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Hydrology and Earth System Sciences
Discussions

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 #3

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Review of "Role of sublimation and riming on the precipitation distribution in the Kananaskis Valley, Alberta, Canada" - E. Poirier et al. (2019)

General comments

This manuscript explores the role of sublimation and riming for a weak precipitation event observed in the Canadian Rockies. The study is done with the WRF model using 1 km horizontal grid spacing and a bulk microphysics scheme. The authors made comparisons to data observed at a single site in order to constrain the model simulation. Then, sensitivity tests were performed in order to quantify the impacts of the melting of snow, the sublimation of solid precipitation and the snow pellet formation on

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the precipitation features. The main conclusion of this study is that the sublimation can have an important impact on the precipitation evolution in a sub-saturated environment at low elevations.

This manuscript is logically presented and the scientific approach is clear. However, few figures need to be improved (see below). Also, the authors need to add some discussions about the limitations of this study since the comparisons between the model and the observed data are performed at a single site and the conclusions are based on a single case study. Moreover, even if the campaign and the numerical tools are clearly referenced, essential details for this study are missing in the manuscript (see below). The manuscript could be published with some improvements to the presentation and more discussion.

Specific comments

1. This study focuses on the roles of the sublimation, melting and riming processes but details about the microphysics parameterizations used are missing. What are the assumptions used to represent the ice species, the conversion between each species, the terminal velocities... then all these assumptions need to be considered in the discussion/explanation of the main results. 2. The local heating/cooling rates associated to the sublimation, the melting and the riming processes are proportional to the mass. It is probably most relevant to plot the mass content of the different species instead of the mixing ratio. Also, the heating/cooling rates can probably be useful to the discussion. You can plot, for example, the vertical profiles of the diabatic heating rate due to microphysics for the different sensitivity tests. 3. The description of the campaign and the available instruments/observations need to be expanded and clarified. For example, Fig 1 shows different sites but the data used in the manuscript were primarily observed at KES. Are there observations available at the other sites? Also, many relevant details for this study are only available in Thériault et al. (2018) and need to be included in this manuscript. It could be interesting to provide a list of the used instruments, the location, the limitations, the observed parameters and the associated references. For

examples, the MMR2 gives the temporal evolution of the vertical profile of the reflectivity and Doppler velocity, and the measurement is affected by the signal attenuation due to e.g. the bright band. Finally, the Parsivel optical disdrometer is mentioned but it is never explained how this instrument is useful. It seems, considering the paper of Thériault et al. (2018), that this instrument is used in order to define the type of the surface precipitation. The different methods (automatic and manual) should be briefly described or at least the authors should specify which one is the most accurate in their opinion.

4. The parameterizations of the microphysics processes evaluated in this study as well as the modifications made to the bulk microphysics scheme of Milbrandt and Yau (2005a,b) should be described in the section 3.2 of the manuscript.

5. The comparison between the CTL simulation and the observations should be discussed in more details, especially the vertical structure. The vertical profile of temperature and dew point temperature obtained in CTL is plotted in Figure2 but never mentioned in the manuscript. The temporal evolution of the vertical profile of the precipitation field observed at KES is given in Fig3a but not compared with the simulation results; at least qualitatively due to the signal attenuation due to the bright band (Matrosov, 2008). MRR2 also provides the Doppler velocity fields; is it possible to compare and assess the species fall speed simulated in the CTL run? Moreover, it is stated several times that the model well reproduces the surface observations. You should say that the CTL simulations reasonably reproduce the observations in order to perform sensitivity studies. However, few parameters simulated in CTL differ from the observations. Indeed, a time shift is visible in the temporal evolution of the accumulated precipitation and the temperature. Do you estimate the impact on the results of this comparison between observations and CTL simulations if you choose another grid box?

6. The figures used to illustrate the sensitivity tests are difficult to interpret. I suggest plotting the differences between the CTL simulations and each sensitivity results. 7. There are spelling and grammar errors throughout the manuscript. I suggest that the

authors read through it carefully and clean it up before resubmitting.

Minor comments

P1-L24. “rain-snow boundary” term is used but defined in the following paragraph. Moreover, the definition is confusing. It is equivalent to the radar bright band/melting layer? P2-L8. Sometimes the term solid precipitation is used. Is it ice-phase precipitation? Or precipitation with high density $\neq$ snow? P2-L12. “using numerical simulations” What type of simulations: 1D, 2D, 3D? P2-L23/28. These studies were performed over mountainous area? P3-Fig1. The title of the right panel is not clear. Do you mean domain area? P3-L11. The name of the field campaign is only given in the conclusions section and should be mentioned here. P4-Fig2. The authors may consider adding details on the skewT-logP diagram in order to define the Lifted Condensation Level and Tw. P4- L9. The relative humidity is never given in the manuscript. If available, the temporal evolution of the relative humidity should be added to the Fig 3. P4-L10. The bright-band is close to the surface at the KES station? P5-L1. “brief period of only snow”. According to Fig3e, there is no S period? Fig3. You should increase the y-axis because the reader may have difficulties to extract the values, for example for the temperature. Few elements are missing in the caption: wind barbs definition, hatched region in fig 3a. P6-L5. The boundary conditions forcing is every 3h, 6h or 12h? P6-L7. Add the number of grid points of the innermost domain in order to have an idea of the surface area. P6-L23. The most common term is probably “graupel” instead of “snow pellet”. P7-L3/18. The section 3.3 can be summarized in one paragraph because the setup of the sensitivity tests is given twice. Also, the first sentence of the section 4 explains that the CTL simulations will be compared to the available observations described in the previous section. P8-L5. .... also investigated “in the CTL simulations” P8-L10. Fig3e indicates a much shorter period of snow pellet precipitation P8-L11. Indicate on Fig 1. or on Fig 4 where is the cross section plotted on Figures 6, 9 and 11. P8-L15/16. Why the vertical movements would initiate only ice crystals and cloud droplets and not the other species? The amount of snow and snow pellet seem much

larger than the amount of ice crystals and cloud and rain water? What is the role of deposition? Fig.6. The intensity of the vertical wind is difficult to read. Also, the definition of the dashed/solid lines for wind is missing in the caption. The wet bulb temperature is plotted but not mentioned in the Section P11-L14. Why do you choose this threshold in order to plot the Fig8? Fig8 is difficult to interpret; you should make a difference between CTL and each sensitivity test. P12-L8. I do not understand the nucleation citation. You never work with the concentration parameter? P13-L8. “flow reversal”. Do you mean wind shear?

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