

Comments to manuscript acp-2014-665:

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

by E. Hammer et al.

### **Paper contents and a general impression**

The refereed paper discusses a series of simulations performed using a parcel model with a moving-sectional representation of water vapour condensation on a population of aerosol particles. The modelled phenomenon is the formation (activation) of cloud droplets. The model input parameters are derived from measurements made at the Jungfraujoch high-altitude research station and include: aerosol size distribution, aerosol hygroscopicity, air thermodynamic properties and air ascent rate. From the measurements, a set of few hundreds of input parameters are constructed with which the simulations are run. Results of the simulations are used to study sensitivity of the model-predicted supersaturation to the input parameters.

First (sec. 3.1), an inverse-modelling methodology is used to find the input ascent rate which matches best the total number of cloud droplets inferred from the measurements.

Second (sec. 3.2), the sensitivity of the model to the input parameters is studied by running all the simulations with a selected parameter altered. The varied parameters are the updraught velocity (4 values tested), the total number of aerosol (2 values tested), the mode size (2 values tested) and the hygroscopicity parameter (2 values tested).

Third (sec. 3.3), the model sensitivity to small-scale variations of the ascent rate is studied using both measured and synthetic fluctuations.

The paper is an extension of the “Modelling the effective peak supersaturation and potential turbulence effects” section presented in the Hammer et al. paper published in ACP earlier this year.

While the topic addressed is of particular interest to the community, and the presented results do have a high potential, my general impression is that the paper calls for a major revision. In the following sections, I detail the main reasons that shaped my opinion.

### **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<sup>1</sup>” and the data “should be held in persistent public repositories<sup>2</sup>”

The paper does not fulfil 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*”

<sup>1</sup>[http://www.atmospheric-chemistry-and-physics.net/submission/obligations\\_for\\_authors.html](http://www.atmospheric-chemistry-and-physics.net/submission/obligations_for_authors.html)

<sup>2</sup>[http://www.atmospheric-chemistry-and-physics.net/general\\_information/data\\_policy.html](http://www.atmospheric-chemistry-and-physics.net/general_information/data_policy.html)

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.

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?);
- the form of drop growth equation used (are the latent heat effects accounted for here? how the molecular/continuum regime transition is accounted for?).

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.

## 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.

### 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,
  - 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,
- 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”,
  - some of the methodology of model initialisation is presented in the last two paragraphs of “2.1.1 Measurement setup”,
  - section “2.2.1 Box model description (ZOMM)” contains a paragraph on filtering the observational data for entertainment;
- 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*
  - 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;
  - 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*”
- 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  $SS_{peak}$* ”
  - **conclusions:** “*On average small-scale variations are raising 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  $SS_{peak}$* ”
- there are numerous vague/ambiguous/unclear statements:
  - “*... turbulence applied to a small linear cooling rate*” (what does it mean to apply turbulence?)
  - “*... (median dry activation of CLACE2011)*”
  - “*... corresponding dew point temperature of the LWC*” (suggests that LWC has a temperature)
  - “*... temperature and the corresponding pressure trajectory*” (what is a pressure trajectory?)
  - “*It is not feasible to measure the updraft velocity at the point of aerosol activation.*” (sometimes it is! please add “at JFJ”)
  - “*To investigate the importance of the fluctuations to the decrease of temperature ...*” (only decrease?)

### Further comments and questions

- In Hammer et al. 2014, the model-derived  $SS_{\text{peak}}$  is defined as simply “highest SS reached along the trajectory”. Here, it is defined using the  $2 \mu\text{m}$  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.
- 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 above-mentioned  $SS_{\text{peak}}$  definition features a threshold on final droplet size (?) Isn't it incompatible?
- What does the model calculate between the starting point at  $\text{RH}=90\%$  and the point of  $\text{RH}=99\%$  at which the equilibrium assumption is lifted?
- 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.
- The Hammer et al. 2014 paper features an error estimate of the measurement-derived  $SS^{\text{peak}}$  of  $> \pm 30\%$ . Why not mention it in sections 3.2 and 3.3 when discussing sensitivity of model-predicted  $SS_{\text{peak}}$  values.
- 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).
- 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?
- As a side note to the above point: in Hammer et al. 2014, the adiabatic lapse rate is assumed to be  $0.6 \text{ K } (100 \text{ m})^{-1}$ , while here the value of 0.65 is used. Why?
- 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?
- 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?
- Figure 3 includes cloud-period labelling which is not used elsewhere in the paper.

Hope that helps!