Response to Anonymous Referee #2

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 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.

We thank Referee #2 for his/her suggestions and comments, which helped improve the manuscript.

We agree with the referee that the aim of our study is to identify physical processes leading to the observed phase and distribution of precipitation at the surface in specific conditions i.e. dry sub-cloud layer and mountainous area. In this context, we think that, after demonstrating the ability of the model using the particular Milbrandt and Yau (2005a) MP scheme to simulate the observed case study, our approach using sensitivity experiments is valuable to identify these physical processes regardless of the MP scheme used. We hope that it is now clearer in the introduction of the paper as detailed below in the answer to specific comment #1.

Specific and minor comments are now addressed point by point below.

Specific comments:

Comment 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 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 represented in models, and in particular in the specific MP scheme used, should be included.

We addressed this comment in 3 steps.

1) *Clarifying the goal of the study. The goal of our study is to identify key physical processes that are associated with the distribution of precipitation types in the Kananaskis valley. We are confident that the physical processes identified in our study are not dependent to the MP scheme used as long as graupel is parameterized. We are mainly interested in studying the impact of the presence of graupel on the distribution of precipitation. The detailed microphysical mechanisms leading to graupel and how they compare in different schemes could be conducted in future work. We tried to put forward the use of the numerical simulations as an analysis tool for physical processes. We hope that it is now clearer.*
   a. *First, this is now clarified in the introduction with this added sentence at the end of 6th paragraph: "After verifying that the model is able to represent this observed case study, numerical simulations are used to investigate physical processes producing the distribution of precipitation in the Kananaskis area.".*
   b. *Second, in the summary part, the following statement has been added at the end of the section: "It is important to notice here that the CTR simulation was rerun with the Thompson et al. (2008) cloud microphysics scheme. This simulation also shows the presence of strong wind shear at KES towards the end of the event.* Less snow reached the surface at that time as well (not shown). *The results are consistent with our goal to use the model as an analysis tool of physical*

processes.". Note some minor modifications to the conceptual model (Fig. 12) were applied.

   c. *Third, the following sentence has been added in the conclusion in the paragraph about the limitations of our study: "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. However, as highlighted above in the summary, using another cloud microphysics scheme should not qualitatively modify results. Similar conclusions on involved physical processes in the distribution and types of hydrometeors at the surface would probably be obtained. Other atmospheric conditions should be further investigated. Relatively more saturated environment would lead to different results as, in a case of weak precipitation, a weaker vertical wind shear. In that case, solid particles do not sublimate and will melt. The diabatic cooling by melting would be reduced, which could allow particles to reach KES.".*

2) *A short <u>description of the graupel formation</u> was added to section 3.2. "The two-moment microphysics scheme predicts the mass mixing ratio and the total number concentration of inverse exponential size distribution of six hydrometeor categories: cloud droplets, rain, ice crystals, snow, graupel and hail. Each category is described by an assumed mass-diameter relationship and an associated fall speed. The evolution of clouds and precipitation is based on many microphysical processes that are mainly divided into cold and warm processes in the microphysics scheme. In this study we focus on the sublimation and melting of ice, snow and graupel as well as the impact of the presence of graupel. This last process includes the collision/coalescence of ice crystals and snow with cloud droplets or raindrops leading to rimed particles. This parameterization differs among bulk microphysics scheme. For example, Milbrandt and Yau (2005) follows Murakami (1990) to parameterize the conversion of snow-graupel. It is based on the rate of collection of snow/ice with cloud droplets as well as vapor deposition. The change from the snow category to graupel category involves a sharp increase in density (100 to 400 kg/m3) and, in turn an increase in the fall velocity (~1 to 3 m/s). Hence, the mass of snow can increase aloft without falling faster until it is converted into graupel. Pre-defined hydrometeor categories are a limitation of bulk microphysics schemes. A more detail description of the conversion process as well as all processes are given in Milbrandt and Yau (2005a, b)."*

3) *<u>Test with another microphysical scheme</u>. To show that the change in the vertical wind shear at KES, which impacts the distribution of precipitation at the surface, the CTR was run with the Thompson scheme (Thompson et al., 2008). For instance, the figure below is the same as Fig. 11 of the original manuscript with the CTR run with Thompson et al. (2008) (panels c and d). We see clearly that there is a strong vertical wind shear above KES. The strength is different than CTR because different amount of snow and graupel is*

produced but solid precipitation is still falling in sub-saturated conditions.

