Atmos. Chem. Phys. Discuss., 13, C8234–C8236, 2013
www.atmos-chem-phys-discuss.net/13/C8234/2013/

Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

Received and published: 19 October 2013

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.

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

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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?

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 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?

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.

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?

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

6) Section 2.2 first line 18 – "for to"

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

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) that is based on forward light scattering by the cloud droplets." Was the PVM calibrated?

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

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. This

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should be made clear.

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

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

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 20419, 2013.