Anonymous Referee #3

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 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.

We thank Referee #3 for his/her suggestions and comments, which helped improving the manuscript. Specific and minor comments are addressed point by point below.

<u>Specific comments:</u>

Comment 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.

More details have been added in section 3.2 and the description of the modifications made on the cloud microphysics scheme are now included. Moreover, it is now clearly stated that our study aims to identify qualitatively physical processes responsible for the types and distribution of the precipitation observed at the surface. In this context, we think that our main results and conclusions are not dependent on the specific cloud microphysics scheme used. This is confirmed by a test we made with the available Thompson et al. (2008) scheme showing similar results. See response to Referee #2, Specific comment #1.

Comment 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.

The mass content is now plotted on all the figures that include hydrometeor fields. Since the mass content is related to the diabatic heating/cooling, we believe they were not necessary here. As an example, we included Figs. 15 and 18 from Emilie Poirier's MSc thesis available online. These figures show the time series at KES of the heating/cooling associated with the CTR and

NO_GRPL runs.

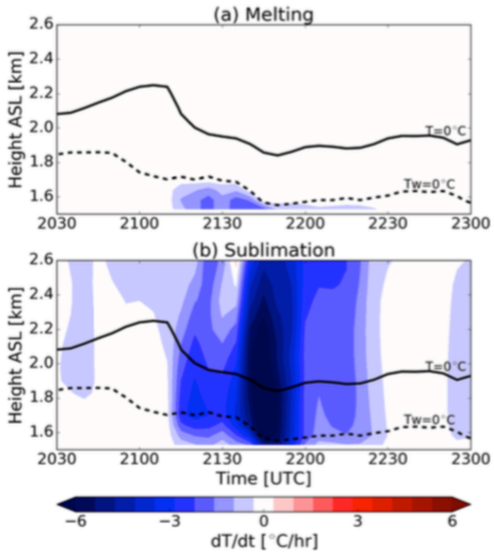

Figure 1.15 Cooling rate (dT/dt) associated with (a) melting and (b) sublimation of snow above the KES site on 31 March 2015 for the run assuming no graupel formation. The solid line indicates the height of the 0°C isotherm and the dashed line indicates the height where the wet-bulb temperature is 0°C.

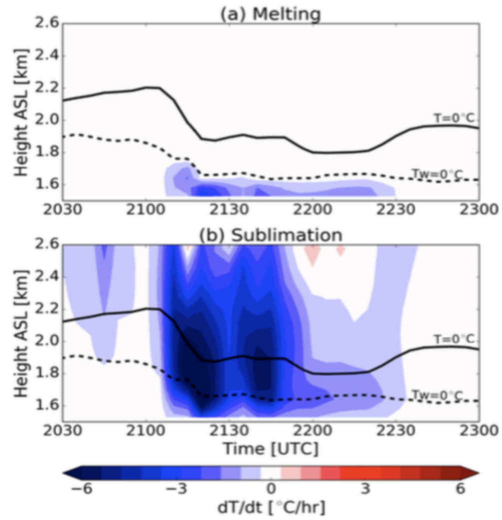

Figure 1.8 Cooling rate (dT/dt) associated with (a) melting and (b) sublimation of snow and graupel above the KES site on 31 March 2015 for the control run. The solid line indicates the height of the 0°C isotherm and the dashed line indicates the height where the wet-bulb temperature is 0°C. The red color indicates an area of heating due to vapor deposition.

These show that the cooling is on the same order of magnitude for both runs. We also see that the diabatic cooling from sublimation occurred at all temperature whereas diabatic cooling from melting occurred below the 0°C isotherm and is delimited by the 0°C wet bulb temperature isotherm. As mentioned previously, the mass content fields show similar behaviours.

Comment 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.

