

Reply to the "Author's Response" and comments to the revised manuscript acp-2014-665 (version 3, uploaded July 16th 2015):

Sensitivity estimations for cloud droplet formation in the vicinity of the high alpine research station Jungfraujoch (3580 m asl)

by E. Hammer et al.

In the previous review, I have called for a major revision of the paper, pointing out:

1. **Limited reproducibility** due to lack of access to software and data the study is based on, as well as due to the lack of sufficient detail in the description of simulation input parameters.
2. **Lack of proper context** – even though the paper subject has been widely studied in the literature, the initial version of the manuscript lacked references to previous studies.
3. **Paper composition flaws** in the initial version of the manuscript.
4. **Ambiguities in the description of the methodology** that I tried to highlight by asking some specific questions.

In my opinion, the authors did address points 2 and 3 (as well as several points raised by the other reviewers) to an extent that makes the manuscript considerably better. Some of my specific questions from point 4 remain unanswered, and being convinced that clarifying these issues will improve the manuscript, I am repeating these questions in my comments to the authors' reply below.

There is no improvement with regard to the first point – reproducibility. The authors have added in the revised manuscript an e-mail address of the model author. I do not consider it any better than having an e-mail address of the corresponding author. There is no information on the software version. The provided references do not give information on how the considered equations are numerically solved. Let me quote the ACP guidelines again: "*Copernicus Publications encourages authors to also deposit software, algorithms, model code, and other underlying material on suitable repositories/archives whenever possible. These materials should be referenced in the article and preferably cited via a persistent identifier as a DOI.*"¹.

Replies to the authors' comments

In the comments below, I address selected points from the authors' reply. The comments below contain:

quotes from my original review doubly indented and typeset in blue;

indigo-coloured quotes from the authors' reply with single indentation;

my present comments in black and with no indentation.

... subsaturated growth of the aerosol is governed by the kappa-koehler equation (this difference is now mentioned in the paper). The kinetic uptake of water from the gas phase is done in a very standard way ...

Then, in my understanding, the κ -Köhler parameterisation has to be used as well above saturation.

If that is correct, please do not state that it concerns only subsaturated growth.

If that is not correct, please clarify how the aerosol solubility/composition is taken into account above saturation.

The meteorological data is provided by MeteoSwiss such that we are not permitted to make it publicly accessible. Nevertheless, all other data (such as aerosol parameters measured by the PSI) are available to the interested public by writing an e-mail to the contact author.

Please at least provide a persistent identifier and version information that can help to acquire the data from the mentioned entities. Please also note that according to the previously cited ACP guidelines, "*Authors are required to provide a statement on how their underlying research data can be accessed. This must be placed as the section "Data availability" at the end of the manuscript before the acknowledgements. If the data are not publicly accessible, a detailed explanation of why this is the case is required.*"

¹http://www.atmospheric-chemistry-and-physics.net/about/data_policy.html

the model timestep choice and integration method (how it copes with the stiffness of the drop growth equations? how it copes with the timestep requirement for simulating the small-scale fluctuations with frequencies up to 20Hz?)

we have added the following text to the model description section "At $S < 0.99$, the model time step is one second, and at $S \geq 0.99$, it is calculated such that the water content of the droplet can change by no more than 2% per time step."

First, I cannot find the added sentence in the revised manuscript.

Second, let me ask the authors again: does this condition result in a timestep value needed to resolve the response of condensation kinetics to the imposed 20 Hz frequency temperature fluctuations? Please mention it in the paper.

The very last summarising paragraph of the paper starts with a statement saying that "*small-scale temperature fluctuations are revealed to be the strongest effect on cloud formation process beside the updraft velocity, which is influenced by the temperature fluctuations.*"

- I suggest rephrasing the above-quoted sentence so it is made clear that the employed model actually does not differentiate the temperature and velocity fluctuations.
- Such sentence also calls for a comment and reference[s] clarifying if that is in agreement or not with previous studies (see e.g. the summary and references in the review article on "Growth of Cloud Droplets in a Turbulent Environment" by Grabowski and Wang, 2013).
- I also suggest supplementing the conclusions with a brief reiteration of the limitations of the employed modelling method: (i) assumed equilibrium state at RH=99%; (ii) no direct coupling between the latent heat release and the parcel heat budget; (iii) subjecting all particles to the same fluctuations.

Please also consider revising the abstract so that the list and priority of conclusions there matches the one from the final section of the paper. I suggest removing from the abstract the mention of the division by 4 of the velocity.

The paper reports on the sensitivity of cloud droplet activation process, in particular the sensitivity to the small-scale fluctuations of vertical velocity and temperature. This is a widely studied topic and the paper clearly lacks references to other studies discussing analogous tools, methodologies and results, e.g.: Clark and Hall 1979, Kulmala et al. 1997, Feingold 2003, Lance et al., 2004, Chuang 2006, Ditas et al. 2012, Partridge et al. 2012.

As of now, the discussion of the methodology and results is left without proper context. This also makes it hard for the reader to understand where the novelty of the presented results lies.

