Review of the ACP manuscript "Can models robustly represent aerosol–convection interactions if their cloud microphysics is uncertain?" by White et al.

The authors used the WRF model coupled with two double-moment bulk microphysics scheme to perform cloud system-resolving simulations of convection in the Congo basin, an idealized supercell, and a case of shallow cumulus convection, and tested the sensitivity of the simulated hydrometeors and precipitation to the microphysics scheme and to CDNC perturbations. The authors showed the simulations are sensitive to microphysics parameterizations much more than CDNC, which has been showed in previous studies but this study highlighted this point to imply aerosol effects are the secondary compared with the uncertainty in cloud parameterization. They further examined the shallow cumulus convection case and found that representation of autoconversion is the dominant factor that drives differences in rain production. The paper is generally written clearly but there are confusing sentences. They are some perspectives that can not be well justified physically such as saying the aerosol-cloud interactions in their study represent the upper limit.

The major problem is that their main point is based on an assumption that those two-moment schemes well represent the aerosol-cloud interactions, which is not the case based on the many past studies and a recent review paper by Khain et al 2015. Two-moment schemes have significant limitations in aerosol-cloud interaction process parameterizations such as nucleation, diffusional growth, and sedimentation, etc (detailed Khain et al. 2015). The paper did not really address the question "Can models robustly represent aerosol–convection interactions if their cloud microphysics is uncertain", so that the title needs to be changed. More literature survey is needed, especially about those studies comparing different microphysics schemes and their responses to CCN or CDNC. Those studies need to be discussed in the introduction and the relevant places in the paper, especially about some important points on the problems with the parameterizations of some specific microphysics processes in the bulk schemes. Therefore, the paper needs major revisions to be accepted as a publication in ACP.

Specific comments:

1. The title needs to be changed. It is relevant but the authors did not conduct an unique study to really address this question. The two-moment schemes can not robustly represent aerosol–convection interactions due to the limitations in representing the most relevant processes as detailed in Khain et al. 2015. If a bin scheme is used, you might end up with similar magnitudes of aerosol indirect effects as the differences among different microphysics schemes. In addition, for specific case simulations, one can not really reveal how it is impacted by aerosols by conducting simulations with realistic aerosols/chemistry configuration. Any aerosol properties and spatial distribution change could change cloud and precipitation.

2. P1, L21-22: aerosol can affect through aerosol radiative effects as well.
3. P1, L24: Albrecht, 1989 actually showed the suppression of precipitation but for warm clouds. So, the sentence is not accurate.
4. P2, L29: "Until recently" should be deleted.
5. P4, L19-25, these sentences could be misleading. First, it is not clear what the authors

mean by saying "the response of different microphysics schemes to perturbations in prescribed cloud parameters". Second, some past studies showed qualitatively different aerosol impact for different cloud types, with a purpose of illustrating a point that aerosol impacts depend on cloud type and dynamical and thermodynamic conditions of each case. So it is misleading to describe a study without giving information about cloud types or specific dynamical and thermodynamic environment. For example, the description about Fan et al. 2012 about aerosol reducing precipitation is not correct. The study did two different cloud cases over the eastern China – one deep convective cloud case with warm cloud base and weak wind shear and the other a winter stratiform cloud case, with aerosol increased precipitation for the former but reduced precipitation for the latter.

6. P5, L3: cloud and precipitation responses > cloud and precipitation responses to perturbation of CDNC
7. P5, L25: Khain and Lynn 2009 is not s study with specified CDNC. CDNC is prognostic in the bin model they used.
8. P6, first paragraph, RRTM and Goddard schemes only talk hydrometeor mass from microphysics calculation (Goddard shortwave scheme takes prognostic CDNC as well). The microphysics-radiation coupling does not account for particle size changes, which means some aerosol effects are missing in those studies.
9. Figure 3, the color scheme needs to be changed. The color difference is too little even between 0 and 100 mm.
10. Figure 5, why compare with the climatology data? This is just a 10-day run, how should we expect it represent the climatology?
11. P. 9 L5-7, this sentence is confusing. Need to be clarified.
12. P. 9, L19: domain averaged cloud water in T250 is 140 times larger than M250. Something could be wrong here. Can you plot the cloudy-point average for cloud water mass total hydrometeor mass to check if they make sense? What is the maximum cloud water mass in T250 and M250, respectively?
13. Figure 6, this figure need to be replotted to show differences of other hydrometeor mass clearly. Right now, only cloud water differences can be seen clearly. I would use different panels for different hydrometeors.
14. P9, last paragraph and Figure 7, the huge increase of cloud water with the increase of CDNC with the Thompson scheme seems not reasonable. How about the change of precipitation?
15. P. 10, L19-21: The sentence "we also see that the simulated hydrometeor classes differ between cases: the difference in the simulated hydrometeor classes in the idealised supercell configuration is different from the difference in the real-data Congo basin configuration" is not necessary. This is what it should be since they are different convective cloud types.
16. P11, first paragraph, the sentences in L5-6 and in L10-11 are repeated.
17. P11, first paragraph, the main point here should be about more significant aerosol impact on hydrometeor mass on the supercell case compared with the Congo case, not the different responses of hydrometeors between the CONGO case and the superell case, since they should be expected for completely different cases. Many past studies have showed that aerosol impacts depend on dynamics and thermodynamics of convective clouds (e.g., Khain 2009; Fan et al. 2009).

18. P11, last paragraph: the lack of appropriate sensitivity of bulk schemes to aerosols is mainly due to the limitation of bulk scheme parameterization in nucleation, diffusional growth, and sedimentation, etc, as detailed in Khain et al. 2015. The invigoration of updrafts can not be simulated since the saturation adjustment approach for diffusional growth of droplets limit such effects. Those aspects should be considered and discussed when interpreting the results on aerosol indirect effects here. Past studies showing the limitation of bulk scheme parameterizations in representing aerosol-cloud interactions need to be surveyed and discussed.
19. P12, L5-7: Again physically it is supposed to be that for different types of convective cases. Hydrometeor differs and hydrometeor responses to CDNC are different as well.
20. P13, L23-25, reword the sentence. Not sure what you really want to say.
21. P15, first paragraph, the authors showed the autoconversion process is not the significant process contributing to the large cloud mass at low –levels. Then the question is what process mainly contributes to it?
22. P15, L19-20, again, the limitation of two-moment bulk schemes in representing aerosol impacts on microphysics processes should be discussed.
23. P17, L21-23, this sentence appears in a few places throughout the study, but the point can not be well justified even for aerosol indirect effects. First, you not know what the reality of aerosol look like in composition and spatial variability, many studies showed that aerosol spatial distribution could significant change storm location such as urban aerosols impact significantly on the precipitation in the downwind area of cities through aerosol indirect effects. Second, since two-moment bulk schemes even can not represent aerosol-cloud interaction processes physically, then how do you justify the aerosol impact here represent the upper limit?
24. P18, L29, this is definitely not the first study to consider two and more cloud cases. A thorough literature search would give you those past studies.
25. P18, L30-31, again, it has been a basic understanding that hydrometeors and their responses to CCN or CDNC vary with different cloud types and convective cases.