The main site was KES but "car-sonde" was performed along Fortress Mountain (FOR) during rain snow transition event. Detailed information about the instruments has been added to the 2 paragraph of section 2: "Most of the observations were collected at the Kananaskis Emergency Services (KES) site located a few kilometers southeast of the Nakiska ski area (NAK) and about 15 km south of the Barrier Lake research station (BAR) (Fig. 1). To characterize the atmospheric *conditions (temperature and relative humidity) aloft, sounding system was used and balloons were launched at every 3 h during precipitation events. The precipitation layer aloft was characterized using a Micro Rain Radar 2 (MRR2, Klugmann et al., 1996). It 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. Basic meteorological measurements were also available (pressure, wind speed and direction, temperature, dew point temperature). A GEONOR weighing precipitation gauge (Rasmussen et al., 2012) was used to measure the liquid equivalent amount of precipitation. An OTT Parsivel 2 (Battaglia et al., 2010) optical disdrometer was used to characterize the type of hydrometeor because it measures the fallspeed and diameter of precipitation particles. Manual observations of weather conditions including precipitation types were also reported in a systematic manner. In addition, precipitation types are automatically diagnosed using the Ishizaka et al. (2013) method also used in Thériault et al. (2018). The manual method is more precise because one can estimate the degree of riming and the exact crystal types. The Ishizaka et al. (2013) method gives a good idea of the degree of riming but it is not possible to diagnose the type of ice crystal because of the bin sizes. Vertical profiles of basic meteorological features were also obtained using a Kestrel attached to a ski pole and a GPS (Thériault et al., 2014) at two other sites to characterize rain-snow transitions at NAK and at Fortress Mountain (FOR). Further details about the field campaign are given in Thériault et al. (2018). "*

Klugmann, D., Heinsohn, K., and Kirtzel, H.: A low cost 24 GHz FM-CW Doppler radar rain profiler, Contr. Atmos. Phys., 69, 247–253, 1996.

Comment 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.

This comment was addressed in 2 steps:

1) *The modifications made to the scheme are described in section 3.2. These sentences have been added: "Given that graupel can sublimate at temperatures >0°C, the same equation was used for snow, which is*

$$QVD_{vs} = \frac{1}{AB_i}\left[\, 2\pi(S_i - 1)N_{0s}VENT_s - \frac{L_s L_f}{K_a R_v T^2} QCL_{cs} \,\right]$$

where

$$AB_i = \frac{L_s^2}{K_a R_v T^2} + \frac{1}{\rho q_{is} \psi}$$

is the thermodynamic function. Also, S_i is the saturation ratio with respect to ice, N_{0s} is the intercept parameter for snow, $VENT_s$ is the mass-weighted ventilation factor (Ferrier, 1994), K_a is the thermal conductivity of air, R_v is the gas constant for water vapor, T is the temperature of air, ρ is the density of air, q_{is} is the saturation vapor mixing ratio with respect to ice and ψ is the diffusivity of water vapor in air.

The sublimation rate equation was moved in the microphysics scheme so that snow and graupel sublimation are computed in the same conditions, which is at all air temperatures. The function polysvp was also corrected in the microphysics scheme to calculate the saturation vapor pressure properly at all temperatures. This bug was fixed in the following version of WRF"

2) *A description of the simulations conducted and the microphysical processes studies are described in the new section 3.3 called Description of the sensitivity experiment. This is the new section:*
   "The control simulation (CTR) is conducted using the modified microphysics and model configuration described in section 3.1 and 3.2. To estimate the impact of temperature changes while neglecting the diabatic heating/cooling due to the precipitation phase transition and no graupel formation, the three following sensitivity experiments were performed:
   a. *The diabatic cooling of melting snow and graupel was neglected. Hence, snow and graupel were allowed to melt into rain but no energy was extracted from the environment to melt the particles. This experiment is called NO_MLT.*
   b. *The diabatic cooling of sublimation was neglected. In a similar manner as for MLT, snow and graupel were allowed to sublimate but the temperature interaction with the environment was not taken into account. This experiment is called NO_SBL.*
   c. *Since that graupel was often reaching the surface at KES during the Alberta field project (Thériault et al., 2018), another simulation was performed. The initiation of graupel was suppressed to turn the production of graupel off. It was also ensured that there were no sources or sinks, hence, no warming from the cloud droplets freezing on the solid particles (snow or/and ice) and no sublimation of graupel since none was produced. This experiment is called NO_GRPL."*

Comment 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 Figure 2 but never mentioned in the manuscript.

