Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1182-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling mixed-phase clouds with large-eddy model UCLALES-SALSA" *by* Jaakko Ahola et al.

Anonymous Referee #2

Received and published: 7 March 2020

This study adds a heterogenous ice nucleation parameterization to the UCLALES-SALSA model. The model is tested with fixed ice crystal number concentration by using a case from the ISDAC campaign that was the focus of an intercomparison study. This paper is well written, and the figures clearly illustrate the main points. As to the results of the study, allowing prognostic INP will reduce the number of ice crystals because of precipitation, causing there to be more sustained cloud liquid, but how is this a new result? Many studies have already shown this (Fridlind et al. 2012; Solomon et al. 2015; Solomon et al. 2018). Also, the variability in the control studies differ significantly from the ISDAC intercomparison, which needs to be explained. Also, it needs to be explained how aerosol concentration above cloud top were chosen and what role the prognostic CCN is playing in the simulations. This model will be a very

Printer-friendly version

Discussion paper

useful tool for studying mixed-phase cloud processes, but I think this study is better suited for a technical report than a scientific publication.

Major comments:

1) Need to include basic detailed about the model in Section 2 even though they may be available in other papers. All details needed to understand the simulations need to be included in this section (CCN activation, etc).

2) This model is clearly more sensitive to ice formation than all the models included in the ISDAC intercomparison. It is important to understand why to understand the sensitivity studies with the new ice nucleation parameterization.

3) How is droplet number concentration specified in the ISDAC ICE4 simulation? Is this prognostic? If so, it would be insightful to see the droplet number concentration in Figure 4. Is this why the results are so different than the intercomparison?

4) It is not clear how the artificial movement of aerosols between bins for numerical stability is affecting the results (lines 240-243).

5) Please explain why droplet freezing occurs throughout the cloud while for the same case Savre and Ekman (2015) found droplet freezing at cloud top. A more detailed discussion of how aerosols and droplet and ice crystal activation are represented in the two models is needed to understand why simulations in the two studies differ.

6) Lines 275-277: More details of the simulations are needed to understand whether this is a correct statement.

Minor comments:

1) Line 198: ". . ..concentration is was. . .". Please reword.

2) Line 202: "was adjusted"

3) Line 203: "represents"

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1182, 2020.

Printer-friendly version

Discussion paper