Thank you for providing these references. The reviewer is correct in that the introduction did not provide a detailed enough discussion of previous work. We have included a discussion of all of these papers in the introduction (sizable section of text not reproduced here), as well as in the discussion of figure 5, which shows similar features to what was found in the studies of Chuang, Feingold and Partridge.

Let me point out that the list of references I provided was composed having in mind the subject of the sensitivity of CCN activation models, and in particular the sensitivity to the small-scale fluctuations of vertical velocity and temperature. Yet, in the revised manuscript, it is presented as a list of studies dealing with modelling of CCN activation in general. Please reformulate the first sentence on page 4 so it includes the mentions of sensitivity and small-scale fluctuations.

Also, I do encourage the authors to fill in the gap in the referenced literature between 1979 and 1997.

Citing Kulmala 1997 in my previous comment, I provided a quote from the paper in J. Aerosol Sci 28, and not the one in Nature (that's just to ensure if the different choice was intentional).

the whole section 2.1.2 bears well too much similarity to section 3.4 (with the same title) from Hammer et al. 2014, ACP

We added a sentence at the beginning of 2.1.2 to make it clear that this section was put from Hammer et al. (2014): “(This section is composed by a summary of section 3.4 from Hammer et al. (2014).)” and removed “see detailed explanation in Hammer et al., 2014”.

My point was to encourage shortening and/or rephrasing it.

Further comments

- page 2, line 21: please add unit to 0.46 (Hz?) – last sentence of the abstract
- page 2, lines 13,23: repetition of “can significantly alter CCN activation”
- page 4, line 14: wrong tense for “find”
- page 5, line 18: “*To develop effective models ...*” – please specify what kind of models
- page 6, line 17: I suggest moving the paragraph with reference to Table 1 from section 2 (Methods) to the end of section 1 (Introduction) and extending it to a brief description of the paper structure.
- page 7, line 14: κ parameter already mentioned while the work of Petters & Kreidenweis is cited only on page 10
- page 8, line 10: there is a “when when” repetition
- page 8, line 11: “updraught” spelling, while elsewhere “updraft”
- page 10: section 2.2 contains just a single subsection 2.2.1 – please skip subsectioning here
- page 10, line 17: isn’t there a cause/effect mismatch in “*Water removal due to precipitation is negligible since it is assumed that the total water content is preserved*”
- page 11, line 11: please specify that the assumption refers to the dry size spectrum
- page 11, line 26: “i.e. equation” seems unneeded?
- page 12, line 13: what does “*but*” refer to in the last sentence of section 2.2.1? (also probably replacing “fast” with “fastest” will match authors’ intent)
- page 14, 20, sect 2.3.4: sensitivity of κ on SS should probably be changed to sensitivity of SS on κ
- page 15: title and content of section 2.4: please replace “reference model” with “reference simulations” – the model is the same
- page 16: is “solid” (i.e. non-dashed) meant when referring to “black” lines?
- page 16, line 15: isn’t it the velocity that has an effect on SS, and not the other way round?
- page 18, line 15: please rephrase “Thus, for the sensitivity...” to something like “To assess the sensitivity...”
- page 19, line 11: I suggest removing the “describing the Raoult term of the Köhler equation”
- page 20: last paragraph: please rephrase so that the reader is aware what “ratios” the text refers to
- page 22 (vs. 10): ZOMM acronym is differently deciphered
- page 23, line 27: unit missing for 0.46
- references:
 - please differentiate between the two Hammer et al. 2014 papers when citing (e.g.: 2014a, 2014b)
 - Clark and Hall 1979 entry is out of alphabetical order in the reference list
 - the Gerber 1991/JAS reference should likely be Gerber 1991/Appl. Opt., 30

Hope that helps!

Editor comment: While I think you have addressed well all comments by Reviewer #2 and #3 in the response to the reviews, there seems to be a mismatch of the response and the revised manuscript. I cannot find the following text fragments (that you had marked in blue) in the revised manuscript.

Reviewer #2:

Specific comments

1) Page 25970, Line 1–9: I would recommend reorganising this paragraph, because duplicated information is given in the 1st and this paragraph, and the flow and the connection with the previous paragraph are just not very good.

... effective peak supersaturation (SS_{peak} ; Hammer et al. 2014). Small-scale fluctuations in vertical velocity can alter the cooling rate of an air parcel and thereby also the corresponding SS_{peak}

3) Page 25974, Line 2: Could the authors please explain why a 6-min time period is chosen? How sensitive is the overall result to the averaging time period?

“(…) given in six minute averages. The six minute periods were chosen according to the instrument with the lowest time resolution which is the SMPS instrument measuring the dry particle size distribution.”

7) Page 25977, Line 11: Similarly, Reasons for choosing 2%?

This was a relatively arbitrary choice, balancing calculation time with accuracy. Reproducing the exact value would have been theoretically possible, but would have required impractical amounts of computer time. “..., which was considered to be sufficient for the determination of SS_{peak} values, without consuming impractical amounts of computer time.”

10) Page 25981, Line 5–7: I am afraid that I don't understand what the authors mean.

