Responses to the reviewers

Importance of aerosols and shape of the cloud droplet size distribution for convective clouds and precipitation

by C. Barthlott, A. Zarboo, T. Matsunobu, and C. Keil   September 29, 2021

We thank both reviewers for reading the manuscript and providing detailed comments. We have carefully considered all comments and changed the manuscript accordingly. Please find below our responses in blue.

Reviewer 1

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

1. 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 $\nu=0$, 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.

   Our equation (1) is based on the particle mass x, but can easily be transferred to particle sizes using a power law for the diameter-mass relation. We added the necessary calculation steps in the paper:

   "Using a power law for the diameter-mass relation $D(x) = ax^b$ ($a = 0.124$ m kg^{-b}, $b = 1/3$), we can transform Eq. 1 from mass x to particle diameter D:

   $$f(D)\mathrm{d}D = f(x)\mathrm{d}x \tag{2}$$
   $$f(D) = f(x)/(\mathrm{d}D/\mathrm{d}x) = f(x)/bax^{(b-1)} \tag{3}$$

   An equivalent way to convert the size distribution from mass to radius (or diameter) for spherical particles is given in Khain et al. (2015):

   $$f(D) = N_0' D^{\nu'} \exp\left(-\lambda' D^{\mu'}\right) \tag{4}$$

with $N_0' = 3N_0 \left(\frac{\pi}{6}\rho\right)^{\nu+1}$, $\nu' = 3\nu + 2$, $\lambda' = \lambda \left(\frac{\pi}{6}\rho\right)^{\mu}$, and $\mu' = 3\mu$. N_0 is the intercept parameter and ρ the bulk hydrometeor density. Both Eqs. 3 and 4 give the same result for the size distribution as a function of the particle diameter."

We also stated that the literature values are based on particle diameter D and added a further comment here:

Figure 1 presents Gamma size distributions with different shape parameters for a fixed cloud water content (QC) and cloud droplet number concentration (QNC) as a function of particle diameter D (see Eq. 3) using a dispersion parameter $\mu = 0.33$. ~~and shows that~~ It can be seen that increasing the shape parameter narrows the size distribution.

At the end of section 2.1 (Model description and simulation overview), we added the variable ν in the text and Tab. 1 to make clear which parameter we modify. Using notation (4) for the CDSD as a function of D and ν' as shape parameter would mean that our values $\nu = 0, 1, 2, 4, 8$ would become $\nu = 2, 5, 8, 14, 26$ as $\nu' = 3\nu + 2$.

Equation 1 only collapses to the exponential distribution for particle mass x with $\nu=0$ AND $\mu=0$. This is not the case for our Figure 1, where the distribution is displayed as a function of particle diameter D and $\mu = 1/3$. Our description of the size distribution also collapses into an exponential distribution for $\nu=0$ and $\mu=0$ (Seifert and Beheng, 2006a). We added this information in the text:

"This function reduces to the classical Γ-distribution with $\mu = 1$, to the Weibull distribution with $\nu = \mu - 1$, and to the exponential distribution with $\mu = \nu = 0$ as a function of particle mass (Seifert and Beheng (2006a)."

Furthermore, the reference value of $\nu = 0$ shows already a very broad size distribution with a maximum at small diameters. A further reduction to -2 is possible, but the resulting size distribution would be even broader and less realistic. As suggested by the reviewer, we made a model run for 11 June 2019 with a shape parameter of $\nu = -2$ and display the 24-h precipitation amount in Fig. R.1 together with the results of the other runs with varied shape parameters. It can be seen that the shape parameter of -2 leads to a precipitation reduction compared to the reference value of more than -23%, whereas the runs with $\nu = 8$ has a reduction of -8% only. We therefore believe that the values of the shape parameter are well chosen in this study and reflect the observational uncertainties of this parameter reasonably well.

2. 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?

We mentioned some of these processes already in the Introduction and now added more information at the end of the model description:

"The size distribution of the cloud droplets has a substantial impact on the simulation results, as various microphysical processes such as condensation, evaporation, autoconversion, accretion, riming, and sedimentation depend on it. Since the optical properties of clouds are also influenced, the shape of the CDSD is also important for the radiation and the energy balance at the surface."

3. 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.

