Review of

**Towards a more reliable forecast of ice supersaturation: Concept of a one-moment ice cloud scheme that avoids saturation adjustment**

by Sperber & Gierens

**Summary and general comment:**
In this study a new concept of handling ice supersaturation in a one-moment scheme is proposed. The model is based on a stochastic approach, starting with a stochastic box model which is then transferred to a grid box of a hypothetical coarse resolution model. The authors explain the theoretic concept, derive the relevant equations for the representation of ice supersaturation in a grid box and run some simulations for checking the agreement between the different models. Overall, there is a potential for representing ice supersaturation in a better way as compared to the instantaneous relaxation of in cloud water vapor to saturation.

The model is based on a very innovative and interesting idea, which leads to a really non-standard approach for representing ice supersaturation in simple one moment schemes. The study is conceptually new and thus is very well suited for ACP.

However, I had a hard time to understand the derivation of the equations and the details of the model, since they are often non-standard and are quite compactly represented. Thus, this very interesting paper could be improved in terms of the representation. In addition, for some scenarios the model has some issues to represent the physical behavior, which could be clarified. Therefore, I would recommend (major) revisions of the manuscript, before the study can be accepted for publication. In the following I will explain my concerns in details.

**Major issues**

1. Representation of the nucleation event:
   In the stochastic model, nucleation takes place instantaneously if the nucleation threshold is reached. There are some issues with this approach. First, the nucleation threshold as derived by Kärcher & Lohmann (2002) is probably not the right quantity for initiating the nucleation, since it will often not be reached (see Spichtinger et al., 2023), especially not during very low vertical velocities/cooling rates, as used for the simulations. Generally, ice nucleation is already triggered if the saturation ratio is close enough to the threshold, see discussion in Baumgartner & Spichtinger (2019). Using the high threshold by Kärcher & Lohmann (2002) might introduce an (incorrect) time shift. Second, the nucleation event itself has a duration, i.e. one has to wait until the nucleation event is completed and the full number of ice crystals (as e.g. diagnosed by the Kärcher & Lohmann scheme) is produced. This time is not included in the model, and might also lead to a (correct) time shift in the cloud evolution; to be precise, the small cap (or parabola) around the maximum in $s$ is missing (see, e.g., figure 1 in Spichtinger & Krämer, 2013). Although it is probably not possible to include the duration of the nucleation event into the model, this should be commented in the text.

2. Notation and derivation of some key equations:
   The key quantities as supersaturation $s$ and others should be introduced more carefully, since different communities use rather saturation ratio than supersaturation. Actually, there is no definition of $s$ in the manuscript.

   I failed to reproduce equation (9) from equations (3) and (8). It would be good if the derivation would be a bit more elaborated; maybe some details can be provided in an appendix. In this context, it would be good to have a formula for the key value $\alpha$ instead of referencing the quite confusing papers by Khvorostyanov & Sassen (1998a,b). In the end of the text there is a kind of explanation, but it would be good to have such a formula at the very beginning, when the key equation (3) is stated. Finally, it would be good to explain carefully, that the equilibrium supersaturation is depending on the cooling rate and can be positive and negative (saturation ratio above and below 1, see figures 3 and following). In the plots, it is also a bit confusing that RHi is used (but as saturation ratio), although in the text and the derivation the supersaturation is used.

Finally, it would enhance the readability if the notation $\exp(x)$ would be used instead of $e^x$. Especially, for equations (23), (35) etc. this would help a lot.

3. Use of a constant $\alpha$:
For slow updraft/cooling regimes the model seems to work quite well, but the authors report that there are issues for higher cooling rates. Especially, the equilibrium supersaturation becomes very high and a relaxation is difficult. To my opinion this stems (at least partially) from the fact that the value $\alpha$ is held constant, although it is composed by number concentration $n$ (which is constant after nucleation) and mean radius $\bar{r}$, which is NOT constant. Thus, the relaxation should be different from exponential (faster or slower) due to the additional change in $\alpha$. Maybe this change in $\alpha$ can be introduced into the model by an additional integration of the mass growth equation for a single crystal, because a monodisperse distribution is used anyway. However, the authors should check, if a change in the mean radius might improve their model results. This should be included into the discussion.

**Minor issues:**

1. Thermodynamic states:
In the abstract, ice clouds in supersaturated air are mentioned, but they can also survive in (moderately) subsaturated air. I would rather use "to be out of equilibrium" instead of "in an ice supersaturated state". Later in the text, ice supersaturation is termed to be an extreme case; maybe again the notation of "far away from equilibrium" might be more appropriate.

2. Contrail radiative effects:
I missed the reference Stuber et al. (2006), which clearly indicated the net warming of contrails due to the infrared component of the radiation.

3. Measurements:
In the introduction, the lack of measurements is strongly emphasized; however, in the discussion, the long term programs MOZAIC and IAGOS are mentioned. Actually, I missed these references in the introduction and would recommend to indicate that there are some (only sparse) measurements, which are indeed helpful.

4. Wording:
To my opinion, the term "saturation adjustment" is already taken; it is dedicated to a certain technique for modeling liquid clouds, although this procedure is closely related to the approach of fast relaxation of supersaturation to equilibrium. For warm clouds, at values of the saturation ratio above 1 (i.e. at very small supersaturation of order of few percents or even less), cloud droplets are formed instantaneously, the excess water vapor is transferred to cloud water, and the partial water vapor is set to saturation values. The principle for ice clouds allowing high supersaturations (of order of few 10 percent) before nucleation and then using a fast relaxation to equilibrium is similar, however a bit different to the original term (also without the numerical issues of latent heat release). Generally, I would like to avoid the term "saturation adjustment" in this context and rather use something like "fast relaxation", but this is a subjective viewpoint. However, I would ask the authors to clarify the term in context of the different thermodynamic phases (liquid clouds and ice clouds), since readers with a different background might be confused. Actually, there are also attempts to avoid saturation adjustment in liquid clouds allowing (moderate) supersaturations, see, e.g., Porz et al. (2018).

5. Figures and figure captions:
For figures 3 and 5 (and others of the constant updraft scenario) I would like to see the evolution of the cloud fraction; this would help to interpret the change in RHi as indicated in the caption. For figures 4 and 6 (and others of a variable updraft scenario) an additional representation of the cooling rate (or updraft) would be helpful. More complete figure captions (for all figures) would also be helpful, e.g. just a remark "scanerio 2 but with a different whatever" does not really help to understand the figure. It would be good if the figures together with the caption could be understood without reading the text carefiully.

6. Diagnostic relation for ice crystal number concentrations:
   It is claimed that the relation $n \sim w^{\frac{3}{2}}$ by Kärcher & Lohmann (2002) should be used for deriving the ice crystal number concentration. However, one should mention here that the relation only works for clear air; for pre-existing ice, a reduction of the number concentration must be taken into account. This should be mentioned in the text, maybe also in the context of competing nucleation pathways, e.g. homogeneous and heterogeneous nucleation.

## References

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Spichtinger, P. and M. Krämer, 2013: Tropical tropopause ice clouds: a dynamical approach to the mystery of low crystal numbers. Atmos. Chem. Phys., 13, 9801-9818, doi:10.5194/acp-13-9801-2013

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