RESPONSE TO REVIEWER 1:

I agree to have the paper accepted by ACP. The only comment that I have is that the authors might want to check the aerosol concentrations in the WRF-SBM simulations that they added in the supplemental material to make sure the simulations were done correctly, because there is a bug in aerosol setup in the SBM version in WRF3.6.1.

Thank you for highlighting this potential technical issue. We have checked our supercell SBM results against a newer version of the SBM in WRF (WERFv3.7.1) and our results and conclusions are not changed.

RESPONSE TO REVIEWER 2:

Review of the paper "Uncertainty from choice of microphysics scheme in convection-permitting models significantly exceeds aerosol effects",

authored by Bethan White, Edward Gryspeerdt, Philip Stier, Hugh Morrison, Gregory Thompson, and Zak Kipling

1. In my view, the most interesting section in the revised paper is the analysis of the reasons of differences between the results obtained using these two. The authors stress a crucial role of representation of autoconversion process. The Thompson scheme uses one of versions the Berry and Reinhard (1974) autoconversion scheme. Gilmore and Straka (2008) (this study is referred now in the new article version) showed that the rates of autoconversion predicted by different versions of the Berry and Reinhard (1974) scheme differ by orders of magnitude. So, a justification of the choice of particular autoconversion scheme is required.

The reviewer is correct that Gilmore and Straka very nicely illustrated a very large sensitivity of autoconversion results dependent upon how the subsequent authors implemented the exact details of Berry and Reinhardt (1974; hereafter B&R74). This is quite illustrative of the problems as one person or another does or does not follow the identical details as found in the original research. This is also well highlighted in Thompson et al. (2004): note in particular the reference to Walko et al. (1995) in the footnote at the bottom of page 521. We justify our choice of staying with B&R74 by stating that we have compared its results as in Thompson et al (2004 and 2008) against a bin/explicit microphysics scheme of Geresdi (1998) and found very favorable comparisons. One coauthor of the current manuscript (G. Thompson) has extensively compared various autoconversion schemes, including one that gained widespread usage in recent years by Khairoutdinov and Kogan (2000) and still believes the choice of B&R74 to be superior via comparisons to observations in real-time numerical weather prediction models.

To make this justification clear to the reader, we have made note of this in our revised manuscript in the Section 3.4 where the autoconversion representations are introduced, at Page 18, Lines 26 – 32.

2. The Morrison scheme uses parameterization of autoconversion developed by Khairoutdinov and Kogan (2000) for drizzle formation in marine stratocumulus. Note that the mechanism of drizzle formation in Sc substantially differs from raindrop formation in Cu and, of course, in deep convective clouds. In this relation, Khairoutdinov and Kogan (2000) wrote in their article: 1) "The proposed bulk microphysical parameterization has been developed and tested for thermodynamic conditions typical for the midlatitude and extratropical stratocumulus layers formed over the areas of upwelling off the west coasts of continents; therefore, it may not be valid to extrapolate its use to other cloud types and conditions" and 2) "We have to emphasize that the proposed scheme is intended for LES of convective STBL with a spatial resolution of tens of meters. Such an LES resolves most eddies of turbulent flow and, consequently, spatial variation in supersaturation, water content, cloud condensation nuclei (CCN) count, drop concentration, etc. This auxiliary information enables one to add a level of complexity to the traditional bulk microphysics schemes by adding, for example, the explicit CCN–cloud drop concentration feedback, as done in this study. Therefore, the proposed scheme cannot be simply extrapolated for use in larger-scale models since the derived water conversion rates depend *nonlinearly* on local (eddy scale) cloud variables".

So, on one hand it is good that the important reason of the differences between results of the Thompson and of the Morrison schemes is found. On the other hand, a justification and reasoning of utilization of the Khairoutdinov and Kogan (2000) parameterization for conditions quite different from those in Sc are required.

The reviewer is correct that the Khairoutdinov and Kogan (2000; hereafter KK2000) scheme was initially developed and applied for LES of stratocumulus. This has motivated the implementation of additional options besides KK2000 for autoconversion and accretion in newer schemes such as P3 (Morrison & Milbrandt 2015a,b). However, we note that other than varying the prescribed values of cloud droplet number concentrations we are running the schemes in their baseline configurations as available in the main WRF release. Thus, although we are not advocating the use of KK2000 for non-stratocumulus cases, the case we present is the same configuration used by any other WRF user running deep convection simulations with the Morrison scheme.