Already when writing the first version of the manuscript, we thought about including the temporal evolution of CAPE, but decided not to include it due to the already high number of figures. Now, we have included this figure together with the lapse rates as new Fig. 7 to support our
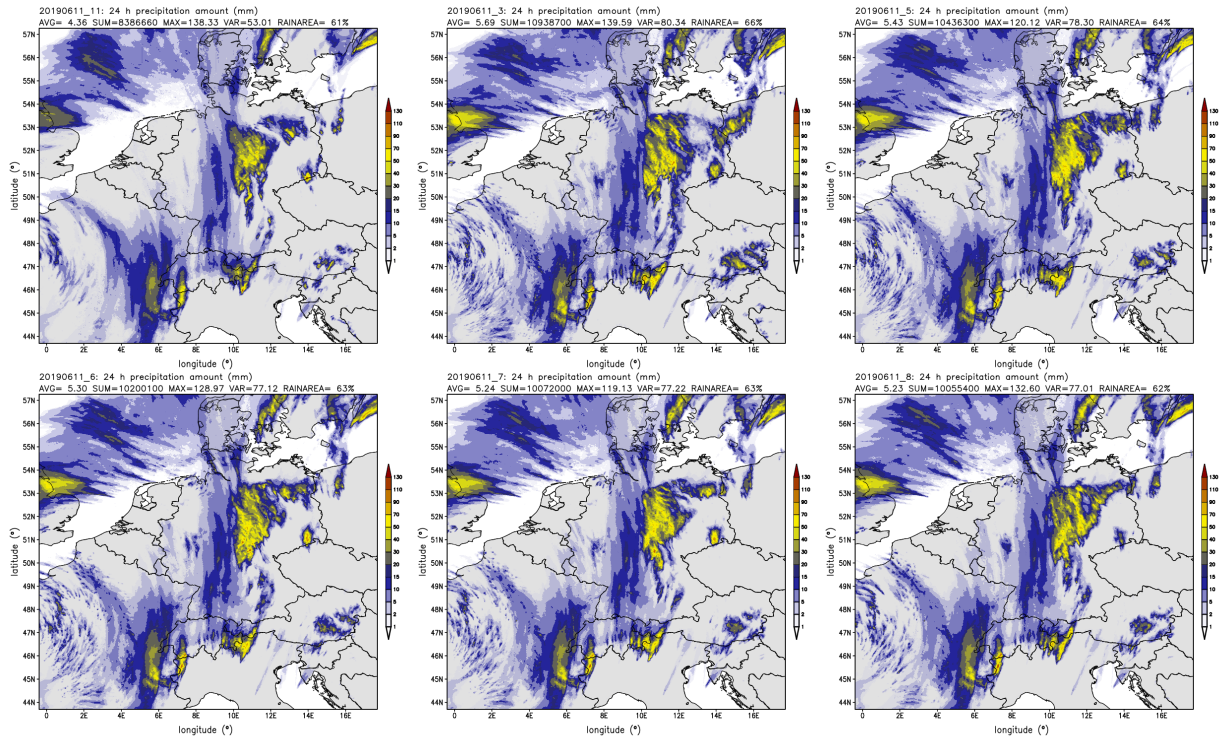
Figure R.1: 24-h precipitation amount on 11 June 2019 for continental CCN concentration and different shape parameters (top left: -2, top center: 0, top right: 1, bottom left: 2, bottom center: 4, bottom right: 8)

description in the text. We think that this supports our conclusions.

4. 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?

The two strong forcing cases of 20130911 and 20160602 show the smallest response in the 24-h amount of the six cases analyzed in this study. The precipitation deviation from the reference run lies between -5% and +4%. The general weaker response of preciptiation to varied CCN concentrations for cases with strong synoptic forcing was also documented by Barthlott and Hoose (2018) or Schneider et al. (2019). As suggested by the reviewer, we calculated the mean precipitation rates for rainy grid points only (Fig. R.2) and find a larger sensitivity for the shape parameter runs and also for the CCN runs in the nighttime maximum for 20160602. We believe that particularly in strong synoptic forcings, clouds behave more like a buffered system and that the sensitivity of surface rainfall to these microphysical uncertainties remains small. We added the following text in the manuscript:

"The comparably small spread in precipitation intensities for both the CCN and shape parameter runs during the nighttime precipitation maximum on 2 June 2016 could be explained by the fact that particularly in cases with strong synoptic forcing, clouds act more like a buffered system and the response of precipitation to these microphysical uncertainties remains small."

Minor Comments

1. 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.

