Responses to Anonymous Referee #1

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, 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.
We thank Referee #1 for his/her suggestions and comments, which helped improved the manuscript. The manuscript was carefully reread to check for language issues. Comments are addressed point by point below.

Specific comments and technical corrections:
Comment 1: The title should be either "Role ... in ..." or "Impact ... on ..."
The title is now "Role of sublimation and riming in the precipitation …"

Comment 2: 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.
Correction was made.

Comment 3: P1, L11: Remove the unnecessary comma after "2015".
This was done.

Comment 4: P2, L4: You may need to add "which is" before "associated with. . .".
Correction was made.

Comment 5: 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?

We agree with the referee. The text was modified as: "These simulations identified two physical mechanisms influencing the location of the rain-snow boundary along the mountainside: cooling by melting of ice-phase particles and adiabatic cooling of rising air. The distance associated with complete melting of ice-phase precipitation was also an important factor."

Comment 6: P2, L12: Consider revise the sentence to "However, Zangl (2007) used numerical simulations to demonstrate (or suggest) that. . ."
The sentence was revised as: "However, Zängl (2007) used numerical simulations to demonstrate that the cooling…"

Comment 7: P2, L13: I am not sure which event is the "same event".
It is now mentioned as: "the same event as Steiner et al. (2003)…"

Comment 8: P2, L14: What do you mean "relatively warm temperature"? It would be better to specify it as "above-freezing temperature".
Yes, it has been replaced.

Comment 9: 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.
The sentence was changed to: "In contrast, Henson et al. (2011) and Fargey et al. (2014) characterized precipitation types over Baffin Island, Nunavut, showing that rimed particles, aggregates, and snow pellets were very common even during light precipitation events."

Comment 10: P3: Caption of Fig. 1: Consider also defining those three-letter identifiers with the real location names in the caption.
They are now defined. The new caption is "Figure 1. Area of interest (left) and 1km mesh domain (right) used for the numerical simulations with the WRF model. BAR stands for the Barrier Lake research station, NAK for Nakiska ski area, KES for the Kananaskis Emergency Services site and FOR for Fortress Mountain. Red line on the right panel indicates the position of the cross section used in Figs. 6, 9 and 11."

Comment 11: 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)".
These three corrections were made.

Comment 12: P3, L17: "GEONOR" should be defined and referenced here.
The new sentence reads as follows: "Instrumentation used included a GEONOR weighing precipitation gauge (Rasmussen et al., 2012), …".

Comment 13: 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.”

The caption of Fig. 2 was changed to “Vertical profiles of air temperature (T, solid line), dew point temperature (Td, dashed line) and wet-bulb temperature (Tw, light colour) at 2100 UTC 31 March 2015 at the KES site. The measurement (OBS) and the control simulation (model) described in section 3.1, are represented by blue and black/grey lines, respectively.”

Comment 14: P4, L10: How do you define bright band in Fig. 3a? Please explain in the text of the figure caption.

The bright band is now defined in the 2nd paragraph of the introduction. It reads as follows “… , which is associated with a maximum reflectivity value (> 30 dBZ) called the radar bright band (Fabry and Zawadzki, 1995).” Given that the sentence related to Fig. 3a reads as follows “The radar reflectivity bright band (>30 dBZ) is located at the elevation where ice-phase precipitation started to melt (Fig. 3a)”. The following sentence was also added to the figure caption. “Reflectivity values > 30 dBZ are associated with the radar reflectivity bright-band.”.

This is the revised Fig. 3.

