

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-481-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on acp-2021-481

Anonymous Referee #1

Referee comment on "Importance of aerosols and shape of the cloud droplet size distribution for convective clouds and precipitation" by Christian Barthlott et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-481-RC1>, 2021

This study attempts to shed more light onto the effects of CCN and shape parameter assumptions on cloud and precipitation properties in the context of NWP, and how the effects differ between differently forced cases. It provides interesting and valuable findings to the existing knowledge and I would like to see it published eventually, since these two microphysical properties are often not very well constrained by observations as pointed out by the authors. However, some analysis and general scientific communication need some major revision to address the concerns.

Major Comments

- Definition of shape parameter and gamma distribution: When talking about the shape parameter in the introduction, the authors implicitly switched between shape parameter for the size distribution and that for the mass distribution (given by Eq. 1), which has a difference of 2. Normally, the size distribution collapses into an exponential distribution for $v=0$, but it is not the case for Fig. 1, which can cause some confusion. Note that nothing is explicitly incorrect, just confusing. This difference deserves to be explained in greater detail in the introduction. Also, when the authors cite that typical values of the shape parameter are 0-14, they are citing studies which calculated the shape parameter for a size distribution. To apply these values to a mass distribution instead, 2 should be subtracted. In other words, the authors cite that a typical range is 0-14 and then test a range of 2-10 (in terms of the size distribution, it's 0-8 in terms of the mass distribution). This may be important because previous studies (e.g. Igel and van den Heever) have found the greatest sensitivity at low values of the shape parameter. The authors may want to test a mass distribution shape parameter of -2 corresponding to a size distribution shape parameter of 0.
- In order to help readers understand how the shape parameter influences the simulations, could the authors please briefly discuss which processes in the model make use of this parameter? Autoconversion, accretion, riming, evaporation, radiation, etc?
- The authors devote nearly a page to CAPE and thermodynamics and include summaries

of the discussion in the abstract and conclusions. Since this is such an important part, please show some figures to support your conclusions.

- Can the authors discuss why there appears to be virtually no sensitivity to either CCN or the shape parameter in the first 10-12 hours of any simulation, even if precipitation rate is high (e.g. Fig. 6, case 20160602)? Perhaps this is just an artefact of averaging over the entire domain? If averages are taken only where there is precipitation, does this lack of sensitivity disappear?

Minor Comments

- Line 21-23, "polluted conditions does not lead to an invigoration" is not equivalent to a negative aerosol effect (could also be no effect). I would suggest the authors rephrase this part.
- Line 27, I think the authors meant *lower* grid spacing, or higher grid *resolution*.
- Line 34, did the authors mean "...are *not* accounted for"?
- Line 67, What is μ assumed to be?
- Figure 3 and 4, I appreciate the detailed description of the cases that will be discussed later in the paper.
- Line 129, Does RRTM account for the shape parameter of the cloud droplet size distribution?
- Table 1, Just to be clear, only the shape parameter of cloud droplets is being changed, correct? Not the shape parameter of any other hydrometeor categories?
- Line 176, I think the finding that lower CCN concentration leads to precipitation enhancement is very much expected.
- Line 187-188, the word "contrast" is potentially misleading as such contrast only exists due to the arbitrary choice of the "reference run" having continental CCN concentration and a shape parameter of 0. Both the effect from CCN concentration and shape parameter on precipitation seems to be monotonic, hence no real "contrast". Similar languages can also be found in many other places in the manuscript, which need to be revised to avoid being misleading.
- Line 211, I'm confused by the word choice "only" as if larger precipitation rate is expected despite the narrower distribution. If the authors chose the word "only" because they are comparing it against the effect from the maritime (or intermediate) CCN case, I should note that these two cases are not comparable as both CCN and shape parameter are different. I do think it is an interesting observation because I would not expect any increase in precipitation rate due to larger shape parameter, but the authors seem to shift the focus from "the effect of CCN and shape parameter on precipitation rates" to a contest of "who is best at increasing the precipitation rate".
- Figure 7, have the authors considered showing the same range for the x-axis so that it's easier to compare between cases? I do not claim it is the right way, but it would be more obvious and convincing that cloud fraction in the weaker forcing cases show a stronger influence from different CCN concentrations.
- Figure 8, I'd argue that it makes more mathematical sense and is visually more intuitive to plot ratios on a log scale rather than percent deviations. -50% and 200% represent a halving and a doubling, respectively, and are therefore arguably relative changes of the same magnitude. But on a linear scale the 200% appears to be the much larger change. The ratios on a log scale would have the same deviation from 1 and appear equivalent.
- Line 265-266, can the authors explain why the sensitivity of QC is distinct? It doesn't seem to be outlier compared to other quantities and is very much expected based on the classical theories.

- Line 280, Please insert "for" between "Except" and "the".
- Line 289, "levers" should be "levels"
- Line 297, does the author mean just "slight reduction" instead of "slight reduction and increase"?
- Line 308, Are the effects of initial hydrometeor density and initial size included in the model?
- Figure 10, I would again suggest showing the same range for the x-axis for easier comparison between cases.
- Line 344-345, This discussion of Grant and van den Heever seems a little out of context since that paper tested midlevel dry layers whereas the authors are discussing RH near the surface.
- Line 347-348, "The strongest sensitivity occurs ..." Sensitivity of what to what? Please clarify.
- Line 356, If the authors wish to understand the importance of cold rain vs warm rain, why not instead take the ratio of melting to autoconversion and accretion? Perhaps melting and deposition (presumably just net deposition on ice) plus riming are equivalent. Either way, I would explicitly state that the authors are taking the ratio of rain production via ice to the ratio of rain production via warm phase processes.
- Line 359, I find that people mean many different things by "CDS sensitivity". I think in this case the authors mean shape parameter sensitivity.
- Line 386, can the authors define "dominant value of each CDS"? Do they mean "modal"?
- Eq 3, should be the integral of $LWC/r_{eff} * constants$ because r_{eff} is also a function of z . Or the authors should specify how they found an average value of r_{eff} such that it could be pulled out of the integral.
- Line 422, I think the small sensitivity is because the reference run is already at a relatively high CCN concentration, hence not a lot of room for r_{eff} to decrease or albedo to increase. The effect of shape parameter could be more noticeable if the reference run has a lower CCN concentration. So it's at least necessary to add some qualification to this sentence.
- Figure 13, typo the ticks on the color bar (some 0s might have stuck out of the figure canvas). I would again suggest plotting the ratio in log scale for more intuitive visualization.
- For figures that have the different microphysical assumption on the x-axis (Fig. 5, 8, 12), it is probably a better idea to not connect the dots between p_0 and c_1 since they are from two separate experiment groups and thus not logically related. Doing this will also make the (mostly) monotonic relationship between, for example, precipitation rate and microphysical assumptions more obvious. It may be helpful to repeat the c_0 result at the start of the shape parameter test set.