Secondly, we agree with the reviewer that virtually all physically-based autoconversion/accretion schemes suffer from the issue of spatial resolution and not resolving local variations in water content and concentration, since they are typically based on bin or numerical model calculations of the growth of drops by collision-coalescence based on local water contents and concentrations. This brings up the larger issue of the effects of sub-grid scale cloud water variability on microphysical process rates. This is of critical importance in large scale models, and has been addressed by coupling schemes like KK2000 with a sub-grid scale pdf representation of cloud water (e.g., Morrison and Gettelman 2008, J. Climate). On the other hand, the effects of sub-grid scale cloud variability on grid-mean autoconversion and accretion is less clear for models at convection-permitting scales, although nearly all models neglect the effects of sub-grid cloud water variability at these scales. While the authors recognize the potential importance of grid resolution sensitivities at these scales, due not just to cloud water variability but especially sensitivity of the cloud/convective dynamics, we note that we are running the model and microphysics schemes in the typical setup for a convection-permitting model (that is, neglecting sub-grid cloud variability).

We note that one of the main aims of our study is in fact to highlight the uncertainty in commonly used model configurations, which are exactly based on these schemes.

We have made note of this justification in our revised manuscript, at Page 19, lines 6 – 9.

3. The authors illustrate vertical profiles of mass contents of different hydrometeors averaged over the entire computational area. As a result, all information concerning the microphysical structure of clouds simulated by different schemes turns out to be lost, at least for specialists in cloud microphysics. It is necessary to present vertical profiles of maximum values of the mass contents. The profiles of cloud averaged values would be also useful. These figures should be accompanied by corresponding comments and analysis.

We note that in the first revision of the paper we updated all profile figures to present both domain-averaged cloudy column profiles, as included in our original manuscript, and cloud-only averaged profiles as requested by the reviewer in round 1 of the review. This was accompanied by corresponding comments and analysis.

As noted in our previous response to the reviewer, the relevant Figures are Figures 6,7,8,9,11,12 and an example of the condensed-point (cloud and precip) average profiles was provided in our response to Reviewer 2's comment 12 in the first set of responses to the reviewers.

However, we do not agree that showing domain-maximum values is the best way for comparison of the cases we present. Although such analysis is useful to understand the evolution of a single cloud, such maxima in our simulations would be calculated over many cloud types and regions in the Congo case, and over multiple cloud lifecycles in both the Congo case and the RICO case. Maximum values of the mass contents represent just one point in the entire domain and can lead to improper conclusions, especially in our Congo simulation where multiple cloud types exist in the same domain but do not necessarily interact.

4. I still do not understand the reason of the existence of small cloud droplets near the surface (Fig. 6 and Fig. 7). If spontaneous breakup of raindrops is not included, the reasons of this very strange effect should be explained. What is relative humidity in the BL?

We note that the profiles in Figures 6 and 7 show the mass mixing ratios, not number concentrations, of cloud water contents (cloud droplet numbers are prescribed in both the Morrison and Thompson schemes). Therefore, the profiles show a large amount of liquid cloud mass in the Thompson scheme, which we have shown to rain out in the Morrison scheme (the authors note that raindrop breakup is included in both the Morrison and Thompson schemes, following the form in Verlinde & Cotton but implemented slightly differently using different diameter thresholds, different self-collection efficiencies etc).

We showed in our first response to the reviewer that this is low-level warm cloud that forms over the ocean. Since both sets of simulations are driven by the same initial and boundary conditions, this could be due to a moist bias in the reanalysis in this region (Washington 2013). However, in a region such as the Congo basin the presence of significant low-level moisture is not at all surprising. Further, it is likely that some of the low cloud formation is driven by saturation from rain evaporation.

Figure R1 shows the mean relative humidity profile for all sets of simulations. Mean values (all domain, all simulation) of relative humidity in the BL is between 55 and 60%, with values higher in simulations using the Morrison scheme. That low-level relative humidity in the (precipitating warm cloud) Morrison simulations is greater than that in the (non-precipitating warm cloud) Thompson simulations, when both are driven by the same BCs, strongly suggests the effect of rain evaporation increasing low-level humidity in the Morrison simulations.

