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Interactive comment on "Role of sublimation and riming on the precipitation distribution in the Kananaskis Valley, Alberta, Canada" *by* Émilie Poirier et al.

Anonymous Referee #2

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Review of Poirier et al.

General Comments:

This manuscript describes a numerical modeling study of a weak precipitation event in a mountainous region and examines the importance of the microphysical processes of snow sublimation and riming on the phase and distribution of precipitation at the surface. High resolution (1 km grid spacing) simulations were done with the WRF model using a 2-moment bulk microphysics (MP) scheme. Comparisons were made to local observations, focusing primarily at a single site. Model sensitivity tests were performed whereby specific processes were shut off in the MP scheme and the impacts were

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examined. The authors argue that the results illustrate the relative importance of sublimation of snow flakes and snow pellets on altering the temperature at low elevations and thus ultimately the resulting precipitation.

Overall this manuscript is well-written and logically presented, though the figures (regarding the presentation of microphysical fields) need to be improved and reworked (see comments below). The scientific methodology is sound and the conclusions are largely supported by the evidence presented (with some limitations; see below) and provide some understanding of the importance of the processes discussed. As is often the case with studies of this kind that are based on a single case study, the authors need to do a bit more work to illustrate clearly the broader implications of the study. In its present form, the manuscript seems somewhat limited to discussion of the specific details of this specific case. However, this should be straightforward to achieve with some added discussion.

Also, although this is a process study, not an examination of model-specific details, the numerical model – in particular the MP scheme – plays a critical role in the analysis on which all of the scientific conclusions are based. Therefore, I believe that closer examination/discussion of some model details is needed to strengthen the conclusions about the processes and, arguably, to expand the relevance of the conclusions. The manuscript could possibly be published with some improvements to the presentation (see below) and a bit more discussion; however, I think going into some more depth with regards to the MP scheme (see below) could strengthen the paper considerably and I would recommend this approach.

Summary criteria: Scientific significance: 2) good Scientific quality: 2) good Presentation quality: 2) good (but please see specific comments 4 and 5)

Specific Comments:

1. The MP parameterization scheme plays a crucial role in this study. Scientific conclusions are made about the relative roles of sublimation and riming based on what is

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simulated by the MP scheme. But due to the complexity of crystal shapes, fall speeds, the (artificial) conversion between snow and graupel (snow pellets), etc., these are difficult processes to model and different schemes parameterize these processes differently. Thus, as presented, the conclusions are weakened by the fact that using a different MP scheme, or even just changing the parameters within the same scheme (with reasonable bounds) could lead to different results. It is not good enough to simply mention that different MP schemes will produce different simulations of sublimation and melting rates (p. 18, line 22) – this point needs to be addressed somehow, either to strengthen the conclusion or to more thoroughly describe the limitations of the results. This is challenging, but it needs to be undertaken to some degree. One idea would be to do some sensitivity tests with changes to the sublimation rates (e.g. changing the capacitance, which is highly simplified in the MP scheme), riming rates (e.g. changing the collection efficiencies for collection of droplets by snow and graupel), rate of conversion between snow and graupel (this is an artificial process anyway), . . . If you can establish that the conclusions are similar despite changes in the parameterization of the process rates within reasonable bounds, this strengthens the conclusions and addresses the inherent limitation regarding the use of a particular MP scheme. If the overall results change dramatically, this is useful in another way in exposing a limitation in this type of modeling study (but you could still make some meaningful comments about the importance of sublimation etc.). Also, some explanation/discussion about how snow and snow pellets, and the processes examined in the study, are are represented in models, and in particular in the specific MP scheme used, should be included.

2. One of the things that comes out of this study is the importance of riming and the impact on the location of precipitation at the surface on whether the rimed ice stays as “snow” or is converted to “snow pellets”. As mentioned above, the importance of the “conversion” rate and its parameterization should definitely be included in the discussion, as well as the inherent limitations of an MP scheme that has these abrupt transitions between categories. Also, part of the discussion could include other types of weather cases where the distinction between snow or snow pellets plays a role in

determining the location of precipitation in mountainous region. I am thinking specifically of the IMPROVE-2 study, on which there were several modeling studies using MP schemes. In fact, there were a couple of papers published that used the Milbrandt-Yau MP scheme (Milbrandt et al. 2009, MWR; and Morrison et al. 2016, JAS).