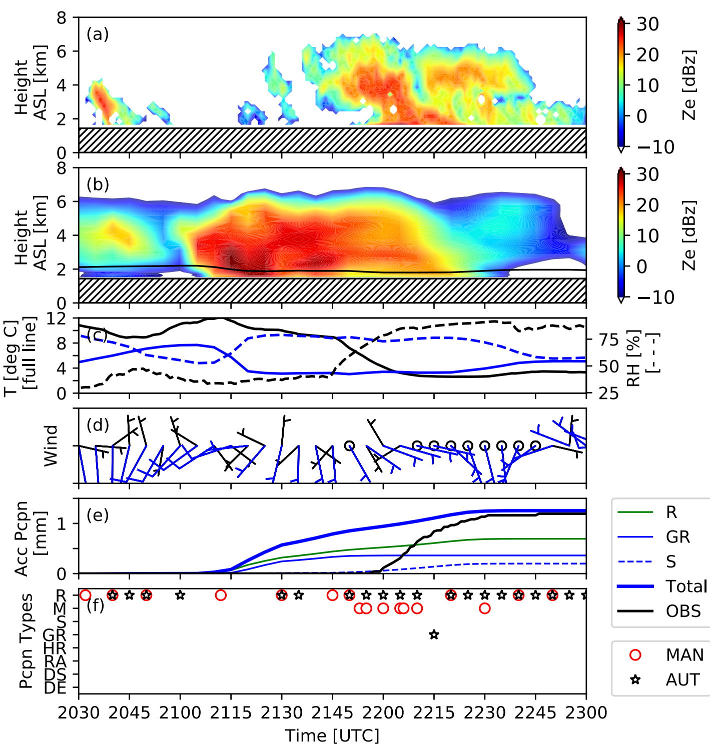

Figure 3 (revised): Atmospheric conditions and precipitation fields during the 31 March 2015 event at KES. (a) Reflectivity field measured by the Micro Rain Radar and (b) is estimated by the model (CTR). Reflectivity values > 30dBZ are associated with the radar reflectivity bright-band.;

(c) surface temperature (T) and relative humidity (RH) observed (black line) and simulated (blue line); (d) wind speed and direction using wind barbs, where the observed is black and simulated is blue. An empty circle is wind speed rounded at 0 knots, a short bar is rounded at 5 knots; (e) unadjusted liquid equivalent accumulated precipitation observed (black line, OBS) and simulated (bold blue line for total, green line for rain, thin blue line for graupel and dashed blue line for snow), and (f) the type of precipitation observed manual (MAN) and automatically (AUT) at KES. These are rain (R), graupel (GR), snow (S), mixed precipitation (M), heavily rimed snow (HR), rimed aggregates (RA), dry snow (DS) and dendrites (DE). Simulated results are for the CTL run. Adapted from Thériault et al. (2018).

Comment 15: P4, L13: Is this 200-m layer a "non-melting layer" or a "partially-melting layer"?
This layer was associated with only solid precipitation, so a 'non-melting layer'. The sentence was revised to "The rain-snow transition is located about 200 m below the 0°C isotherm, which confirms that solid precipitation was not melting until the level associated with a wet-bulb temperature, T_W, > 0°C was reached (Harder and Pomeroy, 2013). ".

Comment 16: P5, L3, and P17, L10: "WRF" has been defined on P3. You don't need to re-define it here.
Correction was made.

Comment 17: P5, L4: Did you "conducted" the 3D simulations, or "used" the simulations conducted by others? The word "used" is confusing.
We conducted the simulations. The first two sentences of section 3.1 were changed to "Three-dimensional (3D) simulations are performed using WRF model, version 3.7.1 (Skamarock and Klemp, 2008), with initial and boundary conditions provided by the North American Regional Reanalysis (NARR) data from the National Center for Environmental Prediction (NCEP) (Mesinger et al., 2006).".

Comment 18: 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 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?
Three comments were added to the manuscript to discuss this:
1) A comment about P3 was added at the end of section 7.1. It reads as follows. "… Finally, the Predicted Particle Properties (P3, Morrison and Milbrandt 2015 and Milbrandt and Morrison, 2016) allows smooth transitions in the riming degree, which produces a more realistic transition between snow, partially rimed snow and graupel."

2) A new paragraph was added at the end of section 7.1 (after the previous answer): "The parameterization of graupel formation and evolution could affect the amount and distribution of precipitation at the surface. This study shows that rimed-faster-falling particles and unrimed-slow-falling particles (snow) reaching KES will not be formed at the same location aloft and it depends strongly on the parameterization. For example, the CTR produces a small amount of snow at the surface. Given that the conversion to graupel occurred in certain conditions, snow remained aloft longer, which altered the graupel formation and its vertical evolution. This suggests that the amount of graupel may be underestimated. Even if this is the case, it would not change the physical processes highlighted in Fig. 12 about the sublimation of snow and graupel and the presence of graupel aloft. It can, however, alter the amount of the different types and timing of precipitation reaching the surface depending on the amount of snow conversion into graupel."

3) Another comment was also added to the conclusion (section 7.2): "Different microphysics schemes would produce different precipitation rates and thus affect the cooling rate associated with sublimation and melting. In a dry environment with temperatures near 0°C, if snowflakes do not sublimate it can overestimate the amount of precipitation produced in models, leading to warm biases. Furthermore, the rate of autoconversion from snow to graupel will also impact the distribution of precipitation aloft and, in turn, at the surface. This is particularly important in complex terrain as previously mentioned in Milbrandt et al. (2009). Using another cloud microphysics scheme, however, should not qualitatively modify results. Similar conclusions on involved physical processes in the distribution and types of hydrometeors at the surface would be obtained."