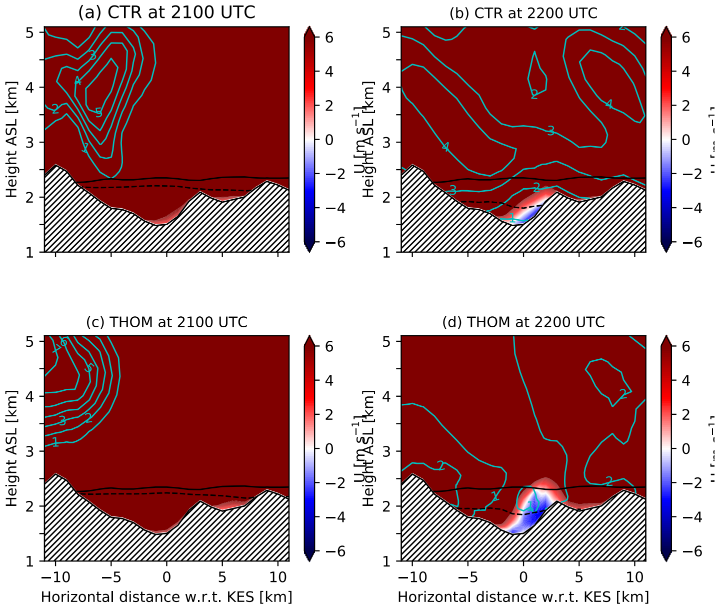


Figure A: The time evolution of the snow field (mass content, g/kg x 10) and the horizontal (east-west) wind field. The black line is the 0°C isotherm at the onset of the event and the dashed black line is at the time indicated on the panel. (a-b) is CTR and (c-d) is with THOM. This figure is only to illustrate the response to the referee and will not be added to the manuscript.

Comment 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).

Yes, the parameterization of graupel is very sensitive to the amount of snow converted into graupel. Some details on the processed studied (graupel formation, melting, sublimation) are now given in section 3.2 as follows. “The two-moment microphysics scheme predicts the mass mixing ratio and the total number concentration of inverse exponential size distribution of six hydrometeor categories: cloud droplets, rain, ice crystals, snow, graupel (i.e. graupel in this study), and hail. Each category is described by an assumed mass-diameter relationship and an associated fall speed. The evolution of clouds and precipitation is based on many microphysical processes that are mainly divided into cold and warm processes in the microphysics scheme. In this study we focus on the sublimation and melting of ice, snow and graupel as well as the impact of the presence of graupel. This last process includes the collision/coalescence of ice crystals and *snow with cloud droplets or raindrops leading to rimed particles. Milbrandt and Yau (2005) follows Murakami (1990) to parameterize the conversion snow-graupel. It is based on the rate of collection of snow/ice with cloud droplets as well as vapor deposition. The change from the snow category to graupel category involves a sharp increase in density (100 to 400 kg/m^3) and, in turn, an increase in the fall velocity (~1 to 3 m/s). The definition of specific hydrometeor categories is a limitation of bulk microphysics scheme. A more detail description of the conversion process as well as all processes is given in Milbrandt and Yau (2005a, b)."*