A discussion was added to the 2nd paragraph of section 4.1. "Concerning the general meteorological parameters, the CTR run show similar patterns than the observations at KES (cf. Figs. 2 and 3). The vertical structure of the temperature and dewpoint are similar but the model is mainly colder and moister than the observations. The wet-bulb temperature is, however, similar (Fig 2.)". The figure was added to minor comment #7.

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?

The simulated reflectivity field was added to Fig. 3 and a comparison is done in the text between reflectivity fields from the MMR2 and the simulated one in section 4.1, which has been completely rewritten, see response to Referee 2 specific comment #3. However, we did not think that it was necessary to plot the Doppler velocity, but it would be interesting if the goal of the study would be to do a model comparison with observations.

This is the revised Fig. 3:

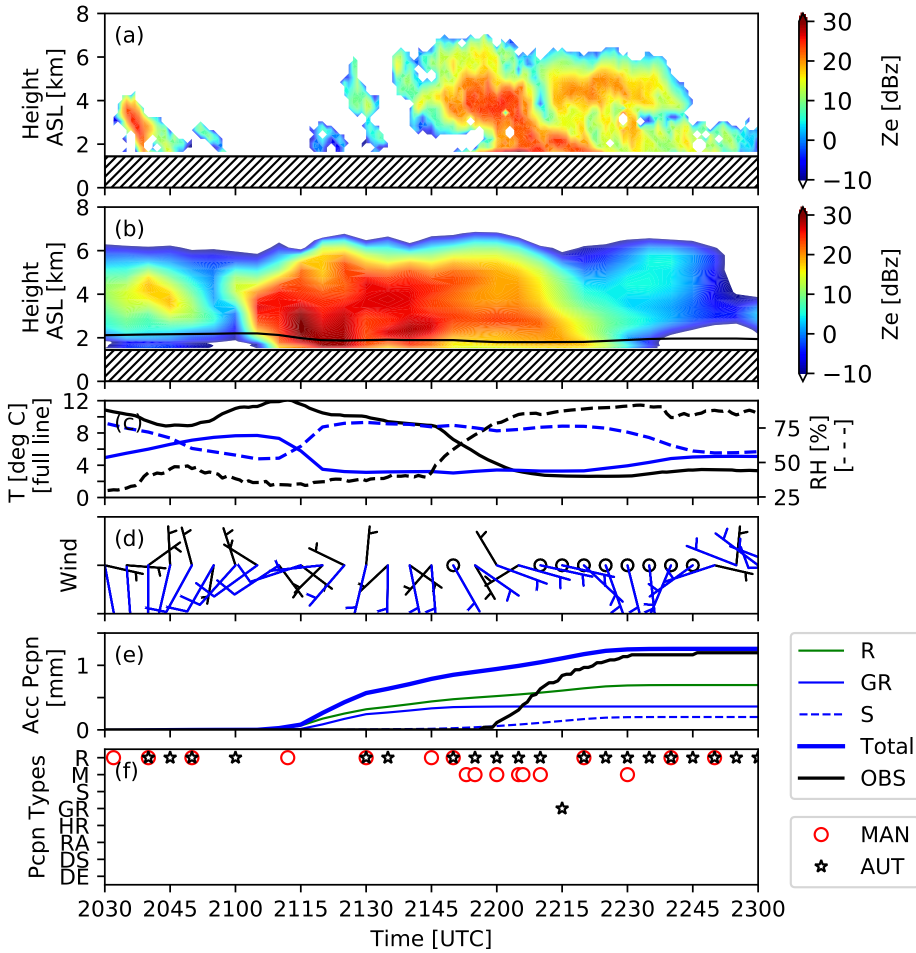

Figure 3 (revised): Atmospheric conditions and precipitation fields during the 31 March 2015 event at KES. (a) Reflectivity field measured by the Micro Rain Radar and (b) is estimated by the model (CTR). Reflectivity values > 30dBZ are associated with the radar reflectivity bright-band.; (c) surface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

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.

It is now explained that we want to demonstrate that simulated results compare well with observations in a qualitative point of view and not quantitative. First sentence in section 4 was modified as: "The CTR simulation is compared to observations to ensure that atmospheric conditions are sufficiently well represented by the model to ensure its use as a qualitative analysis tool of physical processes.". The time shift in the temporal evolution of the accumulated precipitation and temperature is now commented in section 4.1.

