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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 #1

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Review for Xiaoqi Xu et al. "Effects of Liquid Phase Cloud Microphysical Processes in Mixed Phase Cumulus Clouds over the Tibetan Plateau"

General comments:

The authors investigated the effects of liquid microphysics scheme (e.g., autoconversion and accretion) on the surface precipitation rate over the Tibetan Plateau (TP) using Weather Research and Forecasting (WRF) model with the double-moment Morrison scheme. Although the authors stated that the impact of entrainment mixing was also explored, I did not find enough discussion and supporting data.

In the beginning, the authors addressed that the model resolution is critical for pre-

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dicting accurate precipitation over the TP region, but the main body of the manuscript looked at the different treatment of liquid microphysics scheme assumed in the model. I unfortunately do not understand the main purpose of this study because it is still unclear why the authors chose the TP region to investigate uncertainty in the microphysical scheme. I strongly suggest reframing the overall structure, in particular the introduction section at least, to highlight the study goal, motivation, and new findings of this study.

It is an interesting result that the accretion scheme which depends on raindrop size (Cohard and Pinty, 2000) as well as cloud and rain mass mixing ratios (qc and qr) performs better than the other standard accretion schemes which depend only on qc and qr. Although the authors present some interesting results with the scheme comparison, almost findings remained within the scope of the previous studies based on theoretical modeling (e.g., Wood, 2005; Wood et al., 2009; Lee and Baik, 2017) and climate modeling (e.g., Gettelman et al., 2013, 2015). I feel that the authors should add more discussion about the physical mechanism behind the different scheme used.

Therefore, I suggest major revisions and re-review. The authors also have presentation issues to address detailed below.

Specific comments

Lines 55-56 and 59-63: Is it true for the WRF model? Again, is this issue attributed to the model resolution? Or microphysical parameterization?

Line 82-84: This statement is too vague.

Line 86: Where did you state the entrainment-mixing issue? I did not find. This should be removed unless the authors provide data and discussion.

Line 94-95: Please add reference(s) here.

Line 120: Which satellite data did you use? Please add the description and appropriate citation(s).

Line 148-149, Equations (1) and (2): The unit for mass tendency is kg/kg/s in general. Equation (5): Please define xc.

Line 180: “most accretion schemes” needs a couple of references.

Line 219-222: It is unclear because a lower precipitation rate is smoothed by blue. The color scheme in Figure 2 (and also Figure 5) should be modified. In the present figure, I cannot recognize the “rainband oriented in the northeast-southwest direction”.

Lines 347-349 and 353-355, and Figures 7b and 7d: I disagree with this sentence. The autoconversion rate in the CP2k scheme is higher (Figures 7a and 7c), whereas the accretion rate is the lowest. This is inconsistent with your explanation. And also, why the accretion rates are almost similar among various schemes except for the CP2k? If the same accretion scheme (KK00) is used except for the CP2k, it is natural that the accretion rates are almost the same among the other schemes. I am confused about that, and please describe a more detailed explanation.

Sections 3.2.2.1, 3.2.2.2, and 3.2.2.3: These sections should include more discussion. The current description only lists the result from different microphysical scheme use, but the variability due to the different treatment of autoconversion and accretion schemes has been well known (e.g., Wood, 2005; Wood et al., 2009; Gettelman et al., 2013, 2015; Jing et al., 2019). It would help readers if more in-depth discussions with relevant studies are shown in the text.

Line 367-368: The difference of Nc between the INHOMO run and CTRL run is quite small. It is better to conclude that the impact of the entrainment-mixing is less important than the liquid conversion process in this study, although it depends strongly on the cloud type simulated in the TP area. Please add more discussion here (or conclusion section).

Line 400-404: This may imply that the uncertainties in ice- and mixed-phase microphysics schemes are conveyed to the uncertainty of surface precipitation. To confirm this, an additional simulation without the riming process will be helpful to understand the microphysical mechanisms.

Line 424-426: Does it mean that the other microphysical schemes except for the CP2k overestimate the accretion rate and consequent precipitation? Or is this the case only for the TP region? I suggest that the authors refer to the ratio of accretion over autoconversion (e.g., Gettelman et al., 2013; Seifert and Onishi, 2016; Lee and Baik, 2017) among the experiments. This metric will be helpful to evaluate the dependence of microphysical process rates on cloud regimes in the model.

Lines 483-485 and 491-492: Why does the CP2k scheme improve the too early onset of precipitation in spite of the higher autoconversion rate compared with the other schemes?

Figure 1: Please explain the color shade.

Figures 2 and 5: Unclear color contrast. Please consider to change the current color scheme (blue-to-red) to standard rainbow color or white-to-blue etc.

References

Cohard and Pinty (2000): A comprehensive two-moment warm microphysical bulk scheme. I: Description and tests, doi:10.1002/qj.49712656613

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Seifert and Onishi (2016): Turbulence effects on warm-rain formation in precipitating shallow convection revisited, doi:10.5194/acp-16-12127-2016

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Wood et al. (2009): Understanding the importance of microphysics and macrophysics for warm rain in marine low clouds. Part II: Heuristic models of rain formation, doi: 10.1175/2009JAS3072.1

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