Also, a short discussion has been added at the end of section 7.1. It reads as follows: "the graupel and snow fields aloft are different as the production of graupel depends strongly on the parameterization of the conversion from snow to graupel and it is different in Thompson et al. (2008) and Milbrandt and Yau (2005). First, Thompson et al. (2008) follows Berry et al. (1974), and Milbrandt and Yau (2005) follows Murakami (1990). The latter depends on the collection and the vapor deposition. Second, note that the mass converted into graupel also depends on the assumed size distribution of snow, which is an inverse exponential in Milbrandt and Yau (2005) but is different in Thompson et al. (2008). Finally, the Predicted Particle Properties (P3, Morrison and Milbrandt 2015; Morrison et al., 2016) allows smooth transitions in the riming degree, which produces a more realistic transition between snow, partially rimed snow and graupel. "

Finally, a comment on the importance on the conversion snow to graupel in complex terrain is mentioned in the conclusion. It refers to Milbrandt et al. 2009 and Morrison et al. 2016, JAS.

Comment 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.
This part has been completely rewritten and separated into two sub-sections, one comparing CTR results with observations at KES and another one analyzing the vertical distribution of hydrometeors simulated by the CTR run at KES. The simulated reflectivity time series has been added to Fig. 3 as panel (b).

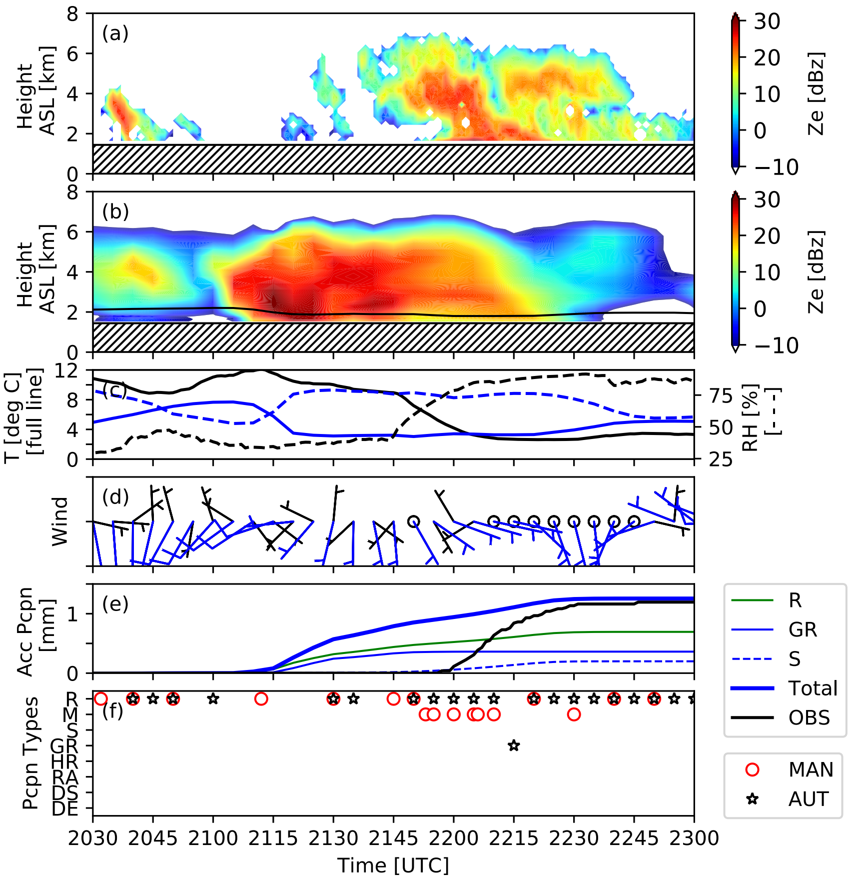


Figure 3 (revised): Atmospheric conditions and precipitation fields during the 31 March 2015 event at KES. (a) Reflectivity field measured by the Micro Rain Radar and (b) is estimated by the model (CTR). Reflectivity values > 30dBZ are associated with the radar reflectivity bright-band.; (c) surface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

This is the new section 4.1:

"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 simulated liquid equivalent accumulated precipitation is compared to observations in Fig. 4. Comparison shows good agreement at KES and NAK but an overestimation by the model near FOR (Fig. 4d). The gradient of precipitation along the mountainside is well represented, showing that rain accumulated in the valley (Fig. 4c). Higher amounts of graupel (4b) are produced at higher elevation where the conditions for riming are

more favorable because of the presence of cloud droplets (Milbrandt and Yau, 2005). Both observations and CTR simulation show that the precipitation amount accumulated at KES is relatively low during this event and dominated by rain (Fig. 4c) with snow at high elevations (Fig. 4b).