Do you estimate the impact on the results of this comparison between observations and CTL simulations if you choose another grid box?

We did try other grid point but the one used in this study compared better with observations.

Comment 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.

We prefer showing the full mass content fields instead of differences and new figures have been done for clarity (Figs. 5 and 6 as well as 11, whereas Figs. 7, 8, 9 are deleted) Hope that you will find our new figures easier to interpret.

Comment 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.

The manuscript was carefully reread to check for language issues.

<u>Minor comments :</u>

Comment 1: 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?

The "rain-snow boundary" in that sentence has been changed to "0°C isotherm" for clarity. Also, the definition of rain-snow transition was improved as follows: "The top of the boundary corresponds to the top of the melting layer aloft, which is represented by the radar bright band (Fabry and Zawadzki, 1995) and the base of the boundary is when all solid precipitation has melted into rain."

Comment 2: P2-L8. Sometimes the term solid precipitation is used. Is it ice-phase precipitation? Or precipitation with high density ≠ snow?

Solid precipitation was changed for ice-phase precipitation throughout the text.

Comment 3: P2-L12. “using numerical simulations” What type of simulations: 1D, 2D, 3D?

It is a 3D simulation, this is now indicated in the text.

Comment 4: P2-L23/28. These studies were performed over mountainous area?

This paragraph is to highlight the studies of precipitation in relatively dry conditions. The first sentence of that paragraph was changed to: “Few studies have examined precipitation features in northern Canada, in relatively dry areas.”.

Comment 5: P3-Fig1. The title of the right panel is not clear. Do you mean domain area?

Titles above panels were deleted (see the revised figure in reply of comment #18).

Comment 6: P3-L11. The name of the field campaign is only given in the conclusions section and should be mentioned here.

The first sentence now reads: “During the Alberta Field Project held in the Kananaskis Valley in March-April 2015, …”.

Comment 7: P4-Fig2. The authors may consider adding details on the skewT-logP diagram in order to define the Lifted Condensation Level and Tw.

Figure 2 has now been completed. This is the revised Fig. 2. The blue tone lines are the CTR and the black/grey tones are the observations.

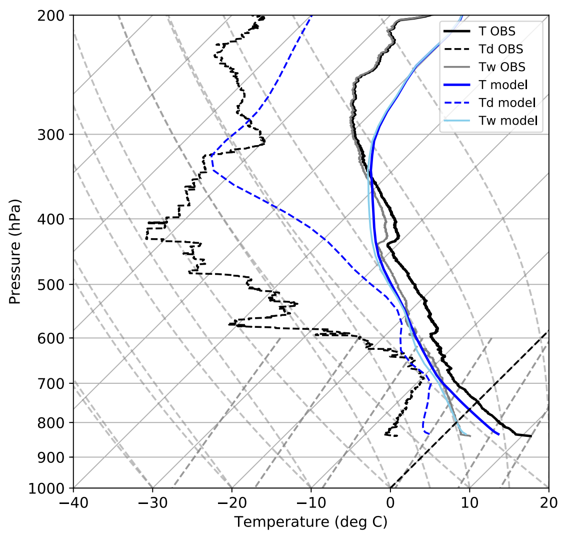

Figure 2. Vertical profiles of air temperature (T, solid line), dew point temperature (Td, dashed line) and wet-bulb temperature (Tw, light colour) at 2100 UTC 31 March 2015 at the KES site. The measurement (OBS) and the control simulation (model) described in section 3.1, are represented by blue and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

Comment 8: 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.
The relative humidity of measured and simulated has been added to Figure 3 instead of the dew point temperature (see response to specific comment #5).

Comment 9: P4-L10. The bright-band is close to the surface at the KES station?
The sentence was clarified: "The bright-band is located at the elevation where precipitation started to melt."

Comment 10: P5-L1. "brief period of only snow". According to Fig3e, there is no S period?
Snow has been changed to rain in the text.

Comment 11: 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.
Figure 3 has been improved and the legend has been completed. Also, simulated reflectivity has been added (see response to specific comment #5).

Comment 12: P6-L5. The boundary conditions forcing is every 3h, 6h or 12h?
This is now indicated as: "The boundary conditions forcing is done every 3 hours.".