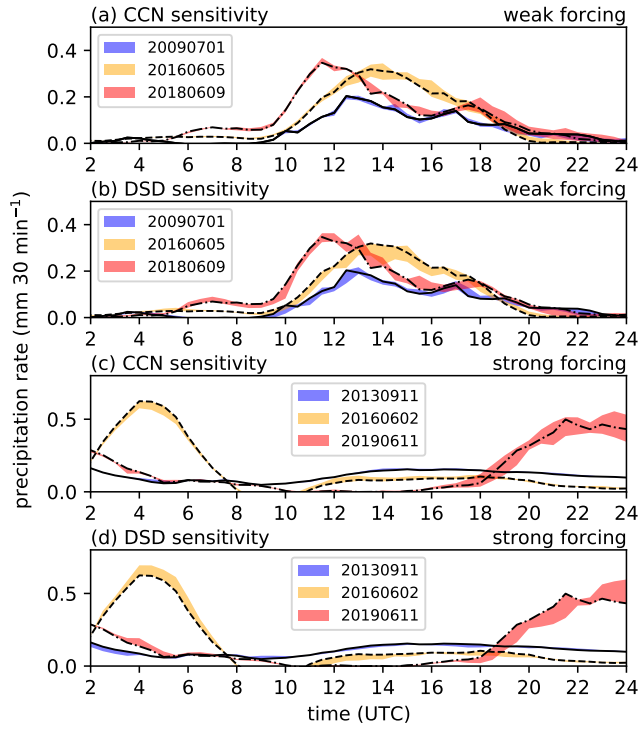We rephrased this part, it now reads:

Figure R.2: Domain-averaged precipitation rate for rainy grid points only.

"By the frequency of updrafts as a function of height, we show a negative aerosol effect on updraft strength, leading to an enervation of deep convection."

2. Line 27, I think the authors meant lower grid spacing, or higher grid resolution.
   Yes, we meant grid resolution and changed the text accordingly.

3. Line 34, did the authors mean "...are *not* accounted for"?
   No, in that sentence we meant "...are accounted for". The following sentence describes uncertainties from microphysical processes.

4. Line 67, What is μ assumed to be?
   μ is the dispersion parameter of the distribution, it is explained in the text. In case of $\mu = 1$ this function reduces to the classical Γ-distribution, with $\nu = \mu - 1$ to the Weibull distribution and with $\mu = 0, \nu = 0$ to the exponential distribution (Seifert and Beheng, 2006a). This additional information has been added to the introduction.

5. Figure 3 and 4, I appreciate the detailed description of the cases that will be discussed later in the paper.
   Thank you.

6. Line 129, Does RRTM account for the shape parameter of the cloud droplet size distribution?
   Yes, in RRTM the effective radii of cloud droplets are based on mass and number concentrations consistent with the microphysics. The effective radii of cloud ice crystals are calculated based on ice mass concentrations only. Only in the latest model release and the new radiation scheme (ecRAD), which has become operational in April this year, there is a module to compute the effective radius of liquid and ice consistent with 2-moment-microphysics to be used by the radiation module.

7. 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?
   Yes, we only change the shape parameter of the cloud droplet size distribution. We added one

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sentence to make that clear:

"Note that we only change the shape parameter of cloud droplets and not that of other hydrometeor categories."

8. Line 176, I think the finding that lower CCN concentration leads to precipitation enhancement is very much expected.

The important finding is the fact that total precipitation systematically decreases for increased CCN concentrations for all cases, independant of the synoptic forcing. The COSMO model with the same microphysics scheme simulated no systematic relationship for weakly forced conditions in earlier research. We have written a separate paragraph on that topic. We also included a citation to a recently published article on invigoration of convective clouds by aerosols by Igel and van den Heever (2021) with this text in the Conclusions:

"Despite many efforts with field experiments and state-of-the-art numerical models, the validity of the invigoration effect is still an open question (Altaratz et al., 2014). In a recent study by Igel and van den Heever (2021), theoretical calculations of a new formulation of the moist adiabatic lapse rate that accounts for freezing, supersaturation, and condensate loading were performed. They find that a CCN-induced increase in storm updraft speed, is theoretically possible, but substantially smaller (and oftentimes even negative), than previous calculations suggested."

Igel, A. L. and van den Heever, S. C.: Invigoration or Enervation of Convective Clouds by Aerosols?, Geophys. Res. Lett., 48, e2021GL093 804, https://doi.org/10.1029/2021GL093804, 2021.

9. 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.

We totally agree with the reviewer and removed that sentence from the manuscript. We also changed one sentence in section 3.3:

"The response of graupel to narrower CDSD reveals an opposite behaviour: some of the days show a graupel increase, others a decrease."

10. 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".

