the northerly wind cases, the model generally overestimates the SS_{peak} for a particular w_{act}. The exact reasons for this difference will be investigated in a subsequent study. However, here it can be speculated about three possible causes: Firstly, the estimated w_{act}, calculated from the horizontal wind speed at the JFJ with using Eq. (9), may overestimate the true updraft at cloud base due to flow convergence in the approach to the narrow gap in which the JFJ is located, or due to flow lines that do not strictly follow the terrain (entrainment of dry air can be excluded based on the cloud event filtering discussed in sect. 3.1). Indeed, reducing w_{act} for the observation based points by a factor of 5 would lead to a near perfect agreement with the modeled data. Secondly, an initially heterogeneous mixed air mass before occurring cloud activation would lower the SS_{peak} and thus lead to an underestimation of SS_{peak} for a particular w_{act}. Thirdly, turbulence is neglected in the simulation. However, considering turbulence in the model is expected to increase the SS_{peak} for a particular w_{act}, which would increase the difference between observations and model results even further. Thus, neglecting turbulence cannot be the sole reason for this difference."

3) Second page of Intro, lines 1-2: "Aerosol indirect effects depend on the number concentration of CCN as a function of supersaturation" is awkward.

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We changed it to: “Aerosol indirect effects depend on the number concentration of CCN. Whereat, the number concentration of CCN is determined by the aerosol number size distribution and hygroscopic properties as well as the present supersaturation.”

4) Section 2.1, line 26 – “the cloud base regularly rises and sinks vertically”. Does it truly sink or does the cloud base become lower?

We changed the sentence to: “the cloud base regularly rises and lowers vertically”.

5) Section 2.2, first line 6 – is volatilization due to the drying significant?

Volatilization of the volatile parts within the particles is indeed an issue due to the drying in the total and interstitial inlet. However, the study by Nessler et al. (2003) showed simultaneous dry and ambient measurements at the Jungfraujoch site. Nessler concluded that particle losses due to volatilization mainly concern small particles with a diameter below 100 nm. Referring to this study we can assume that volatilization has negligible effects on the results of our study.

6) Section 2.2 first line 18 – “for to”

Thanks. We changed it.

7) Section 2.2, first line 24 – I assume these differences are in sizing. Are there differences in sizing as well?

It is indeed based on sizing. The text was improved for clarity: “The interstitial inlet

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consists of a cyclone (PM2.5, Very Sharp Cut Cyclone, BGI, USA) which was used to remove droplets larger than 2 µm in aerodynamic diameter. It was operated at ambient temperatures (typically at -12 to 11 C; Henning et al., 2002) and at a flow rate of 20 lpm."

8) Section 2.2, second line 13-16 – I suggest "Cloud presence and liquid water content (LWC) were measured with a PVM-100 (Gerber, 1991) which employs a measurement principle that is based on forward light scattering by the cloud droplets." Was the PVM calibrated?

Yes, the PVM was calibrated in a period of about every 3 weeks using 1) a calibration plate that scatters the light corresponding to a certain LWC value and 2) clear sky conditions to perform a zero calibration. We added your suggestion to section 2.2 with the additional information: "The PVM-100 was regularly calibrated during the several campaigns using 1) a calibration plate that scatters the light corresponding to a certain LWC value and 2) clear sky conditions to perform a zero calibration."

9) Section 3.2, lines 13-14 – rather than "internally mixed aerosol", I suggest "the same or greater hygroscopicity".

We changed the sentence to: "Consequently, a certain supersaturation determines an activation threshold dry diameter (D_{act}) above which all particles of the same or greater hygroscopicity act as CCN."

10) Last paragraph of section 3.3 – the +/- 30 % is an experimental uncertainty, but there are other uncertainties based on the fact that you are making some assumptions.

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This should be made clear.

The variability of the kappa - Ddry-relationship from the 17 month climatology of CCNC measurements (Jurányi et al., 2011, shading in Fig. 4b) is also addressed in the 30 % uncertainty for SS_{peak}. Nevertheless, we added the following sentences: "Uncertainties based on the assumptions made in the five steps for retrieving SS_{peak} are not accounted for in the $\pm$30 % experimental uncertainty. However, these assumptions should not have a big influence on the overall uncertainty of SS_{peak}."

11) Section 4.2, line 8-9 – "besides ... processes" needlessly complicates this sentence.

We revised the sentence to: "The particle hygroscopicity parameter κ is required as a function of particle size for the approach applied in this study to infer the effective peak supersaturation of the clouds which were observed (see Sect. 3.2, step 3)."

9) Section 4.3, line 15 – 49 nm shows up as a 10 percentile value in the table. Does that mean that no values below 49 nm were measured?

We changed the sentence to: "The observed D_{act} values range between 49 and 195 nm (10th and 90th percentiles), which overlaps with the range of typical D_{act} values in liquid clouds reported in other studies (e.g. Lihavainen et al., 2008; Asmi et al., 2012; Anttila et al., 2012)."

10) Section 4.4, third lines 16-17 – the statement that larger droplet number concentrations (I assume that the first "condensation" on line 17 was intended to be concentration) results in a larger condensation sink assumes either no difference in

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LWC or that the situations being compared were adiabatic. This needs to be discussed.

We changed the sentence as follow: "...due to the increased condensation sink term, compared to a cloud formed at equal updraft velocity and equal LWC but with a smaller $N_{96--600}\ldots$".

11) Consider adding Leaitch et al (JGR, 1996) to your table 3.

The reference was added as follow:
low-level stratus |Gulf of Maine/Bay of Fundy |< 0.1 |Leaitch et al., (1996)

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