Comment 13: P6-L7. Add the number of grid points of the innermost domain in order to have an idea of the surface area.
The number of grid points of the innermost domain has been added in section 3.1: "The high-resolution domain is shown in Fig. 1; it has 118 x 106 grid points.".

Comment 14: P6-L23. The most common term is probably "graupel" instead of "snow pellet".
It has been changed throughout the manuscript.

Comment 15: 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.
We chose to keep section 3.3 but to change the beginning of section 5 as: "The roles of phase changes and of the production of graupel on precipitation amounts and types reaching the surface at KES are investigated by comparing the CTR simulation with sensitivity experiments (NO_MLT; NO_SUB; NO_GRPL).". For section 4, it now begins as: "The CTR simulation is compared to observations to ensure that atmospheric conditions are sufficiently well represented by the model to ensure its use as a qualitative analysis tool of physical processes."

Comment 16: P8-L5. . . .. also investigated "in the CTL simulations"
Section 4 has been separated in two sub-sections for clarity.

Comment 17: P8-L10. Fig3e indicates a much shorter period of snow pellet precipitation
The manual observations reported some mixed precipitation, which could include graupel and/or snow mixed with rain. This is now better described in section 2 as "Manual observations at the KES site show that light rain started at 2030 UTC 31 March 2015, changing to a mixture of rain, snow and graupel between about 2150 UTC and 2215 UTC, then to a brief period of only rain (Fig. 3e).

Comment 18: P8-L11. Indicate on Fig 1. or on Fig 4 where is the cross section plotted on Figures 6, 9 and 11.
Figure 1 has been modified to indicate the position of the cross section (red line) plotted on Figs. 6 and 11 (Fig. 9 has been deleted).

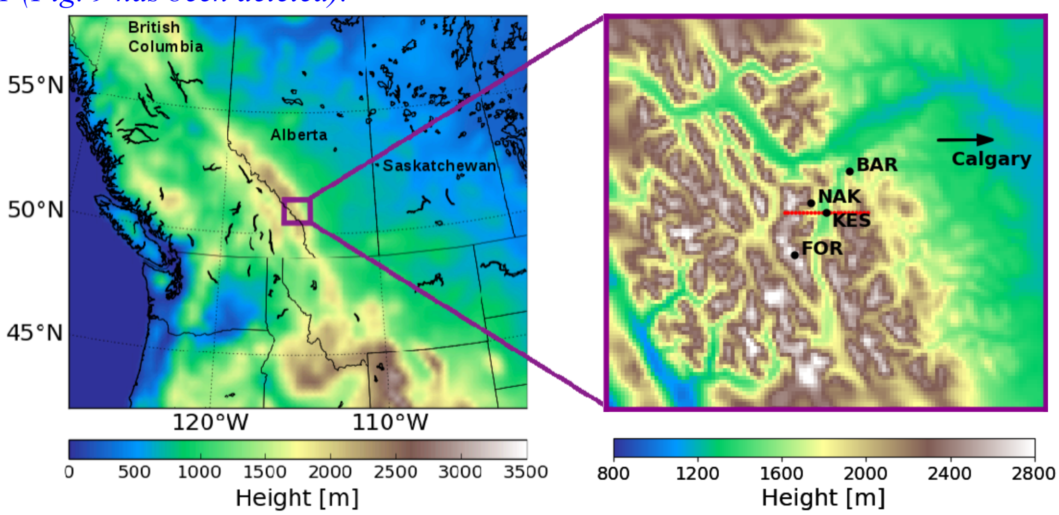

Figure 1. Area of interest (left) and 1km mesh domain (right) used for the numerical simulations with the WRF model. BAR stands for the Barrier Lake research station, NAK for Nakiska ski area, KES for the Kananaskis Emergency Services site and FOR for Fortress Mountain. Red line on the right panel indicates the position of the cross section used in Figs. 6, 9 and 11.

Comment 19: 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?
The vertical movement over the western barrier corresponds to air ascent, which favours heterogeneous nucleation of ice crystals and cloud droplets. It is the combination of ice crystals and cloud droplets, which initiate the snow formation. The water vapour depositional growth plays a role as this growth favours the formation of snow, which is more efficient for larger particles (either droplets or ice crystals). Clarifying Figure 6 is now helpful to answer these points (see response to comment #20).

