Review of manuscript no. acp-2021-481: **"Importance of aerosols and shape of the cloud droplet size distribution for convective clouds and precipitation"** by Christian Barthlott, Amirmahdi Zarboo, Takumi Matsunobu, and Christian Keil.

General

This paper studies the response of convective cloud systems and their precipitation production to changes in CCN and CDSD size distributions (concentration and shape parameter). Using the ICON model, numerical simulations of different synoptic systems (6 real cases) are conducted for the area covering central Europe. Comparison of the clouds and precipitation properties under different CCN size distributions is done for concluding about aerosol effects on deep convective systems. The simulated results, which are classified to weak and strong synoptic forcing, point on increased total cloud water and decreased total surface rain, with increased CCN concentration and narrower size distribution. The precipitation response is stronger for weakly forced cases. Explanations for these results are suggested by analysis of different hydrometeors types in the simulations and the related formation mechanisms. Less efficient collision-coalescence is demonstrated in more polluted cases (suppressed warm rain formation) and stronger rain evaporation at low levels. The simulated results also show a negative effect of aerosol on the convective intensity meaning there is no convective invigoration. This work examines the interaction of the clouds with their thermodynamic environment as well showing the impact of precipitation on the environmental instability.

To summarize, it is a very interesting and valuable work dealing with an important subject which is still not fully understood.

I have a few comments that should be addressed before publication:

1) This study used a bulk microphysical scheme with a saturation adjustment assumption. This method is limited in its ability to simulate rightly the aerosol effect on warm cloud processes. First, the condensation efficiency cannot be accurately described by a saturation adjustment scheme. It was shown that the supersaturation values in clouds depend heavily on the aerosol loading (Pinsky et al., 2013, Seiki and Nakajima, 2014, Dagan et al., 2015). This major effect is neglected in this work. Another major effect is the aerosol impact on the drops' effective terminal velocity (Koren et al., 2015). A bulk scheme is limited in its ability to describe the full

range of terminal velocities and so it neglects this major effect too. The authors should regard this major issue and the limitations of the method used here for this type of study should be discussed in more details.

2) This study examines the effects of changes in CCN and CDSD size distributions on deep convective systems. It examines both warm and cold processes. Nevertheless, there is no description or treatment of changes in IN size distribution. If there are changes in aerosol properties it will affect the IN properties and hence the mixed and cold processes. This issue should be explained in the manuscript regarding the treatment of IN in the model and its consequences on the results.
3) The terminology used in the paper for describing the aerosol loading is very confusing (maritime, continental, polluted). The use of maritime and continental can regard the thermodynamic conditions as well and that why it is confusing. I suggest to change it to clean, intermediate, polluted and highly polluted and to use it in a consistent way throughout the paper.
4) In order to validate the model simulations there is a need to compare it to measurements. I suggest to add a figure which is similar to fig. 4 that will present observed accumulated rain or some other cloud properties for the 6 cases. This will enable estimation of the validity of the simulated results.
5) Fig. 6: The meaning of the shading is not explained in the figure caption so the figure is unclear. It should be added.
6) The idea of considering the interaction of the convective clouds and the available instability as a type of "lifetime effect" is problematic as it treats a whole cloud system and not a single cloud (as the lifetime effect). This idea should be examined again and any way it should be explained better.
7) Fig. 8: I suggest to present mean vertical profiles of the different types of hydrometeors and cloud processes instead of the way it is presented now in figure 8 (similarly to fig. 7). The suggested way of presentation will connect better to fig. 7 and will help to present a full picture of the explanations.

The relevant papers:

1. Pinsky, M., Mazin, I., Korolev, A., and Khain, A. (2013): Supersaturation and diffusional droplet growth in liquid clouds, J. Atmos. Sci., 70, 2778–2793.
2. Seiki, T. and Nakajima, T. (2014): Aerosol effects of the condensation process on a convective cloud simulation, J. Atmos. Sci., 71, 833– 853.
3. Koren I., Altaratz O. and Dagan G. (2015): Aerosol effect on the mobility of cloud droplets. Environmental Research Letters. 10, 10, 104011.
4. Dagan G., Koren I. & Altaratz O. (2015): Competition between core and periphery-based processes in warm convective clouds - from invigoration to suppression. Atmospheric Chemistry and Physics. 15, 5, p. 2749-2760