3. The "verification" of the CTR simulation, described in the first few paragraphs of section 4, is a bit weak and should be strengthened. On p.8/ln 20, it states "In summary, the weather conditions at KES are generally well represented by the model." First, I suggest changing this to, "...the meteorology . . . is .. well simulated...". More importantly, you should say generally well represented (simulated) for what purpose, because the simulation is not perfect, as shown in Figs. 2-4. I think what you mean is that it is simulated sufficiently accurately that you can proceed to make meaningful conclusions about your scientific objectives based on the model. This should be stated (and defended). A model reflectivity time series, corresponding to the observations in Fig. 3a), would be useful.

4. It would be useful to have precipitation accumulation maps like Fig. 4 (but with (a) separated into snow and snow pellets as separate panels) for all of the sensitivity runs. Or, perhaps better, for the sensitivity runs plot the differences, EXP(x) – CTR, for each precip type. This would illustrate, e.g., the lateral shifts in precipitation when specific processes are shut off.

5. The presentation of the hydrometeor fields in the figures could be improved considerably. First, linear scales for mixing ratios (or mass contents) do not work well. I suggest hand-picking a few specific ranges for the plotting, and be consistent for all hydrometeor types; e.g.: 1e-6, 1e-5, 1e-4, 2e-4, . . . whatever it takes to clearly illustrate and discriminate low and high values. Explain/show better what is meant by "cloud droplets and rain" (Fig. 5a) – e.g. use different colors (note, rain could be present aloft, formed by coalescence). Also, I suggest plotting mass contents (rho_a*q_x), not mixing ratios (q_x). For the time series plots, you could combine Fig. 10 with Fig. 5 (i.e. add Fig. 10a panel to Fig. 5), and do this for all runs. This would remove the need for

Figs. 7, 8, and 10, it would provide more info for the sensitivity runs (i.e. magnitudes of values, not present in Fig. 8). This could either be separate 6-panel figures for each run or a single 24-panel, which is probably doable since you would not need to repeat the color legends or y-axes for each run. All this would go along way to improving the presentation and description of the effects of the various sensitivity runs.

Minor Comments:

- In the atmospheric sciences, and certainly in the field of cloud microphysics, the term “graupel” is used. Is there a reason the authors opt to use “snow pellets”, which is more of a layperson (or weather forecaster) term? Since this is a scientific article that examines microphysical fields and processes, I would think the authors should use “graupel” throughout, and simply mention briefly early on this graupel is often referred to commonly as “snow pellets” (e.g. in the AMS Glossary of Meteorology).

- p.6/ln 33 – I recommend against making a reference to your M.Sc. Thesis and simply make the claim to these modifications here in this paper. Also, what specifically does the correction to the saturation vapour pressure calculation refer to – was this a bug in the original scheme? (And by chance has it been corrected in any recent official WRF releases?)

- p. 7/ln 8, “accreted particles”. Unclear. I assume this means “rimed crystals”. Degree of riming? Partially rimed or bona fide graupel?

- Section 3.3: I suggest adding a table of model runs, with the run name and a brief description. For run names, I would suggest (only) “CTR”, “NO_MLT”, “NO_SUB”, and “NO_SNP” (or, better, “NO_GRPL”). For the SNP run, please elaborate on how, specifically, graupel was shut off. The second paragraph (“The data are . . .”) is not relevant in this section.

- Fig. 3: I suggest adding a panel for model reflectivity, corresponding to panel (a). Also, this and all other discussion/figures about the model simulations are from the

1-km domain only, right? This should be stated clearly somewhere.

- Fig. 4: According to Fig. 3d, a significant amount of the precipitation was from snow pellets, with some snow, at least at that location. Perhaps it would be useful to separate the accumulated precip from snow and snow pellets, rather than combining them in Fig. 4a.

- The time series plots look quite choppy. Is it possible to output the time series with higher temporal frequency in order to produce smoother plots? (Not a big deal; just a suggestion.)

- Was there any "hail" in the simulations? Clearly this case does not support hail in nature, but there is a hail category in the MP scheme which is also used to represent small frozen raindrops. If the model hail mixing ratios are indeed zero in all the simulations, this should be stated and state that for this reason hail is excluded from the figures.

- p. 12, line 6, "Less ice crystals ...[ref to nucleation]. You are not showing number concentration, you are showing mass – there is less ice (crystal category) mass, not fewer number. This is probably not due to nucleation, but rather changes in depositional growth.

- p. 12, line 15, "...more rain reaches the surface because the environmental temperature is higher..." But it is T_wet, not T, that counts (determines melting), right? In that regard, perhaps it would be good to plot the T_wet = 0C isotherm in Fig. 8. (and Fig. 12)

- p. 12, lines 24-26. Suggest omitting paragraph or relocate this as an intro to section 6.

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