Comment 20: 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

Figure 6 has been redone and the definition of dashed/solid lines for wind has been added in the caption. The temperature was plotted and not the wet-bulb temperature. It has been corrected in the new caption. This is the revised Fig. 6.

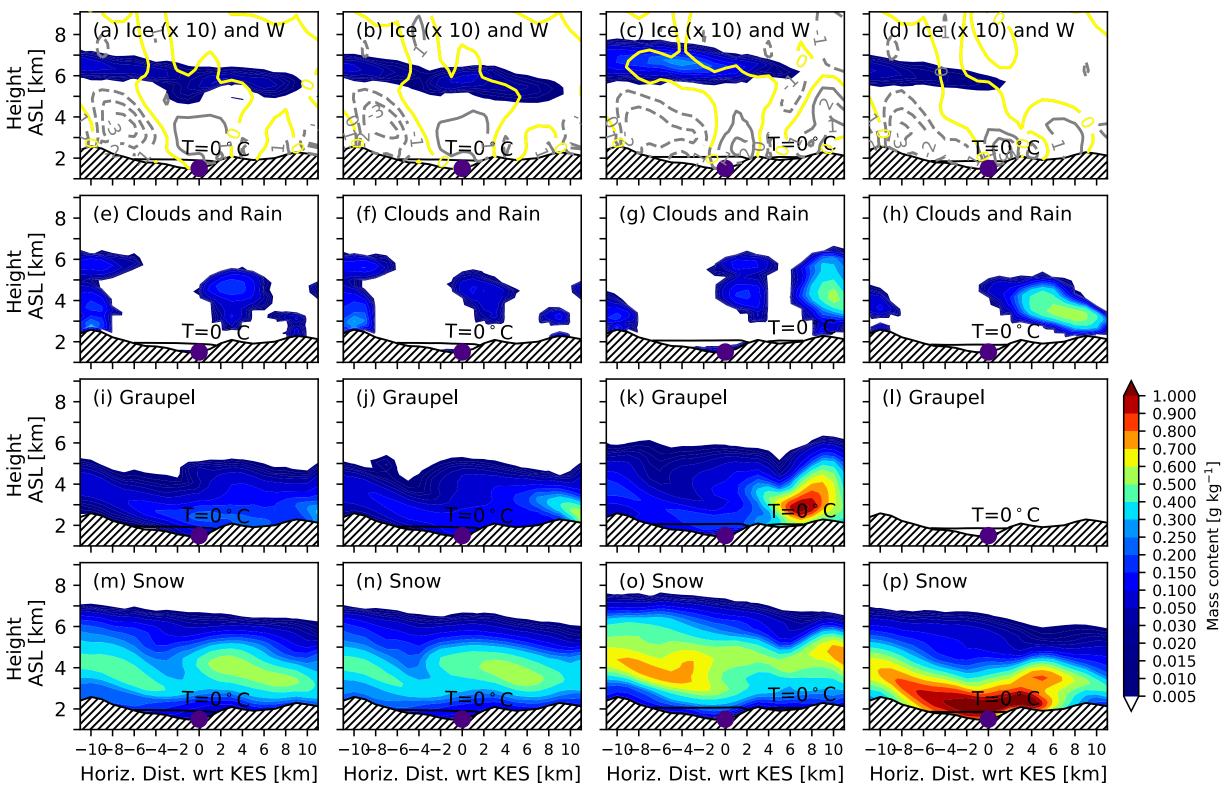

Figure 6: Comparison of the vertical cross-section across the Kananaskis Valley along the red line in Fig. 1 showing the mass content of hydrometeors during the 4 simulations conducted for CTR, NO_MLT, NO_SBL and NO_GRPL from left to right. (a-d) is ice mass content (x10 g/kg) with vertical velocity (m/s). The yellow line is 0 m/s, the dashed lines are negative values and solid lines are positive values, (e-h) is clouds and rain mass content, (i-l) is graupel mass content and (m-p) is snow mass content. The 0°C isotherm is indicated by the solid black line. Panels a-p have the same colour scale. The location of KES is indicated by the purple dot.

