Review for Hammer et al., ACPD, 2014

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General Comments

This study uses a data set of aerosol and cloud residuals measured at the Jungfraujoch station during summer 2011, and is an extension of the analysis conducted by the authors in an earlier publication (Hammer et al., *Atmos. Chem. Phys.*, 14, 1123–1139, 2014). In this manuscript, the authors attempt to relate the updraft velocity to the effective supersaturation of a cloud parcel, neither of which can be measured directly. As a result, different approaches are used to estimate these two parameters, primarily through a box model but also using basic assumptions about the vertical structure of the atmosphere. The latter is found to be inconsistent with the results from the box model and the authors therefore focus on the former results simulations. A sensitivity analysis using the box model reveals that the supersaturation achieved in the air parcel is much more sensitive to the parcel's updraft velocity than to the aerosol's size and hygroscopicity. An interesting finding is that the discrepancy between the variability in the model output and the 25th to 75th percentile of the overall model trajectories for the air parcel is reduced if a turbulent component is added to the updraft velocity. This finding emphasizes that the peak supersatuation within an air parcel can be highly sensitive to short-lived variations in updraft velocity.

One of the biggest issues with this study is that it uses the box model to fine tune a calculated updraft velocity based on the observations, and then, as far as I understand, compares it to the effective supersaturation determined by the same box model using the same trajectory. It is therefore not surprising that the comparison with the model improves when the same model is also used to predict all the parameters. Of course we can still learn a lot from the model output, but the text should clearly state which parameters were derived from the model versus derived from basic atmospheric assumptions. This would allow the reader to understand the significance of the findings.

This lack of clarity is mostly because of two technical issues. The first is that this study relies heavily on the results from the authors' previous study, but it does not explain how all the parameters were calculated. This makes it difficult for the average reader to follow how an estimated updraft velocity is different from a model updraft velocity, for example. (Note - to me, both of these values are modelled, only one uses a much more sophisticated model than the other.) This is not appropriate for a stand-alone paper and can be easily remedied by additional text briefly, but thoroughly, describing the key parameters derived in the previous study and used in this one. The second technical issue is that the notation is difficult to follow and I found myself needing to make a table just to keep track of all the variations of the parameters. If the authors could provide a table that logically grouped the parameters based on their origins, this would greatly increase the readability of the manuscript.

Based on this, I would recommend that this paper be accepted subject to the important, but minor, revisions stated above, and the specific comments below.

Specific Comments

Page 25972, line 12

You should state that you are assuming that none of the water is lost to precipitation and reasons to back up this assumption.

Page 25974, line 11

What is the estimated time between droplet activation and the observation site? Would you expect coalescence to occur?

Page 25972, line 26

What is the basis of this wet adiabatic lapse rate? Are there no observations that you can use?

Page 25976, line 7

The last statement on this line seems unintuitive. Normally one would think that if your model does not account for latent heat, then the wet adiabatic lapse rate would be the one that would be unnecessary.

Page 25977, line 5

A brief, but thorough, description of the calculations used to determine SS_{peak} from measurements should be in included here. It is unfair to the reader to expect that they have the manuscript from the previous study readily available.

Page 25978, line 13

How does using an average κ affect your results? It is unclear from the previous description that you are even using a varying κ. For this reason, it is important to include a description of your calculations, as mentioned above.

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.

Page 25979, line 12

It is not surprising that the modelled data points are closer to the model simulations for the median case. It is true that the signal is clearer between SS_{peak} and w than in the previous study. However, the results presented here reflect the model, and in fact, our previous understanding of updraft velocity and supersaturation. This figure does not really reveal any new understanding that is not already represented in the model.

Figure 4

Since you are studying the effects of updraft velocity on peak supersaturation, the axes on this figure should be reversed.

Page 25979, line 11

How was SS_{peak}^{estim} derived from measurements?

Page 25979, line 19

Shouldn't the black lines by definition run through the green points since they are the median? While the black lines do seem to fit the green points better, the residuals are by no means centred around zero. The bias in the fit, and possible sources, should be discussed in this section.

Page 25979, line 27

The points shift down in your current figure.

Page 25981, line 7

Was κ also kept constant over size?

Section 3.3

These results are really quite interesting. It would be worthwhile to consider moving this section earlier so that it is not passed over by an inattentive reader.

Page 25984, line 4

To which of the modelled values are you referring?

Page 25984, line 5

This is really quite remarkable. What percentage of the points now fall within the 25th and 75th percentile lines?

Technical Comments

Page 25972, line 15

Change to "the ideal gas law and the Clausius-Clapeyron equation".

Page 25974, line 21

Change "can not" to "cannot".

Page 25975, line 17

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

Page 25975, line 23

Remove the comma between "that" and "which".

Page 25976, line 3

Change "while" to "where".

Page 25976, line 9

Change "was ranging" to "ranged".

Page 25976, line 16

Sentence should be "According to Köhler theory".

Page 25976, line 25

Your wording of "corresponds to the SS_{max}^{mod}" suggests that this variable has been used before, whereas you are actually introducing it here.

Page 25978, line 9

Consider changing the title of this section to "Reference model for sensitivity analysis" so that the reader can easily refer back to this section later.

Page 25978, line 12

This sentence should read "For this purpose" if you are referring to the reference simulation.

Page 25979, line 1

Figure 4 is mentioned before Fig. 3.

Page 25984, line 17

Change wording to "is faster than the time".

Page 25984, line 18

Remove "also" from the sentence to make it less awkward.

Page 25984, line 25

Consider removing "being able" from the sentence.

Page 25985, line 18

Change “indicates” to “results in” or “causes”.

Page 25985, line 22

Consider changing the sentence to “particle size had a stronger influence on”.

Page 25986, line 7

Remove “presumably” from the sentence.

Page 25986, lines 10–11

Consider changing the text to “are more strongly influenced by small-scale varations. The decreasing influence...”

Page 25986, lines 16–17

The present tense should be used to emphasize the results.

Page 25986, line 22

Change the text to “independent of the amplitude”.