We also did not expect an increase in precipitation rate due to larger shape parameters. But we wanted to quantify for how many times the precipitation rate is above or below the one from the respective reference run. The fact that the shape parameter sensitivity runs have higher precipitation rates in 26–33% of the time, means that lower rain intensities are present for the majority of time. Consequently, the accumulated rain amount is less after 24 h for these runs. We deleted the word "only" from the text and rephrased the following sentence to make that clearer. It now reads:

"The shape parameter sensitivity runs have larger precipitation rates in 26–33% of the time. Thus, lower rain intensities than in the respective reference run are present in the majority of time which explains the reduced precipitation totals after 24 h."
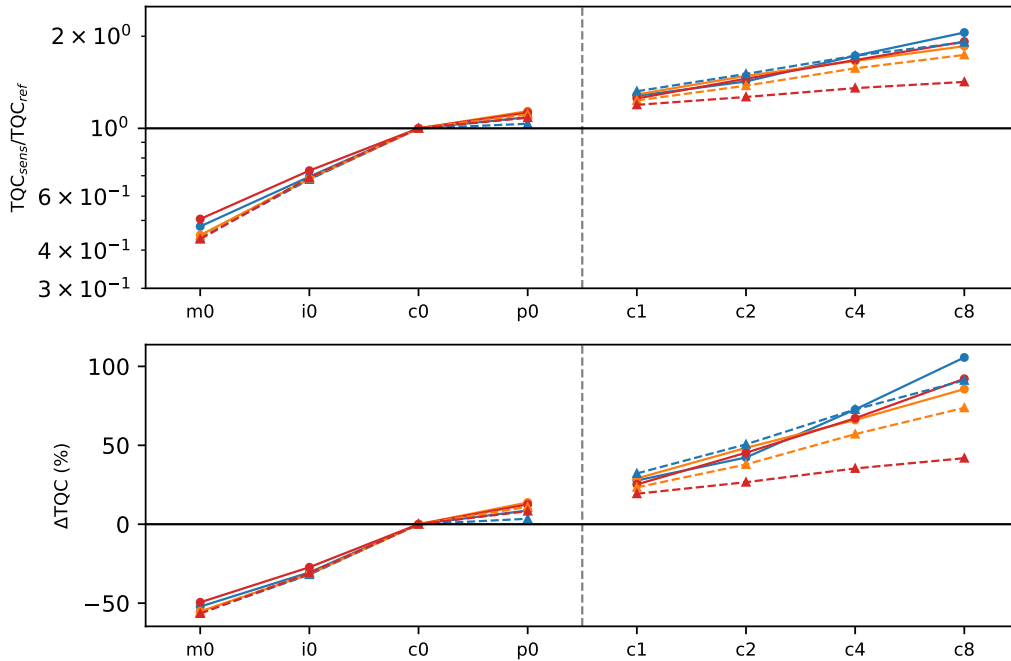
Figure R.3: Ratio of sensitivity to reference run (top) and percentage deviations from the reference run of total cloud water TQC (bottom).

11. 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.

    We followed the reviewer's suggestion and modified the x-axis range accordingly.

12. 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.

    Actually, a 200% increase would mean a triplication and not a doubling. We have tested the reviewer's suggestion for total integrated cloud water TQC (Fig. R.3). Although it may mathematically make more sense to plot ratios on a log scale, we believe that the percentage deviations are visually intuitive as well and the percentage numbers may be better suited for understanding the changes with respect to the CCN concentration and shape parameter. We therefore decided to keep Fig. 8 and the entire discussion in section 3.3 based on percentage deviations from the reference run.

13. 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.

    We agree with the reviewer that this behaviour is expected, we just wanted to mention the range of QC deviations. We rephrased the text:

    "The sensitivity of QC ranges from 56% reduction in a clean environment to more than 105% increase with continental CCN concentration and narrow CDSD. "

14. Line 280, Please insert "for" between "Except" and "the".

    done

15. Line 289, "levers" should be "levels"
    done

16. Line 297, does the author mean just "slight reduction" instead of "slight reduction and increase"?
    We meant a slight reduction for one case and a slight increase for the other. We rephrased the text:
    "For two of the cases, the larger shape parameter and narrower CDSD leads only to a slight reduction (11 June 2019) and slight increase (5 June 2016) in riming. "

17. Line 308, Are the effects of initial hydrometeor density and initial size included in the model?
    Yes, all hydrometeor categories have prescribed forms of the initial particle size distribution which evolve as a function of number and mass densities.

18. Figure 10, I would again suggest showing the same range for the x-axis for easier comparison between cases.
    When we use the same x-axis range for all days, details are hard to see for the days with weaker rain intensities (20090701 and 20130911). In particular for the case of 20090701, the behaviour described in the text is not visible anymore. We therefore decided to keep the different axis ranges. A note about the different axis ranges was already included in the figure caption.

