Response to Anonymous Referee #2

1. I have reviewed "Effects of Liquid Phase Cloud Microphysical Processes in Mixed Phase Cumulus Clouds over the Tibetan Plateau" by Xu et al. I do not have any complaints about the analysis itself, but I have serious doubts that the manuscript is sufficiently relevant beyond the one case investigated to be within the scope of ACP. The article describes an analysis performed on a single synoptic system transiting the Tibetan Plateau. The authors focus on warm cloud processes, performing sensitivity studies with different autoconversion/accretion/droplet evaporation parameterizations; why they focus on these processes is not well explained, in particular since the precipitation in their study case is clearly initiated in the ice phase (figure 4), so one would expect that only accretion and ice/mixed-phase processes matter.

Reply: Thanks for the positive evaluation, and we respectfully disagree on the relevance comment. Yes, accretion and ice/mixed-phase processes matter. However, autoconversion and entrainment-mixing processes could be important as well, esp. in context of understanding the outstanding problem of overprediction of precipitation over the Tibetan Plateau. The reason is that accretion is the collection process between rain drops and cloud droplets, which are significantly affected by rain/cloud liquid water content and number concentration. These microphysical properties are influenced by autoconversion and entrainment-mixing processes. These three processes are likely to be intertwined.

So far, it is still unknown how the three liquid-phase processes affect TP precipitation, and whether improving the parameterizations of these three liquid-phase processes can mitigate the problem of overpredicted precipitation. Also unknown is which liquid-phase process is the most important in affecting TP precipitation, and which commonly used scheme can best describe the most important process and why. To address the concern, we have reframed the structure of introduction (Page 4, Line 55-66; Page 5, Line 81-90).

In addition, to further highlight the main purpose of this study, the phase “precipitation overprediction” is emphasized in all sections in the revised manuscript. For example, the titles of Sections 3.2 and 3.3 are modified to be “Sensitivity of precipitation overprediction to different liquid-phase processes” and “Reasons for improvements of precipitation overprediction in CP2k”, respectively.

According to the referee comment, we have significantly shortened the description of the simulations with different autoconversion and entrainment-mixing schemes. Please see the response to Comment 2.

2. Not surprisingly, the authors find that autoconversion and homogeneous vs inhomogeneous cloud droplet evaporation make very little difference in accumulated precipitation. In a revised manuscript, I would suggest getting rid of several pages of unsurprising results and replacing them simply with one paragraph along the lines of, "we analyzed the effect of different autoconversion parameterizations and mixing assumptions and found them to have no substantial impact."

Reply: Thank you for your suggestions. We have significantly shortened the detailed description from several pages to less than one page (Line 349-369). By combining Comment 3 and the other referee comment, we also add some discussion with previous studies in a very concise way (Supplement, Page 1-2, Line 9-22).

3. The finding that accretion is an important control on accumulated precipitation is also not very surprising. Furthermore, it is not clear what we are supposed to do with this information. When parameterizations are developed, they are usually tuned to do something reasonable in one or a handful of test cases, but it is understood that they will probably not give results that match observations in every conceivable case – usually far from it. So it is not surprising that some parameterizations do better than others at reproducing this particular case. However, that does not mean that the winner in this case will produce the best results in other cases. Are the authors recommending that the Cohard and Pinty (2000) accretion parameterization should be used generally, or generally for Tibetan Plateau studies? How does one case study support that recommendation? If that is not the recommendation, what is new or useful about the results? That different warm cloud microphysics schemes can lead to wildly different simulations of individual cases is nothing new; for an example of a study that draws this conclusion in a more generalized way, with interesting statements about science implications, see White et al. (2017), https://doi.org/10.5194/acp-17-12145-2017
Thus, I recommend that the authors substantially revise their manuscript to focus on conclusions that are of use beyond this one case study. If this is not possible, I do not think that ACP is the appropriate journal.

Reply: Thank you for the suggestion. We have revised the paper accordingly. Briefly, month-long simulations and related discussions are added in Section 4 in the revised manuscript (Page 25-26). Figure R1 and Table R1 are also added in the revised manuscript. We design one-month simulations which start from 0000 UTC 21 July to 0000 UTC 21 August, 2014 with the same domains using the three accretion schemes (the KK00 scheme in the control run, CP2k, and Ko13) to figure out whether the mitigation of overprediction on precipitation from the CP2k only works in the particular case in Section 3 or is generally valid.

Figure R1 shows the temporal evolution of the area-averaged daily precipitation rate in domains 02 and 03 from the three accretion simulations and the observations. Compared with the observed precipitation, the control run significantly overestimates precipitation for most days, especially in domain 02. The results of Ko13 are very close to those in the control run, while CP2k significantly reduces precipitation overprediction with *p*-values of student's t-test less than 0.01 for both domain 02 and domain 03. The average precipitation rate in the observation, the control run, Ko13, and CP2k are, respectively, 1.56, 2.46, 2.49, and 2.17 mm/day over domain 02, and 4.54, 5.80, 5.87, and 5.17 mm/day over domain 03. These numbers confirm the better performance of CP2k than the other accretion schemes. Table R1 shows that CP2k has higher HSS scores than the control run and Ko13 over both domains 02 and 03.

