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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity estimations for cloud droplet formation in the vicinity of the high alpine research station Jungfraujoch (3580 m a.s.l.)" *by* E. Hammer et al.

Anonymous Referee #2

Received and published: 16 December 2014

Using box model simulations and measurements at a high alpine station, the manuscript investigates the sensitivity of effective peak supersaturation to several factors such as updraft velocity, particle size distribution, hygroscopicity, and turbulence.

General comments:

The objective defined on Page 25969, Line 17–24 is relatively incremental and does not bring in the big picture. In particular, the manuscript starts with pointing out that it is important to understand aerosol-cloud interactions and to reduce uncertainty in aerosol radiative forcing, but how this work can be applied to address this overall goal

is not clearly put. Why is it important to understand how effective peak supersaturation varies with those factors? Based on the finding, do the authors conclude that climate modellers should consider including these effects? Why and why not? I believe that the authors have very specific science questions in mind, but it would help readers to put this work into the context if those science questions could be better defined and described.

The majority of the figures are about the ratio of effective peak supersaturation in "modified conditions" to that in control run. What is missing yet important question to ask is: do we need to care about a ratio of 1.1, 1.2, or we only need to worry if the ratio goes up to 5 or 10, for example? What are their consequences? It would be extremely valuable for both readers and the authors if a clear implication and path forward can be provided in the manuscript. Additionally, since this manuscript is about sensitivity, I feel that the authors need to provide stronger/more rigorous justifications about certain choice/threshold used in the paper.

Generally, the manuscript can be written more concisely and can be structured a bit better. Quite a few bits are disorganised; some bits of text are duplicated and interrupted the flow. I would suggest that the authors take another careful look at the manuscript and reorganise some awkward paragraphs that seem to be misplaced somehow.

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.

2) Page 25971–25972: It is OK to list/explain a number of measurements in 2.1.1 one by one, but it would be much better if certain connections and reasons behind these measurements can be given in this section, so readers can start linking these measurements with model input. For example, which measurements are exactly used as model

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input? Additionally, it wasn't clear why suddenly temperature, pressure trajectory, and the regime of 90% needed to be calculated. Readers could figure out eventually, but this kind of connection is sort of the authors' responsibility to make it clear.

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?

4) Page 25975, Line 13–15: While this manuscript focuses on effective peak supersaturation, how to measure it is not mentioned until Page 25977 with a very short statement that refers to Hammer et al. (2014). I would recommend providing a brief review in Sect. 2.1.1, because this variable is the key of the manuscript!

5) Page 25976, Sect. 2.2.2: I found this section is disorganised and it is quite hard to understand what the authors try to convey. Could the authors please consider using a schematic illustration or Figure 2 to explain/link Eq. (3) and a lot of variables in the text? Additionally, a very minor suggestion – it doesn't mean anything for readers if the model run is #516 or #1. If this number has a specific meaning or important implication, perhaps the authors could clearly describe it. Otherwise, I would suggest removing the number to make the manuscript read better and more concise.

6) Page 25977, Page 3: Could the authors please explain why choosing to find the highest water vapour saturation which lead to droplets larger than 2 microns in diameter? Any physical basis for the choice of 2 microns?

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

8) Page 25977, Line 24: Could the authors please clarify "what" exactly is independent of w here?

9) Page 25981, Line 1: Could the authors please describe what kind of w/Ncn range is here?

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

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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?

12) Page 25984, Line 4–6: Could the authors please provide proper metrics to support the evidence of “Improves the relationship”?

13) Page 25996, Figure 6 and related text on Page 25981 and 25982: The way of writing could be misleading – one may thought that the changes in diameter AND in number concentration are made simultaneously, which I don’t think is true. The authors may wish to consider rewriting it more precisely. Additionally, since the observations support higher number concentration AND larger particles, it would be interesting to demonstrate when these two factors are combined, how will effective peak supersaturation change?

14) Figure 8: Could the authors please explain what causes the spread of the ratio at a given effective peak supersaturation? Why a factor of 5 and 10 is a reasonable choice for sensitivity test?

15) The authors may wish to be consistent to use either “peak effective supersaturation” or “effective peak supersaturation” throughout the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25967, 2014.