Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-137-RC2, 2019

Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "The sub-adiabatic model as a concept for evaluating the representation and radiative effects of low-level clouds in a high-resolution atmospheric model" *by* Vasileios Barlakas et al.

Anonymous Referee #1

Received and published: 24 July 2019

General comments:

The authors investigated a sensitivity of the vertical distribution of the assumed adiabaticity (i.e. sub-adiabatic versus vertical homogeneous cloud model) to their cloud radiative effect of low-level clouds. The effect of the different adiabatic model applied was examined by using ICON-LEM, and also evaluated differences in the cloud properties by switching from single-moment to double-moment microphysics scheme in their model. They conducted simulations on six case days and found that the sub-adiabatic model resembles better characteristics of liquid clouds rather than the vertically homogeneous assumption.

The authors have revised several minor issues according to the referee comments in access review before ACPD. While the manuscript is scientifically sounds and is publishable with further improvements and clarification, I still feel that the manuscript does not reach the standard of ACP.

My major concern is that the simulations examined in this study are very limited (Page 8 Line 15-18). The authors conducted simulations using six case days, but actually looked at in details only the case of 3 June 2016. How general are they? Doesn't the vertical structure of adiabaticity depend strongly on the cloud regimes and types or their life-stage? In the present form of this paper, objectives are too narrow. The described relationship among cloud micro- and macrophysical properties and radiative effect using high resolution simulation may provide key suggestions on aerosol-cloud interactions, but the findings as they are, are by no means general. With some more simulation cases or a bit more analysis for all the case days in detail, I think this will make a publishable work.

Specific comments:

Section 2.3: Please describe the model resolution, domain size, as well as timestep used in the simulations. The general description of ICON-LEM on page 3 (lines 16-17 and 28-30) is confusing with regards to this.

Equation (7): It is better to add a sentence about the factor 2/3, rather than 5/9, citing relevant papers (e.g., Szczodrak et al., 2001; Wood and Hartmann, 2006; Lebsock and Su, 2014). Equations (14) and (15) as well.

Figure 3 and caption: qL -> QL or CLWP

Figures 6, 7 and 8: The order of subfigures is not consistent with the caption.

Table 6: I found several mismatches between Table 6 and citing main text (e.g., page 18 line 16), which made reviewers very difficult to track...

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Discussion paper

Page 23 Line 11-13: This sentence is too vague. Please raise more specific source of uncertainty, and describe how the scrutinization is required.

Page 25 Line 12-13: This sentence recommends double-moment cloud microphysics, but page 23 line 12 points weakness of the double-moment.

Appendix B: Please change the appendix title. Appendix section is not just a list of supporting materials. The current version does not have any explanation about the figures in the appendix (Appendix C as well).

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