Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1063-RC2, 2020

Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Effects of Liquid Phase Cloud Microphysical Processes in Mixed Phase Cumulus Clouds over the Tibetan Plateau" *by* Xiaoqi Xu et al.

Anonymous Referee #2

Received and published: 4 February 2020

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.

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."

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.

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2019.