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 wetbulb temperature is, however, similar (Fig. 2). However, the timing of the precipitation differs. The simulated and observed relative humidity are similar, and even if temperatures are different before the onset of precipitation, they reach similar values during the precipitation event (cf. Fig. 3c). The wind direction is highly variable, but both the simulation and observation have southerly components before the onset of precipitation while the simulations exhibit slightly stronger winds during the event (cf. Fig. 3d). Ice-phase precipitation is simulated at temperatures >0°C in the Kananaskis area as reported during the field project. Precipitation amounts simulated at KES are very low and reach 1.3 mm during the simulated event in agreement with observations (Fig. 3e).

The rain-snow boundary occurred at warmer temperatures than if the environmental conditions were saturated, and is reproduced between 2100 and 2230 UTC 31 March 2015, as measured by the car-sonde at FOR (Thériault et al., 2018), which varied from 1750 m and 1830 m. The simulated height of the melting layer at about 1600 m (Fig. 5a-d) corresponds to that measured by the MRR2 bright band (Fig. 3a). The reflectivity computed is higher than observations and it is difficult to discern the bright band near the surface because of the high reflectivity fields probably produced by graupel (Fig. 3b). The comparison of Fig. 3e and 3f shows an agreement between the type of hydrometeors simulated and observed, with the predominance of rain and the presence of graupel. We notice that precipitation begins earlier in the simulation than in the observation (almost 1 hour) as shown in Fig. 3e and by the time lag between Fig. 3a and 3b.

In summary, the meteorology at KES is generally qualitatively well simulated during the precipitation event. This statement allows us to use the model to investigate the impact of microphysical processes on the phase and distribution of precipitation at the surface."

This is the revised Fig. 2.

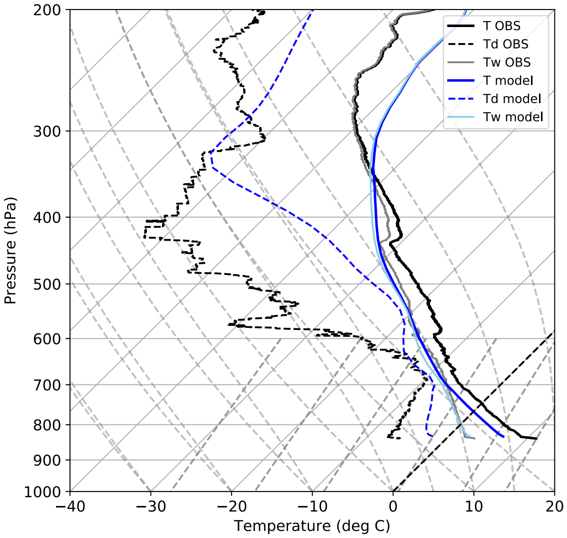


Figure 2. Vertical profiles of air temperature (T, solid line), dew point temperature (Td, dashed line) and wet-bulb temperature (Tw, light colour) at 2100 UTC 31 March 2015 at the KES site. The measurement (OBS) and the control simulation (model) described in section 3.1, are represented by blue and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

Comment 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.

Snow and graupel have been separated in a new version of Figure 4. Also, NO_SUB and NO_GRPL have been compared with the control run in the figures (Figures B and C) below. Note that there is not necessarily a lateral shift in precipitation, because the accumulated precipitation is similar in all cases. These are 1.64 mm CTR, 2.18 mm for NO_MLT, 1.39 mm for NO_SBL and 1.45 mm for NO_GRPL. There is, however, a change in the timing of the precipitation intensity depending on the sensitivity experiment. It has been clarified in the text at the end of section 6. "Note that no lateral shift of the precipitation has been observed between the simulations because the accumulated precipitation is comparable among the runs, but the timing is different."

