# Response to Anonymous Referee #1

**We thank the reviewer for the very helpful and detailed comments. We greatly appreciate the time she/he took in making such a detailed review of our paper, and we believe that the manuscript has been substantially improved as a result of these suggestions. Comments are summarized and responses are in italics below.**

**1. Limited reproducibility**

Following the ACP guidelines, “A paper should contain sufficient detail and references to public sources of information to permit the author's peers to repeat the work” and “the data should be held in persistent public repositories”.
The paper does not fulfill either of those requirements. There is no information as to where the observational data can be accessed. There are no pointers to the source code of the programs used to obtain the results. The model input parameters are not described in sufficient detail to reproduce the simulations independently.
The numerical model used is said to be described in:

- Luo et al. 2003, GRL 30: “Dehydration potential of ultrathin clouds at the tropical tropopause”,
- Hoyle et al. 2005, JAS 62: “The origin of high ice crystal number densities in cirrus clouds”,
- Hoyle et al. 2013, ACP 13: “Heterogeneous formation of polar stratospheric clouds...”.

The first reference should probably read “Luo et al. 2003, JGR 108" but anyhow none of the two papers contain formulation of the model presented in a way allowing result reproducibility (i.e. governing equations, numerical schemes). The second reference also lacks full model formulation as it mentions that “The details of the model are given in (Luo et al. 2003)”. The third paper does feature a full-length section on the model formulation, but as the titles of all mentioned papers suggest, it is focused on ice microphysics, and contributes little to the reproducibility of the presented study.

*As mentioned in the author’s response on 30 September 2014, we are unable to place a copy of the model online (the author of the original model version is not happy to do so).*
*Nevertheless, an interested reader may contact Beiping Luo for a copy of the model if they wish to reproduce the results. A statement to this effect has now been added to the manuscript text.*
*The current model operates in the same way as described in the references above, with the exception that solid phases (e.g. ice) cannot form, and that the 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, and we now direct the interested reader to chapter 12 of Seinfeld and Pandis 2006, which describes the approach used in the model, and provides all the equations necessary to re-create this part of the model.*
*The meteorological data is provided by MeteoSwiss such that we are not permitted to make it publicly accessible. Nevertheless, all other data (such as aeorosol parameters measured by the PSI) are available to the interested public by writing an e-mail to the contact author.*

*We corrected the reference from Luo et al. 2003, GRL 30 to Luo, B. P., C. Voigt, S. Fueglistaler, and T. Peter, 2003: Extreme NAT supersaturations in mountain wave ice PSCs: A clue to NAT formation. J. Geophys. Res., 108, 4441, doi:10.1029/2002JD003104.*

Calculation of supersaturation in the model is central to the presented study. Yet, the given sources do not provide information on:

- how the supersaturation budget is calculated in the parcel (with prescribed temperature evolution and the latent heat release not accounted for);
- 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 >= 0.99, it is calculated such that the water content of the droplet can change by no more than 2% per time step.”*
- the form of drop growth equation used (are the latent heat effects accounted for here? how the molecular/ continuum regime transition is accounted for?).
  *This has been answered in the response to questions above, and the appropriate text has been added to the model description section.*

*We have added the following text to the model description section: "After the droplet growth is calculated, the total water content of the droplets is deducted from the total water content of the air parcel, and the saturation of the gas phase is re-calculated. The total water content (i.e. gas plus condensed phase water) is conserved throughout the simulation." And "The latent heat release due to condensation of water is accounted for implicitly in the use of a moist adiabatic lapse rate to calculate the temperature trajectory along which the model is run."*

None of those papers refer to the $\kappa$-Köhler parameterisation used in the present study. Few model constants are explicitly mentioned in all four papers combined, and some of those assumptions should be discussed in the context of a sensitivity study (e.g. the mass accommodation coefficient of unity as mentioned in Hoyle et al. 2013). I do not mean that the model formulation/documentation should be part of this very paper. It can be published, for instance, in an e-print repository like arXiv, but giving the readers access to it is essential to *permit the author's peers to repeat the work*. Let me underline that, reproducibility principles aside, access to the model documentation and code would bring answers to several question listed below.