19. 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.
    We deleted that sentence, but kept a citation to this paper in the introduction.

20. Line 347-348, "The strongest sensitivity occurs ..." Sensitivity of what to what? Please clarify.
    We rephrased this sentence to
    "The biggest differences resulting from CCN and shape parameter variations occur at maximum rain water contents."

21. 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.
    We also tested the ratio of melting to autoconversion and accretion and find that the results are merely identical to the ratio using deposition and riming as cold rain process. To make it clear that we mean the ratio of rain production via ice to the rain production via warm phase processes, we rephrased the beginning of section 3.5. It now reads:

    "As neither their absolute values nor their relative contribution to warm and cold-rain processes were addressed, we now inspect the ratio of rain formation via ice (i.e. the cold-rain contribution as the sum of vapor deposition and riming) to rain formation via warm phase processes (sum of autoconversion and accretion). The analysis of this ratio as a function of mean precipitation rate in Fig. 10 reveals a dominant cold-rain contribution for most of the cases. "

22. Line 359, I find that people mean many different things by "CDSD sensitivity". I think in this case the authors mean shape parameter sensitivity.
    Throughout the paper, we differentiate between CCN sensitivity and CDSD sensitivity (meaning the shape parameter sensitivity). But as changes in the CCN concentration also affect the cloud droplet size distribution, we agree with the reviewer that it is better to say shape parameter sensitivity instead of CDSD sensitivity. We changed it throughout the entire manuscript. We also changed "CDSD-runs" to "shape parameter-runs".

23. Line 386, can the authors define "dominant value of each CDSD"? Do they mean "modal"?

Yes, the dominant value is the most frequently occurring value in the distribution. We changed it to modal value in the text and also replaced D_{dom} with D_{modal}.

24. Eq 3, should be the integral of LWC/r_{eff} constants because reff 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.

We compute the cloud optical thickness τ_c after Serrano et al. (2014), who use a simple approximation given by Stephens (1994):

$$\tau_{\mathrm{c}} = \frac{3\mathrm{LWP}}{2\rho_{\mathrm{L}}r_{\mathrm{eff}}} \tag{1}$$

Where LWP is the total vertical liquid path of the cloud, ρ_{L} is liquid water density and r_{eff} is droplet effective radius. For computational reasons, we averaged the effective radius over the height levels of 2, 3, 4, 5 km agl.

Serrano, D., Nez, M., Utrillas, M. P., Marn, M. J., Marcos, C., and Martnez-Lozano, J. A.: Effective cloud optical depth for overcast conditions determined with a UV radiometers, Int. J. Climatol., 34, 39393952, https://doi.org/10.1002/joc.3953, 2014.

Stephens GL. 1994. Remote Sensing of the Lower Atmosphere: An Introduction. Oxford Univ Press: New York, NY; 521.

We included the information about averaging r_{eff} and the reference of Serrano et al. (2014) in the paper.

25. 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.

We do see a strong sensitivity in the dominant cloud droplet size (i.e. the maximum of the CDSD) and the cloud optical depth also for the shape parameter variations, it is only for the effective radius and the cloud albedo that the shape parameter runs show a weaker response than the runs with varied CCN concentrations. So the high CCN concentration of the reference run is not responsible for a general weaker response in the shape parameter runs. However, for most of our results, we find that going from maritime to continental yields a much larger response than from continental to continental polluted conditions. The question if shape parameter variations with other CCN concentrations show a stronger response is currently ongoing work in our group. We added this comment:

"The weaker response of the effective radius and the cloud albedo to shape parameter variations could be influenced by the fact that the reference run already has a comparatively high CCN concentration. We are currently working on the question of how different shape parameters behave at other CCN concentrations."

26. 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.

Some of the 0s were missing due to a too small viewport, this has been corrected now. However, we like to stick to our selected visualization form with the logarithmic frequency of updraft grid points and percentage difference as we believe that the necessary features are obvious to the reader. The same form of analysis and display was also performed in a recent model intercomparison effort studying the impacts of varying CCN concentrations on deep convective cloud updrafts by Marinescu et al. (2021). This paper is also cited in our manuscript.

27. 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 p0 and c1 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 c0 result at the start of the shape parameter test set.

This is a good point, we changed Figs. 5, 8, and 12 in the proposed way. We did not repeat the c0 result at the start of the shape parameter test set, because this would be a repetition and for most illustrations, it is zero anyway.