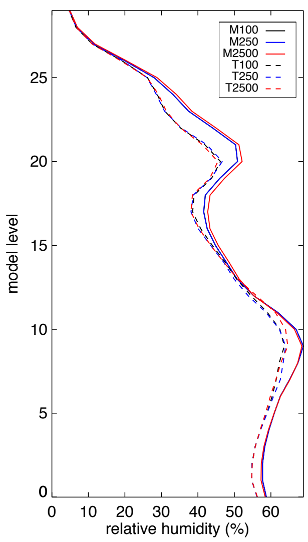

Figure R1: *Congo case: domain mean profiles of relative humidity over the 10 days of the simulation for the Morrison (solid lines) and Thompson (dashed lines) schemes, for simulations with prescribed cloud droplet number concentrations of 100 (black lines), 250 (blue lines) and 2500 (red lines) drops per cc.*

5. The section of Conclusions was improved, but still remains weak. The key finding as it is formulated in the paper is: “In the context of our finding, this strongly suggests that an accurate description of the autoconversion process in warm-rain regimes is fundamental not only to a realistic representation of cloud and precipitation, but also to its response to varying aerosol concentration”. This finding is not new and seems somehow trivial. It is not necessary to analyze in detail three case studies to conclude that the level of raindrop formation and the growth rate of raindrop mass are of crucial importance for warm and mixed-phase cloud microphysics. Actually, autoconversion rate determines the difference between cloud types: maritime vs. continental. No solution or even suggestion concerning the ways to improve the representation of the autoconversion is proposed in the study. At the same time two moment bulk schemes allow calculation of the mean volume radius. Note that the mean volume radius is a very robust quantity, which vertical profile

depends on droplet concentration, i.e on the CCN concentration. The mean volume (or effective) radius can be also calculated using adiabatic LWC and droplet concentration. It is also known that raindrop onset begins when the mean volume radius exceeds its a critical value of 13-14 um (Freud and Rosenfeld, 2012, Khain et al., 2013, Rosenfeld et al. 2014). This allows to calculate the height of the first raindrop formation quite accurately. Might be this condition can be used for testing and improvement of the schemes?

We note that the finding quoted by the reviewer is not our key finding. Indeed, our key result is presented as 'variabilty in aerosol response due to choice of microphysics scheme differs not just between schemes, but that the inter-scheme variability differs between cases of convection'.

We agree with the reviewer that for warm clouds, autoconversion should be the main way in which differences in droplet concentration affect simulations (as opposed to accretion, whose dependence on number concentration is not even included in KK2000, or droplet sedimentation, or effects on the droplet size distribution shape parameter that depends on the number concentration in both the Morrison and Thompson scheme used in this study). However, we note that the paper is already long and contains a lot of analysis, and whilst further testing of the autoconversion process between the two schemes would be interesting it is not the focus of this paper. Furthermore, the Thompson scheme's implementation of B&R74 contains the principle point that the reviewer makes: that collision-coalescence produced warm rain begins at almost exactly 14 microns using the method as implemented by Thompson. This is one of the principle reasons B&R74 was chosen rather than K&K2000. While other persons who claim to implement B&R74 (as pointed out by Gilmore and Straka), they have often done so not using the exact same 3 characteristic diameters of B&R74's original paper; whereas Thompson has.

We make note of this justification of the B&R74 implementation in page 18, lines 26 – 32 of our revised manuscript.

We have included in our discussion a note that care should be applied using autoconversion schemes in different regimes for which they were originally developed, as in the KK2000 scheme. This can be found on Page 26, lines 17 – 19 of the revised manuscript.

We note that cloud droplet number concentration is prescribed as a parameter in both bulk schemes used in this paper (i.e. neither version of the scheme used here is prognostic in cloud droplet number concentration). Therefore, for the K&K2000 scheme calculation of the mean volume radii based on the specified number concentrations is unlikely to provide much meaningful information (note that, as described above, B&R74 does effectively calculate a volume-based mean radius). However, we have included a description of the reviewer's suggested method to calculate the effective radius for readers who may be interested to perform such tests in schemes with prognostic cloud droplet number concentrations. This can be found on Page 26, lines 29 – 31 of the revised manuscript.

However, we emphasize again that the focus of the paper is not to make improvements to each of the schemes, but to highlight the wide variability in response of these two bulk microphysics schemes to aerosol not just with respect to each other, but more importantly with respect to each other between types of convection – a result that can likely be extended to all bulk schemes and therefore to global models as well as cloud-resolving models.

I also recommend to refer the recent studies by Igel and van den Heever (2016a,b, 2017), where different values of the shape parameters of Gamma distribution is considered and important reason of difference between the results of bulk schemes. Igel and van den Heever proposed the optimum shape parameter of gamma distribution at the stage of diffusion growth.

The suggested references are now included in our revised manuscript on Page 3, lines 24 – 33.

Of course, available bin microphysics models (WRF, SAM, parcel models with a very detailed description of raindrop formation) can be useful. The discussion of the possible ways to improve the autoconversion schemes is desirable.

We have included the reviewer's suggestion of ways to test and improve schemes on Page 26, lines 32 – 33 of the revised manuscript.