*As mentioned above the model ZOMM so far was only used for ice microphysics, thus, the $\kappa$-Köhler parameterisation was implemented in the model for this study. We described this implementation in Sect. 2.2.1 with reference to Petters and Kreidenweis (2007).*

**2. Lack of proper context**

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 with remarks on the deficiencies of the approach to "simulate the effects of turbulent mixing by applying a highly time-dependent w to a Lagrangian parcel calculation of condensation growth";
- Kulmala et al. 1997 with investigation on "the effects of fluctuations of saturation ratio on droplet (cloud condensation nuclei) growth by stochastic approach employing an advanced growth model for cloud droplets";
- Feingold 2003 where "an adiabatic parcel model has been used as a tool to investigate the relative sensitivity of the radiatively important cloud drop effective radius to [...] parameters such as updraft velocity ...";
- Lance et al., 2004 where "a detailed numerical cloud parcel model [... is used to] determine a most probable size distribution and updraft velocity for polluted and clean conditions of cloud formation";
- Chuang 2006 - a study on "Sensitivity of cloud condensation nuclei activation processes to kinetic parameters" that also uses an adiabatic parcel model;
- Ditas et al. 2012 where "sensitivity of the supersaturation on observed vertical wind velocity fluctuations is investigated with the help of a detailed cloud microphysical model";
- Partridge et al. 2012 with discussion on local vs. global sensitivity analyses and the applicability of inverse modelling approach to droplet formation sensitivity studies.

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.*

**3. Paper composition**

There are numerous flaws in paper composition, for instance:

- the overlap with the Hammer et al. 2014 paper published earlier this year in ACP is excessive:
  - most of section 2.1.1 "Measurement set-up" is composed of material from section 2.2 "Instrumentation" therein,

    *We rephrased the overlapping material accordingly.*

- 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)".*

- there are cases where the section contents clearly do not match the section titles:
  - first (and only) two paragraphs of section "2. Methods" are only related to "2.1 Observational data",

    *We added the first two paragraphs in "2. Methods" to "2.1 Observational data".*
  - some of the methodology of model initialisation is presented in the last two paragraphs of "2.1.1 Measurement setup",

    *We moved the last two paragrahps of "2.1.1 Measurement setup" to a 2.2.1. and rephrased it accordingly.*
  - section"2.2.1 Box model description (ZOMM)" contains a paragraph on filtering the observational data for entertainment;

    *We moved the paragraph "Cloud periods that exhibited evidence of substantial entrainment or mixing were not included in the analysis. Such clouds were detected by analysing the activated fraction of the aerosol particles as a function of aerosol size. Periods where the largest size bins were not at least 90% activated were excluded. This is the same procedure to that used by Hammer et al. (2014)." to a new Section "Defined cloud periods" after 2.1.1.*
- there are repetitions in the text, e.g.:
  - definition of $SS_{peak}$ is given thrice:

    1: *The highest supersaturation that a particle experiences for a sufficiently long time to grow to a stable cloud droplet is defined as the effective peak supersaturation*

    2.3.1: *The effective peak supersaturation ($SS_{peak}$) is the highest saturation encountered within an air parcel, which leads to activation of aerosol*

    2.3.1: *the $SS_{peak}$ is defined as the highest supersaturation that a particle experiences for a sufficiently long time to grow to a stable cloud droplet*

    *We removed the subsequent definitions after Section 1 and rephrased the sentences accordingly.*
  - the very same sentence "*Previous studies have found that a high $SS_{peak}$ can be caused by ...*" is used to begin subsections 2.3.3 and 3.2;

    *We rephrased the sentence beginning the subsection 3.2 as follow: "According to previous studies, low $SS_{peak}$ can be caused by small updraft velocity or a large number of potential CCN. Conversely, a high $SS_{peak}$ can be caused by a high updraft velocity or a low number of potential CCN (see Sect. 2.3.3).".*
  - almost the same wording is used in section 3.3.2 and in the conclusions: "*combinations of amplitudes and frequencies ... small-scale fluctuations in the vicinity of the JFJ*"