Therefore, the effects of CP2k on reducing precipitation overprediction are not limited to our specific case. It is recommended that the CP2k accretion parameterization should be used generally, at least for Tibetan Plateau studies. We would argue that the conclusion is valid in other regions beyond the Tibetan Plateau, according to the theoretical analysis in Section 3.2.2. We add these discussions in the revised manuscript (Page 30, Line 610-616). Especially, the sentence "More studies are needed to understand whether these findings are applicable to regions beyond the Tibetan Plateau as well " is added.

We also agree that many studies have found that different warm cloud microphysics schemes can lead to different simulations of individual cases. Combined with the other referee comment, we have added some discussions and White et al. (2017) is cited (Page 25, Line 508-511; Page 6, Line 101).

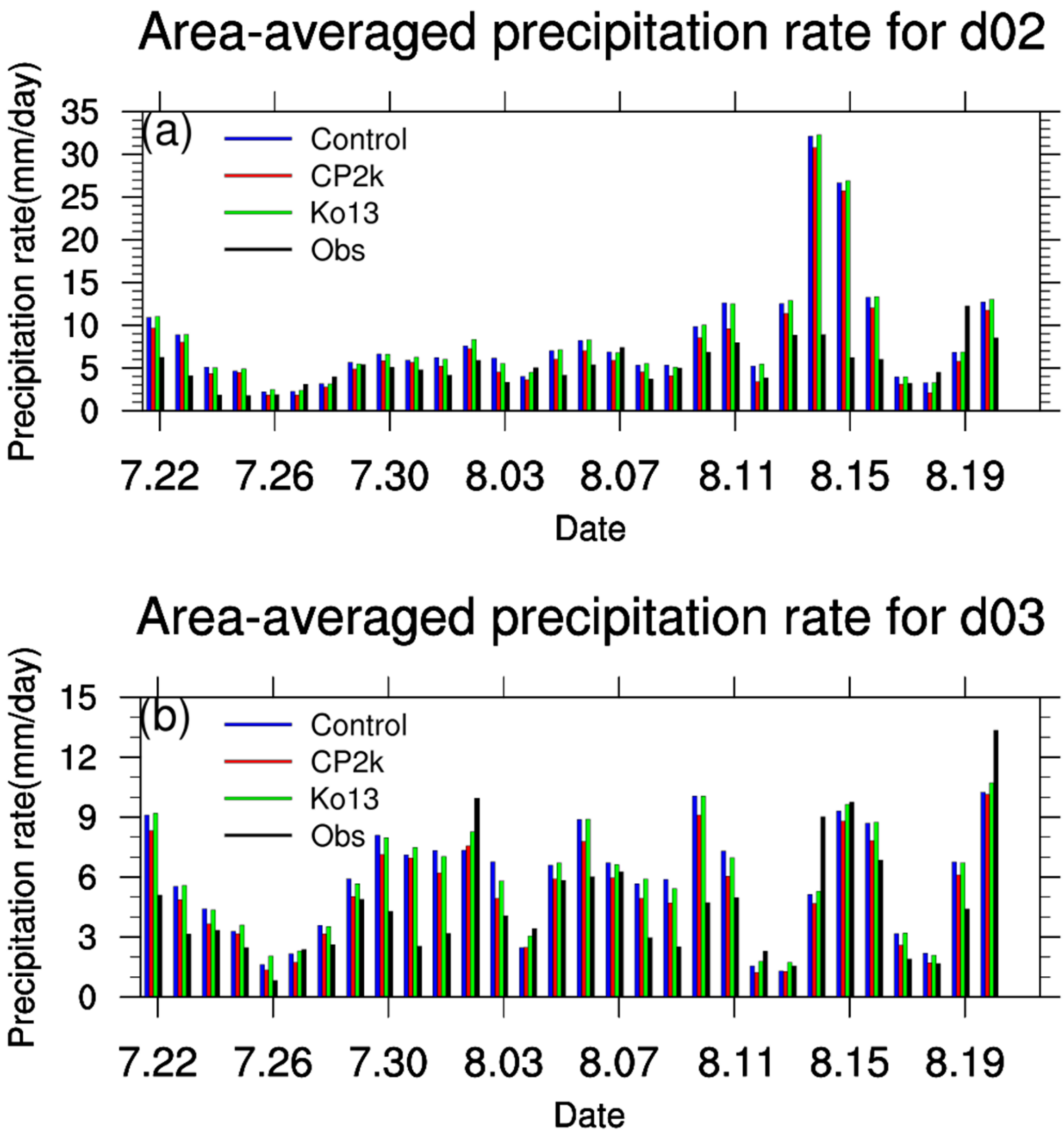

Figure R1. Time series of area-averaged daily precipitation rate (mm/day) from 0000 UTC 22 July to 0000 UTC 20 August 2014 over (a) domain 02 and (b) domain 03 in the observations and three accretion cases (the control run, CP2k and Ko13).

Table R1. The values of four elements a-d and Heidke skill score (HSS) for three one-month simulations over domain 02 and domain 03 of the control run and CP2k, Ko13 (different accretion schemes).

	a	*b*	*c*	*d*	HSS
domain 02					
Control	3780	5052	1924	24584	0.403
CP2k	3749	4369	1955	25267	0.435
Ko13	3764	4825	1940	24811	0.413
domain 03					
Control	1188	2856	93	2538	0.220
CP2k	1163	2355	118	3084	0.262
Ko13	1181	2908	100	2531	0.211