Comment 19: P7, L4: The acronym "CTL" should be defined earlier, i.e. when it first appears in the text.
The first occurrence in the text of CTR is now in section 3.1 where it is defined.

Comment 20: 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.
This section was updated and we used diabatic heating/cooling instead of latent heating/cooling.

Comment 21: P7, L20-25: Observations are poorly presented in Fig. 4. See a comment given later (P8, Fig. 4).
See reply to comment 23 below. The amount for each station was added to the caption.

Comment 22: 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?

It has been removed. The winds simulated are stronger during the events (~5 knots). It has been clarified in the text and in the figure caption.

Comment 23: 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.

It is now mentioned in the caption of Fig. 4: "Line contours represent the topography.".
We think that it is better to show a direct comparison between observed and simulated accumulated precipitation with circles in (c). Circles are now larger. The numbers are added in the caption as suggested. The numbers are KES (2.7 mm), Nakiska (2.2 mm), Fortress (3 mm) and Barrier Lake Station (0.8 mm). We hope that it is now clearer.
This is the revised Fig. 4.

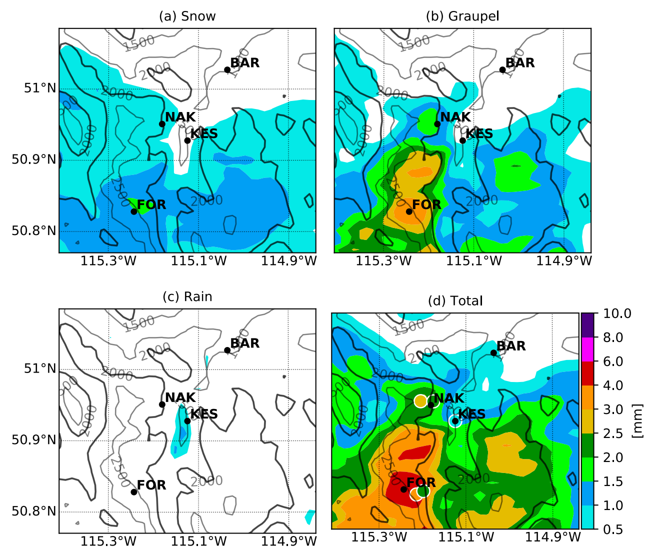

Figure 4 (revised): Simulated unadjusted accumulated solid precipitation (mm) including (a) snow and (b) graupel, (c) rain and (d) total accumulated precipitation between 2000 UTC 31 March 2015 and 0000 UTC 1 April 2015. The coloured circles in (d) are the observations at 4 locations. These are KES (2.7 mm), Nakiska (2.2 mm), Fortress (3.0 mm) and Barrier Lake Station (0.8 mm). Accumulated precipitation is in liquid equivalent. The black lines are the topography in meters.

Comment 24: P11, L2: Change "role" to "roles".

The correction was made.

Comment 25: P11, L12: Change "is" to "are".

It has been changed.

Comment 26: P11, L15: Do you mean "is considered to produce a similar. . ."?

The sentence was changed to "The distribution of hydrometeors at KES for NO_MLT is similar to the CTR with very little change in precipitation and cloud distribution (Fig. 8a and c, which are now in the revised Fig. 5 – 1st and 2nd column)."
This is the revised Fig. 5.