    *We rephrased the wording in the conclusions as follow: "Furthermore, small-scale fluctuations in the vicinity of the Jungfraujoch were simulated based on several sinus functions with combinations of amplitudes and frequencies.".*
- some statements seem incoherent (i.e. need rephrasing):
  - **abstract**: "*It was found that the updraft velocity, defining the cooling rate of an air parcel, is the parameter with the largest influence on $SS_{peak}$*"

    *We rephrased it: "The updraft velocity, which defines the cooling rate of an air parcel, was found to have the greatest influence on $SS_{peak}$."*
  - **conclusions**: "*On average small-scale variations are raising the $SS_{peak}$ values to a larger extent than the other investigated parameters in this study*"

    *Here the connection to the next sentence was missing. Therefore we rephrased it as: "On average small-scale variations of temperature raise the $SS_{peak}$ values to a larger extent than the other investigated parameters in this study:"*
  - "*effect of $SS_{peak}$ on updraft velocity*" & "*influence that the vertical wind potentially has on the $SS_{peak}$*"

    *We replaced "potentially" with "most likely".*
- there are numerous vague/ambiguous/unclear statements:
  - "... turbulence applied to a small linear cooling rate" (what does it mean to apply turbulence?)

    *we replaced turbulence with "small-scale temperature fluctuations"*
  - "... (median dry activation of CLACE2011)"

    *There was a word missing, we added: "median dry activation diameter of CLACE2011"*

- "... corresponding dew point temperature of the LWC" (suggests that LWC has a temperature)
  *We rephrased it as follow: "Assuming all the water is in vapour phase, the dew point temperature, was calculated via the ideal gas law and the Clausius-Clapeyron equation".*
- "... temperature and the corresponding pressure trajectory" (what is a pressure trajectory?)
  *We replaced all wordings "pressure trajectory" to "pressure along the air parcel trajectory".*
- "It is not feasible to measure the updraft velocity at the point of aerosol activation." (sometimes it is! please add "at JFJ")
  *We added "at the JFJ".*
- "To investigate the importance of the fluctuations to the decrease of temperature ..." (only decrease?)
  *It was meant to the mean decrease of temperature. However, we rephrased it to: "To investigate the influence of the small-scale fluctuations of $SS_{peak}$ on the temperature ($T_{turb}$) and pressure along the air parcel trajectory from the initialization point …".*

**Further comments and questions**

- In Hammer et al. 2014, the model-derived SSpeak is defined as simply "highest SS reached along the trajectory". Here, it is defined using the 2 µm diameter threshold. I guess that the change was needed due to employment of the fast-varying input data resulting in supersaturation fluctuations - this should be explicitly mentioned and discussed.

*We added to Sect. 2.3.1.:" It is important to note that in Hammer et al. (2014) the definition of the $SS_{peak}^{mod}$ simply was the "highest SS reached along the trajectory". The new definition described above is needed for investigating the small-scale fluctuation described in Sect. 3.3. The comparison $SS_{peak}^{mod}$ obtained by the two definitions respectively was within 10%."*

- If I understand correctly, the model is stopped at different heights above cloud base (but always at the altitude of JFJ). Thus, the time the droplets are given to grow differs from simulation to simulation. Yet, the abovementioned $SS_{peak}$ definition features a threshold on final droplet size (?) Isn't it incompatible?

*This seems to be a misunderstanding. It is true that the model is always stopped at the altitude of the JFJ which is always at different altitudes above the cloud base. However, the threshold is not defined on final droplet size but on the threshold of the droplet size at the point droplets grow fast (i.e. at point of activation). We therefore added: "The threshold is, therefore, defined on final droplet size but on the threshold of the droplet size at the point droplets grow fast (i.e. at point of activation).".*

- What does the model calculate between the starting point at RH=90% and the point of RH=99% at which the equilibrium assumption is lifted?

*Between RH=90% and 99%, the model calculates the water content of the aerosols, assuming that they are in equilibrium with the gas phase. It does this using the k-Koehler equation, as described in the text immediately above equation 2. We have slightly modified the text to be clearer that we mean that the water content is calculated when we say the growth is calculated.*

- Even though very small-scale fluctuations of air thermodynamic properties are considered, all droplets in the model are exposed to the same conditions. Worth mentioning/discussing.

