Review of “The impact of mesh size and microphysics scheme on the representation of mid-level clouds in the ICON model in hilly and complex terrain” by Omanovic et al. (egusphere-2024-1989)

This manuscript addresses how the ICON model can represent clouds at different horizontal resolutions (1000 and 65 m) and with different cloud microphysical schemes (one- and two-moment microphysics), re-simulating in total for cases originating from observational campaigns in Switzerland and Austria. Considering the increasing use of high-resolution models in weather and climate prediction, this study touches upon an important subject, but does not substantiates its claims. To strengthen the manuscript, I believe that a much deeper analysis of the effects of resolution and cloud microphysical schemes are necessary. Thus, I cannot recommend the publication in the current stage.

Major Comments

Constraining the influence of the large-scale model. I understand that all simulations are driven by COSMO-1 analysis (ll. 114 – 116). How much does this large-scale forcing affect the simulations, and especially the development of clouds? In other words, how much alike are thermodynamic properties in the COSMO-1 driver model and the corresponding ICON runs? Besides the analyzed impact of vertical velocity variances, the ability of the nested models to change the underlying thermodynamics is probably key to understand how resolution and cloud microphysics can affect the simulated scenarios.

Extend the analysis of cloud microphysical properties. The authors restrict their analysis of cloud microphysical properties to liquid and ice masses. While I understand that liquid and ice masses are predicted by both microphysical schemes, addressing how well droplet and ice concentrations are predicted in the two-moment and prescribed in the first-moment scheme is crucial to evaluate how well cloud microphysics are captured. Moreover, the direct comparison of process rates (e.g., condensation/evaporation, accretion) could enable more detailed insights on how the cloud microphysical schemes behave.

Numerical effects. The authors did not address the effects of numerics. Increasing the resolution from 1000 to 65 m should go along with a substantial decrease in numerical diffusion. Simultaneously, approximately doubling the number of predicted moments in the cloud microphysics scheme will do the opposite, i.e., potentially increasing the numerical diffusion of cloud properties.

Minor Comments

L. 102: What is meant by “peak-to-peak-distance”? The width of the valley?

Ll. 111 – 112: Both ICON resolutions use the same 80 vertical levels. How much are these levels apart? Why did the authors not address the impact of vertical resolution, which probably has a strong impact on vertical velocities and hence cloud microphysics.

Ll. 126 – 163: The description of cloud microphysics combines the underlying physics and the applied models in a slightly unfortunate way, making it hard for the reader to disentangle what processes are actually represented in the cloud microphysical model. For instance, most activation parameterizations do not depend on a “critical size” (l. 134), but only the critical supersaturation. Moreover, most models do not represent hygroscopic growth of aerosols (l. 132). Is it resolved in the applied models? Lastly, do the models use saturation adjustments for their treatment of condensation? If yes, this needs to be mentioned, and potential impacts on the simulated cloud microphysics should be discussed.

Ll. 174 – 178: Please elaborate on the categorization of altocumulus, stratocumulus, and cumulus. Why are these clouds considered “mid-level” clouds, although cumulus and stratocumulus are typically considered low-level boundary-layer clouds?

Ll. 187 ff.: How is “cloud cover defined”? Figure 2 states that the cloud cover is derived from a certain height level. Many authors use an integrated quantity (e.g., cloud optical thickness) to define cloud cover. What are the benefits in defining the cloud cover in the applied way?

Appendices: There are many nice figures in the appendices. What about integrating them as sub-panels into the main text?

Tabs. 3 ff.: Why do the authors use LWC and IWC and not integrated quantities such as LWP and IWP? The former might be more affected by subtle differences in cloud depth.

Ll. 192 – 194: Largest differences at the cloud edge might hint toward differences in turbulent (or numerical) mixing/diffusion. This should be addressed more thoroughly.

L. 196: What is “simulation 3”?

Ll. 214 – 216: A stratocumulus of 2000 m depth is rather unusual.

Figs. 7 and 9: What do the dashed lines in panels b to f indicate?

Figs. A2, A5, B1: These plots would benefit from a clear indication of the location of the cloud.

Technical Comments

Ll. 80 – 82: The campaigns are probably not “completely independent of each other”. I suggest rewording.

L. 84: Change “case studies” to “cases simulated here”.

L. 269: “LWP” has already been introduced as an abbreviation for “liquid water path”. Use it.

Ll. 273 ff.: Semicola are overused in the text.

L. 286: “Ac” is not defined.

L. 314: To what does the “at” point to?