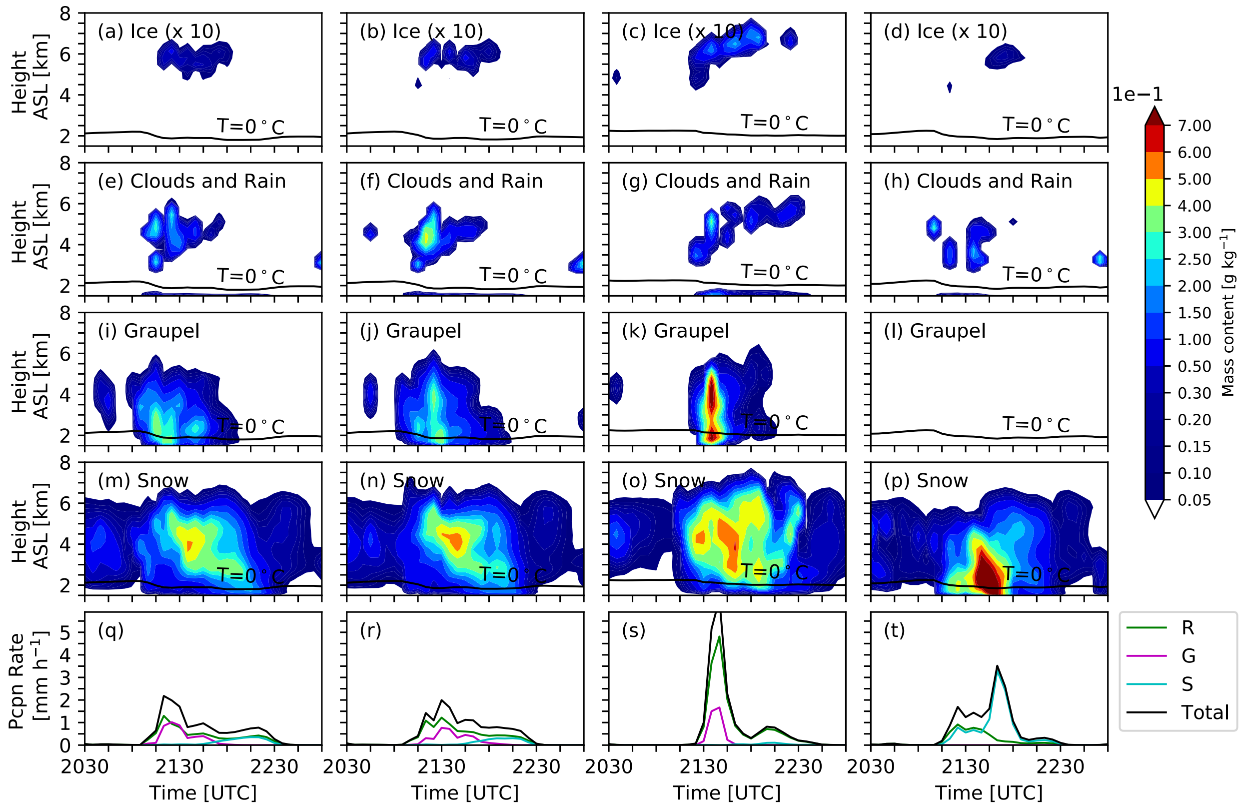

Figure 5 (revised): Comparison of the time evolution of hydrometeors at the surface and aloft at KES during the 4 simulations conducted for CTR, NO_MLT, NO_SBL and NO_GRPL from left to right. (a-d) is ice mass content (x10 g/kg), (e-h) is clouds and rain mass content, where rain is only formed through melting of ice, so it is only present near the surface, (i-l) is graupel mass content, (m-p) is snow mass content and (q-t) is the surface precipitation rate of rain (R), graupel (G) and snow (S). The 0°C isotherm is indicated by the solid black line on (a-p). Panels a-p have the same colour scale.

Comment 27: P12, L3: Change "suggests" to "suggest".
It has been changed.

Comment 28: P12, L6: Change "differs from the CTL simulation" to "differ from their counterparts in the CTL simulation".
It has been changed.

Comment 29: P12, L22: Either delete "studies", or change "cases" to "case".
The word 'studies' was deleted.

Comment 30: P13, L6: Replace "changes" with "change".
The correction was made.

Comment 31: P17, L14: Do you mean "resulted in stronger upward" (rather than "weak")?
It should be "stronger" instead of "weaker" (shown in Fig. 10). It is corrected in the text.

Comment 32: P17, L17: Why are snow particles transported upward due to downslope flow?
The sentences starting on P17, L16 to L18 were clarified as follows. "The snowflakes produced on the western barrier are being transported eastward by the wind. The down valley flow produced by the diabatic cooling from sublimation prevents the snow from reaching KES because it falls at around 1 m/s. The decrease in mass content is probably associated with a combination of the sublimation of snow and a change in its trajectory associated with the convergence of the flow field produced by the down valley flow near the valley floor and the westerly flow aloft."

Comment 33: 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?
This restriction could partly explain this warm bias of HRDPS observed in the valley because allowing snow sublimation at temperature above 0°C produces cold and dense air locally in the valley. A comment was added in the conclusion: "In a dry environment with temperatures near 0°C, if snowflakes do not sublimate it can overestimate the amount of precipitation produced in models that lead to warm biases."