

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 #1

Received and published: 19 March 2019

General comments:

This manuscript investigates the role of sublimation and riming in orographic precipitation in the Kananaskis Valley based on a well-documented mixed precipitation event from a field campaign in the spring 2015. The authors analyzed the observed data and conducted a set of numerical simulations to isolate and quantify the impacts of these two important physical mechanisms in the precipitation process. Their major conclusions include 1) sublimation can have a greater impact than melting on the precipitation evolution under sub-saturated conditions in the lower atmosphere, 2) diabatic cooling due to sublimation or melting can result in change in the precipitation environment,

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allowing coupled interactions between orographic flow and precipitation, and 3) the orographic precipitation distribution cannot be simulated adequately if the thermodynamic impact of sublimation (and melting) is not represented correctly in the numerical models.

The data and techniques used in this study are clearly described, referenced, and easy to follow. The conclusions are well-supported and consistent with the stated objectives. This study represents an original and interesting contribution to our understanding of the thermodynamics and microphysics of precipitation in complex terrain. The manuscript is well-organized. But there are some language issues (grammatical and stylistic errors). Some figures need to be revised for clarity. Therefore my recommendation is to accept for publication after some minor revisions.

Specific comments and technical corrections:

The title should be either “Role . . . in . . .” or “Impact . . . on . . .”

P1, L9: Replace “where the field campaign took place during March-April 2015” with “during March-April 2015”. It has already been mentioned at the beginning that the field campaign took place during this period.

P1, L11: Remove the unnecessary comma after “2015”.

P2, L4: You may need to add “which is” before “associated with. . .”.

P2, L8: “the distance associated with complete melting of solid precipitation” may not be considered as a physical mechanism. Isn’t it just a factor?

P2, L12: Consider revise the sentence to “However, Zangl (2007) used numerical simulations to demonstrate (or suggest) that. . .”

P2, L13: I am not sure which event is the “same event”.

P2, L14: What do you mean “relatively warm temperature”? It would be better to specify it as “above-freezing temperature”.

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P2, L28: Consider change the sentence to “precipitation types over Baffin Island, Nunavut, were characterized by Henson et al. (2011) and Fargey et al. (2014)”. The study area of Fargey et al. (2014) was not restricted to Iqaluit.

P3: Caption of Fig. 1: Consider also defining those three-letter identifiers with the real location names in the caption.

P3, L7: Remove the comma after “including”.

P3, L11: Consider replace the second “during” with “in”.

P3, L12: Replace “Thériault et al. (2018)” with “(Thériault et al., 2018)”.

P3, L17: “GEONOR” should be defined and referenced here.

P4, Caption of Fig. 2: “CTL” for control should be defined somewhere in the text, and consider change the caption to: “Vertical profiles of air temperature (solid line) and dew point temperature (dashed line) at 2100 UTC 31 March 2015 at the KES site. The measurement and the CTL simulation are represented by blue lines and black lines, respectively.”

P4, L10: How do you define bright band in Fig. 3a? Please explain in the text of the figure caption.

P4, L13: Is this 200-m layer a “non-melting layer” or a “partially-melting layer”?

P5, L3, and P17, L10: “WRF” has been defined on P3. You don’t need to re-define it here.

P5, L4: Did you “conducted” the 3D simulations, or “used” the simulations conducted by others? The word “used” is confusing.

P6, Section 3.2: About the two-moment microphysics scheme, some recent studies (Morrison et al. 2015, Milbrandt et al. 2016) showed that there is a systematic bias in this scheme, which is linked to the fact that ice-phase particles are represented by

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pre-defined categories. Essentially, in situations with light riming, the scheme accounts for the mass growth of snow but not the increase in density and fall speed, unless the riming rate is sufficiently high that snow is converted to graupel, which has a higher terminal fall speed. Such configuration allows lighter hydrometeors to stay in the air too long before being converted to heavier hydrometeors. Could you comment to what extent this bias may affect the simulations in your study?

P7, L4: The acronym “CTL” should be defined earlier, i.e. when it first appears in the text.

P7, L6: Consider changing “latent heating/cooling due to the melting...” to “diabatic heating/cooling due to the precipitation transition”. Latent heating is due to the condensation, not from melting of snow.

P7, L20-25: Observations are poorly presented in Fig. 4. See a comment given later (P8, Fig. 4).

P7, L30: You can remove “(<5 knots)”. It is kind of confusing. Do you mean the simulated winds are less than 5 knots, or they are not stronger than observed winds for more than 5 knots?

P8, Fig. 4: What do the line contours represent? My guess is elevation. Please mention it in the figure caption. Also, it is hard to read the observations from the circles in (c). It would be better to plot them separately in (d). Or simply mention the observed amounts in the caption.

P11, L2: Change “role” to “roles”.

P11, L12: Change “is” to “are”.

P11, L15: Do you mean “is considered to produce a similar...”?

P12, L3: Change “suggests” to “suggest”.

P12, L6: Change “differs from the CTL simulation” to “differ from their counterparts in

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the CTL simulation”.

P12, L22: Either delete “studies”, or change “cases” to “case”.

P13, L6: Replace “changes” with “change”.

P17, L14: Do you mean “resulted in stronger upward” (rather than “weak”)?

P17, L17: Why are snow particles transported upward due to downslope flow?

P18, L7-10: Operational meteorologists in western Canada noticed that the High-Resolution Deterministic Prediction System (HRDPS) based on the MY2 microphysics scheme often has a warm bias in the valleys. You mentioned on Page 6 that in the MY2 scheme, snow sublimation can only occur when the temperature is below 0C. Based on your conclusion given here, do you think this sublimation restriction is partially responsible for the warm bias?

References:

Milbrandt, J. A., and H. Morrison, 2016: Parameterization of cloud microphysics based on the prediction of bulk ice particle properties. Part III: Introduction of multiple free categories. *J. Atmos. Sci.*, 73, 975–995, doi:10.1175/JAS-D-15-0204.1.

Morrison, H., and J. A. Milbrandt, 2015: Parameterization of cloud microphysics based on the prediction of bulk ice particle properties. Part I: Scheme description and idealized tests. *J. Atmos. Sci.*, 72, 287–311, doi:10.1175/JAS-D-14-0065.1.

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