*We added in Sect. 3.3.1: "It was assumed that each particle experienced the same real-time fluctuations."*

- The Hammer et al. 2014 paper features an error estimate of the measurement-derived $SS_{peak}$ of > ±30%. Why not mention it in sections 3.2 and 3.3 when discussing sensitivity of model-predicted $SS_{peak}$ values.

*We agree that it is important to note the error estimate of measurement-derived $SS_{peak}$ and therefore added in Sect. 2.3.1: "A relative uncertainty of about ±30% was estimated for $SS_{peak}$"*

- Why not give the reader a hint on the uncertainty of the model predictions with respect to such parameters as the timestep, bin layout, bin number and the debated values of constants (e.g. mass accommodation coefficient).
  *A description of the limitations on the model time step has been added, as described above. The following text has been added to the model description section: "The aerosol size observed in a single SMPS measurement has an uncertainty of 10% (Wiedensohler et al., 2012); however the input distributions used in the basic model simulations consist of median size distributions taken over the CLACE 2011 campaign. The bin resolution used in the model is the same as that measured by the SMPS. Any uncertainties in the model calculation resulting from the resolution of the bin sizes or the aerosol size distribution would be much smaller than the differences in simulated peak supersaturation caused by varying the number and size of the aerosols, as is done in Fig 6.*

- In the paper, the parcel model is fed with a prescribed temperature profile instead of an adiabatic one that results from the simulated droplet growth. In my understanding, the only reasons to do so would be that an actual temperature profile is accurately known or that the intended profile differs significantly from an adiabatic one. Here, the profile was not measured, and instead an approximate one is used. Why?
  *The background trajectory (i.e. without the fluctuations) is essentially an adiabatic trajectory, with the cooling rate corresponding to an average rate which is present when latent heat is released by the droplets. The simulations were done in this way as the model does not contain a calculation of latent heat release from growing droplets.*

- As a side note to the above point: in Hammer et al. 2014, the adiabatic lapse rate is assumed to be 0.6 K (100 m)-1, while here the value of 0.65 is used. Why?

  *This was simply an arbitrary difference in the number of significant figures used for the value in the different studies. We agree that using 0.6K here too would have been more consistent, however the difference of 0.05K will not influence the results in a significant way.*

- The model used features ice microphysics. I assume (although it is not said explicitly in the paper) that ice microphysics was turned off for the presented simulations as the whole discussion relates liquid clouds. Yet, the simulation parameters cover negative cloud-base temperatures. Would the model predict ice nucleation if it was turned on in the model?

  *It's correct that the ice microphysics was turned off for the presented simulations. This was because 1) The current ice parameterisation is not tested for warm, mixed phase clouds, and 2) We specifically excluded cases where partial glaciation had occurred from the simulations by only using input data from cloud periods where the activated fraction was greater than 0.9 above the activation threshold diameter. We now state explicitly in the paper that we did not include ice formation in the simulations.*

- In section 2, the two prevailing wind directions are mentioned, while it seems that beginning from section 2.1.2 only the NW advection is considered - why?

  *That's correct. Apparently, we did not explicitly state that. The reason that we only considered clouds from NW wind direction is that there are relatively few measurement points when southeast wind was present, and also because the clouds coming from the northwest are mostly found to be formed locally by rapid updraughts, in contrast to the clouds from the south, which are often stratus, which has been advected from further away. This makes the clouds from the northwest more suitable for our study. We added: "In here, only clouds reaching the JFJ with NW wind directions are considered. Relatively few measurement points when when SE wind was present, and also because the clouds coming from the NW are mostly found to be formed locally by rapid updraughts, in contrast to the clouds from the south, which are often stratus, which has been advected from further away. This makes the clouds from the NW more suitable for our study."*

- Figure 3 (Now Fig. 4) includes cloud-period labelling which is not used elsewhere in the paper.

*That's true; however, we thought that this is better than showing a broken time series. We hope that this is acceptable. We have now added text to the caption of Fig 4 to state that the lines and the labels serve to identify different cloud periods between which there are gaps of non-cloudy time.*