Comment 21: 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.

Figure 8 was deleted. The information is given in Fig. 5. This is the revised Fig. 5.

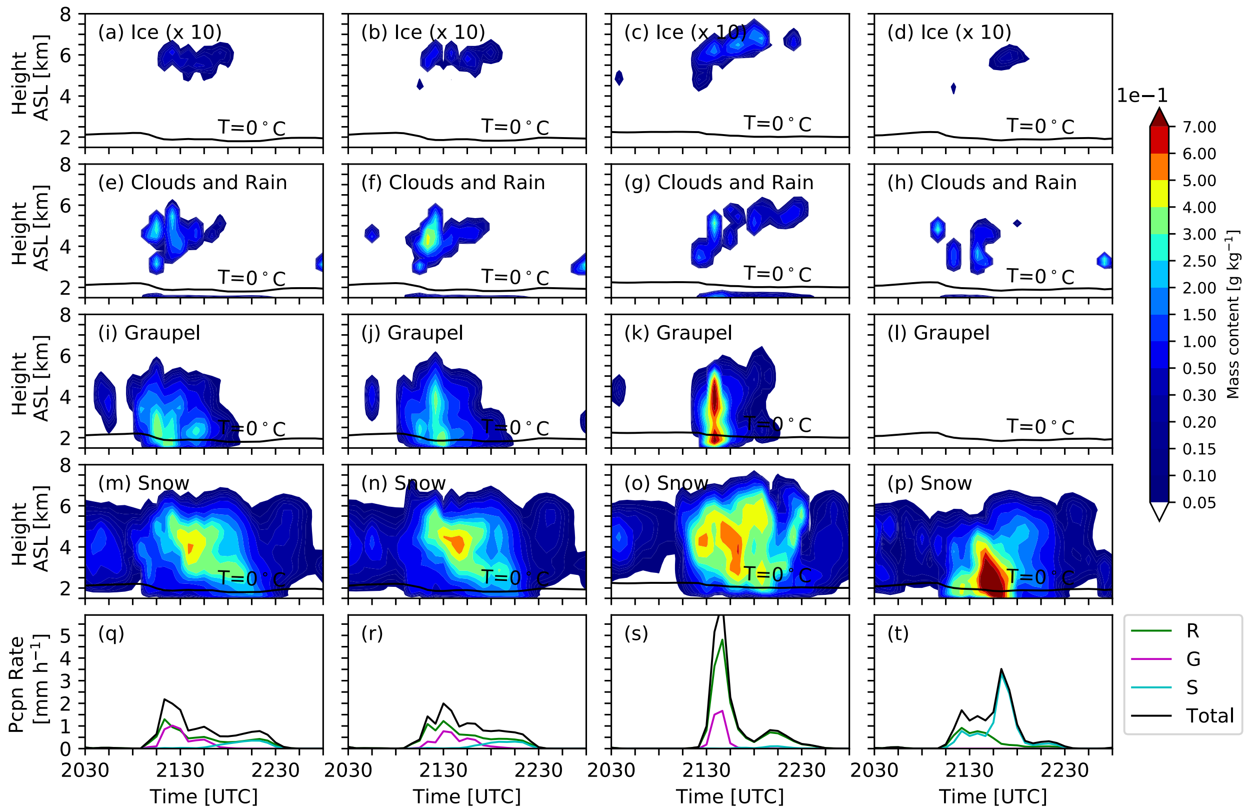

Figure 5 (revised): Comparison of the time evolution of hydrometeors at the surface and aloft at KES during the 4 simulations conducted for CTR, NO_MLT, NO_SBL and NO_GRPL from left to right. (a-d) is ice mass content (x10 g/kg), (e-h) is clouds and rain mass content, where rain is only formed through 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 22: P12-L8. I do not understand the nucleation citation. You never work with the concentration parameter?

The heterogeneous ice nucleation is parameterized using Meyers et al. (1992), which gives a number of pristine ice crystals formed depending on air temperature and ice supersaturation. Then, an assumed size of newly formed ice crystals allows computing the mass content of pristine ice formed.

Comment 23: P13-L8. “flow reversal”. Do you mean wind shear?

Yes, the term “flow reversal” was replaced by “wind shear” for clarity.