Response to Anonymous Referee #2

We thank the reviewer for the helpful and detailed comments. We appreciate the time she/he took in reviewing our paper. Comments are summarized and responses are in italics below.

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.

We reorganized the paragraphs as suggested in the specific comments.

We agree, that some information was not clearly put into the big picture: 1) why it is important to understand the variation of the effective peak supersaturation, 2) why the modellers should consider these effects, 3) what the importance of the range of the ratios says and 4) more rigorous justifications about certain choice/threshold used in the manuscript. Thus, we implemented this into the manuscript as follow:

To answer the other reviewer's question, we deleted the last paragraph in Sect. 1 but added to make 1) and 2) clearer: "To develop effective models it is important to know the influence of the variation of several key aerosol parameters influencing the cloud droplet formation. It has been pointed out by Boucher et al. (2013) and Spichtinger et al. (2008) that the main uncertainties in the aerosol radiative forcing are due to aerosol-cloud interaction dynamical factors such as turbulent strength and entrainment controlling the cloud condensation rate, and the key aerosol parameters such as aerosol number concentration and size distribution, and to a much lesser extent, the composition. I.e. the interplay of dynamics versus effects purely attributed to aerosols remains highly uncertain. Thus, in this study the influence of the variation the turbulent strength and the updraft velocity on the cloud activation is investigated using a cloud parcel model."

To answer 3) we added in Sect. 4, Conclusions at the very end: "Summarizing, small-scale temperature fluctuations are revealed have the strongest potential effect on cloud formation processes . The variation of aerosol number concentration and hygroscopic properties had a lesser influence than the aerosol size.

To 4):. The ratios chosen for the updraft velocity (2 and 5) were not properly discussed in the manuscript. The mean of the modelled updraft velocities is ~1 ms^{-1} with a maximum value of 5.6 ms^{-1} and a minimum value of 0.03 ms^{-1}. The maximum value of updraft velocities deviating from the mean therefore is ~5 (5 times 1). We added in Sect. 3.2: "The ratio 5 describes the maximum deviation from *the mean value of w_{act}^{mod} and the ratio 2 is given from the 75th and 25th percentile of w_{act}^{mod}, which are about a factor of 2 from the mean value."*

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.

We deleted the according paragraph and added some sentences of it to the 1st paragraph as follow: "(…Cherian et al.,2014;Dufresne et al.,2013;Levy et al.,2013). It has been pointed out by Boucher et al. (2013) that the main uncertainties in the aerosol radiative forcing are due to aerosol–cloud interaction dynamical factors such as turbulent strength and entrainment controlling the cloud condensation rate, and the key aerosol parameters such as aerosol number concentration and size distribution, and to a much lesser extent, the composition.

… effective peak supersaturation (SSpeak; 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},."

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 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.
We added the following sentence at the beginning of section 2.1.1
A number of quantities measured at the JFJ were either used as model input directly, or were used to calculate model input parameters. These included the aerosol size distribution, the temperature and pressure, wind speed and direction, and the total water content of the air.

We also agree that the description why the regime of RH=90% was calculated is a bit out of context. We deleted this paragraph as the wish from another reviewer:

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?

The 6-min period was chosen according to the duration of measuring one size distribution with the SMPS..The main influence this time resolution has on the results is on the number of points in the plots. A longer averaging period would have resulted in fewer model runs being performed, and thus probably also a smaller range of results simply due to the lower probability of capturing extreme values. There would be no systematic shif t in the results though. We added an explanation about that to the according sentence:
"(…) 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. "

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!

According to your suggestion we added a brief review of the effective peak supersaturation in Sect. "Effective Peak Supersaturation.: The SS_{peak} was retrieved as follow: 1) the activation threshold diameter was determined from the measurements of the total and interstitial number size distributions 2) the aerosol hygroscopicity was obtained from the simultaneous CCNC measurements 3) the activation threshold diameter was combined with the aerosol hygroscopicity to infer the effective peak supersaturation. A relative uncertainty of about 30% was estimated for SS_{peak}. A detailed description *how the SS_{peak} was estimated from the measurements performed at the JFJ can be found in Hammer et al. (2014)."*

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.

We tried to link Figure 2 with Eq. (3): "This was done by superimposing a time series of temperature fluctuations measured at the JFJ upon the linear temperature trajectory along which the model was run (see black line in Fig. 2). The time series of fluctuations was chosen to be simply that, which was measured at the JFJ during the time taken for the air parcel to ascend from the point where the model was initialized (indicated in Fig. 2 with RH=90%), to the JFJ (indicated in Fig.2 with JFJ)."

There is no specific meaning for the model run Nr. Thus, we removed the number from Fig. 2 and have rewritten the last sentence in Sect. 2.1.1. as follow: "… for the model run detected at the JFJ on 8 August 2011 18:20UTC."

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?

According to several cloud studies, e.g. Juranyi et al. (2011) and Henning et al. (2002), a diameter of 2 microns is a good threshold to distinguish hygroscopic grown particles from cloud droplets.
We therefore added: "(…) larger than 2 µm in diameter. In earlier studies it was found that a diameter of 2 µm is a good threshold distinguishing the hygroscopic grown particles from activated cloud droplets (Jurànyi et al., 2011; Henning et al., 2002)"

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."

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

We rephrased the sentence to make it clearer as follow: "The aerosol-limited regime is characterized by a relatively high ratio of w/N_{CN}, by a high activated fraction of aerosol particles (larger than 90%) and the aerosol-limited regime is basically independent of w."

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

We added: "(…) is lower. Thus, the ratio of w/N_{CN} at these low SS_{peak} values is relatively low (at about 0.003) and is increasing with an increase in SS_{peak}^{ref}. (up to about 0.03)"

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_{peak}^{ref}*, 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_{peak}^{ref}*, where the updrafts are generally higher, the critical saturation of the largest particles plays less of a role in determining the* SS_{peak}*.”*

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

We added: “… slightly improves the SS^{fluc}_{peak}*–*w^{mod}_{act}*-relationship at lower updraft velocities as can be seen in Fig. 9. At updrafts of 0.1 to 5* ms^{-1}*, the* SS_{fluc}^{peak} *to* W_{mod}^{act} *relationship is improved slightly, with 44%of the points lying within the range of the 25-75th percentile of the measured values, compared with 40% when fluctuations are not included”*

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?

To make it clearer we added: “… (see Fig. 6). The effects of changing the particle number size distribution and the particle number concentration were investigated separately.”

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?

We added the following to describe the spread of the ratio: “Nevertheless, there is also a spread of the ratio at a given \text{SS}_{\text{peak}}^\text{ref}. This is explained by the variable nature of the temperature fluctuations – at the point where aerosol activation occurs, the cooling rate will sometimes be greatly modified by the temperature fluctuation, in some cases it will be rather close to the average cooling rate. In the latter case, the SSpeak from the simulation including fluctuations will be close to the SSpeak calculated from the reference simulation ...”

We believe that the reason for the factor of 5 and 10 is already well described with the following sentence: “The factors 5 and 10 are resulting in a similar range of temperature amplitudes used for the sinus curve simulations described in Sect. 2.2.2.”

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

We checked that and use “effective peak supersaturation” throughout the paper.