We rephrased to make it clearer: “In Sect. 2 it is described that the topography at the JFJ defines two main wind directions, NW and SE wind. As shown by Hammer 2014, the particle number concentration and size measured at the JFJ differs between these two wind directions, with more and larger particles being measured during SE wind conditions. The variability of number and size is smaller within data collected from a single wind direction than the difference between the two wind directions. Therefore, we test the influence of particle number and size by varying these parameters over a similar range as the difference between values measured during SE and NW wind conditions.

11) Page 25981, Line 23–38: I am not sure how “updrafts are generally smaller: and only the largest particles activate” explain “more pronounced effect at low effective peak supersaturation. Could the authors please elaborate on this a bit more?

We rephrased to make it clearer: “This can be explained by the fact, that changing the size of the particles, changes the minimum supersaturation at which the particles can activate. At low SS, updrafts are generally smaller (colour coding in Fig. 6), and only the largest particles activate. At these large size ranges, usually a low particle number concentration is present and therefore if the particles are smaller (larger) SS_{peak} will be higher (lower). At higher $SS_{peakref}$, where the updrafts are generally higher, the critical saturation of the largest particles plays less of a role in determining the SS peak.”

Editor comment: The only difference between this text and the text in the original manuscript is the beginning where 'This is because' was replaced by 'This can be explained by the fact', but even this change is not reflected in the revised version. In addition, I would like to see a more thorough response to the reviewer's comment.

Editor comment: Also, here I cannot find the following changes in the revised manuscript.

Reviewer #3:

Page 25979, line 7

Please expand on why these two updraft velocities are so different. This is a very important point since the remainder of the paper only relies on the modelled updraft velocity.

We added: "... than the estimated $W_{actestim}$. As discussed in Hammer et al. 2014, The reason that the modelled W_{act} is much lower than the estimated value is possibly due to the air mass being accelerated as it passes through the narrow pass where the JFJ is located. This may lead to higher windspeeds being measured at the JFJ than those which were actually present when the cloud was formed at a lower altitude. To investigate the sensitivity ..."

Page 25975, line 17

To what fluctuations are you referring? This sentence is vague.

We changed it to: "To investigate the importance of the small-scale fluctuations of SS to the decrease ..."

Editor comments

In addition, I have a few more comments you should consider in a revised manuscript. (page and line numbers refer to the manuscript without marked changes)

p. 2, l. 18: do you mean 'if SS_{peak} is between 0.2-0.4%' ? (Otherwise, it might be read as 0.2-0.4% being the 'maximum influence')

p. 2, l. 20: independently of

p. 4, l. 14: find → found

p. 4, l. 18-p. 5, l.27: I appreciate that you added more references and context to the introduction. However, a more thorough discussion and summary of the previous findings might be better. For example, you mention several times that various studies found highest sensitivity for polluted conditions. I suggest a more structured discussion instead of listing each finding from individual papers separately.

p. 6, l. 17: 'To make the readability of the manuscript easier' is an odd expression. Better 'For clarity' or simply remove this fragment.

p. 7, l. 24: If rosemount is a manufacturer name, it should be capitalized

p. 8, l. 9: Do you mean 'In the current study' by 'In here'?

p. 8, l. 9-13: Something seems wrong with this sentence. Maybe the confusion arises because one of the 'when' (l. 10) should be a verb?

p. 8, l. 15: This is an odd expression in a paper. Is this paragraph taken from Hammer et al. (2014) or rephrased? If the former, it should be shortened; if the latter, simply add the reference at one or two places in the text.

p. 9, l. 17: lead → leads

p. 9, l. 19: hygroscopically grown

p. 9, l. 22: is there an 'of' missing between 'comparison' and 'SS(peak,mod)'?

p. 10, l. 3, 4: Since there is no Section 2.2.2, the headers should be combined and only a Section 2.2. should exist.

p. 10, l. 17/18: Shouldn't be this sentence the other way around? It is assumed that the total water content is preserved, since water removal due to precipitation is negligible.

P. 11, l. 16: ...size distributions at/below the cloud base? (Word missing)

p. 11, l. 19: Before, the campaign was only called 'CLACE', not 'CLACE2011' – please use consistently one of them

p. 12, l. 14/15: I don't understand this sentence.

p. 12, l. 25: Add space between 'Fig2' and 'with'

p. 13, l. 10: This sounds odd, as if a model run had been detected.

p. 14, l. 23/24: The two papers should be cited as (Hammer et al., 2014a, b). 'a)' should be the paper that is first cited in the manuscript, 'b)' the other one. Make sure, that throughout the whole manuscript you refer correctly then to either paper.

p. 16, l. 5-8: This sentence seems very convoluted. Either restructure it or split into two.

p. 18, l. 26: 0.4%

p. 19, l. 26: 'Köhler theory' should be 'Köhler equation' as a theory does not have terms.

p. 22, l. 19: Specify 'it' – SS_{peak} or the updraft velocity?

p. 23, l. 27: 0.46%

p. 24, l. 7: Should 'influenced' be 'influential'?

p. 24, l. 5-10: The last sentence seems repetitive with the first one.