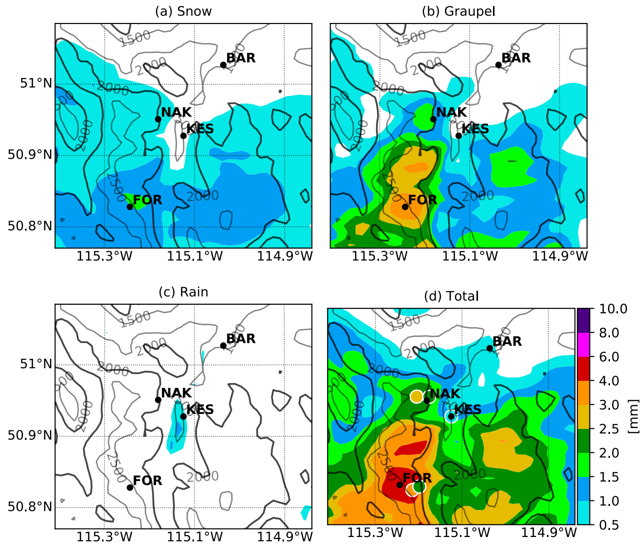


Figure 4 (revised): Simulated unadjusted accumulated solid precipitation (mm) including (a) snow and (b) graupel, (c) rain and (d) total accumulated precipitation between 2000 UTC 31 March 2015 and 0000 UTC 1 April 2015. The coloured circles in (d) are the observations at 4 locations. These are KES (2.7 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.

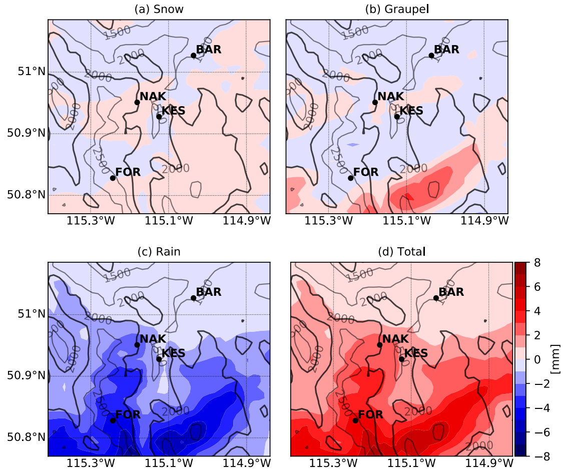


Figure B: The simulations without the temperature change from sublimation (NO_SBL) compared with CTR (NO_SBL-CTR).

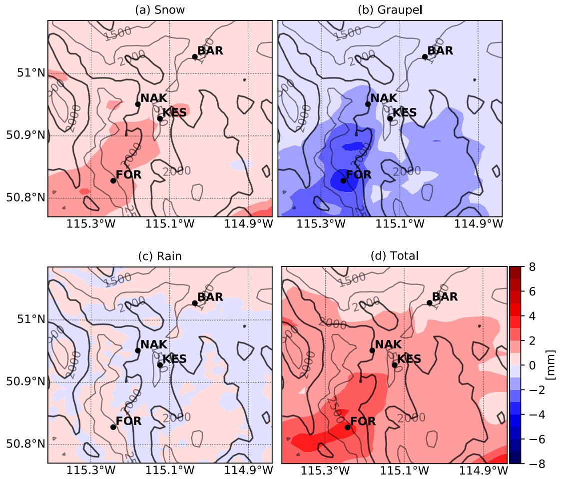


Figure C: The simulations without the temperature change from sublimation (NO_GRPL) compared with CTR (NO_GRPL-CTR).

Comment 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.

The figures have been redone mainly as suggested. The main changes are:

1) *Figure 5 is a 18-panel figure showing all 4 experiments. With this revised Figs. 5 and 6, we deleted Figs. 7, 8 and 9 but kept Figs. 10 and 11.*

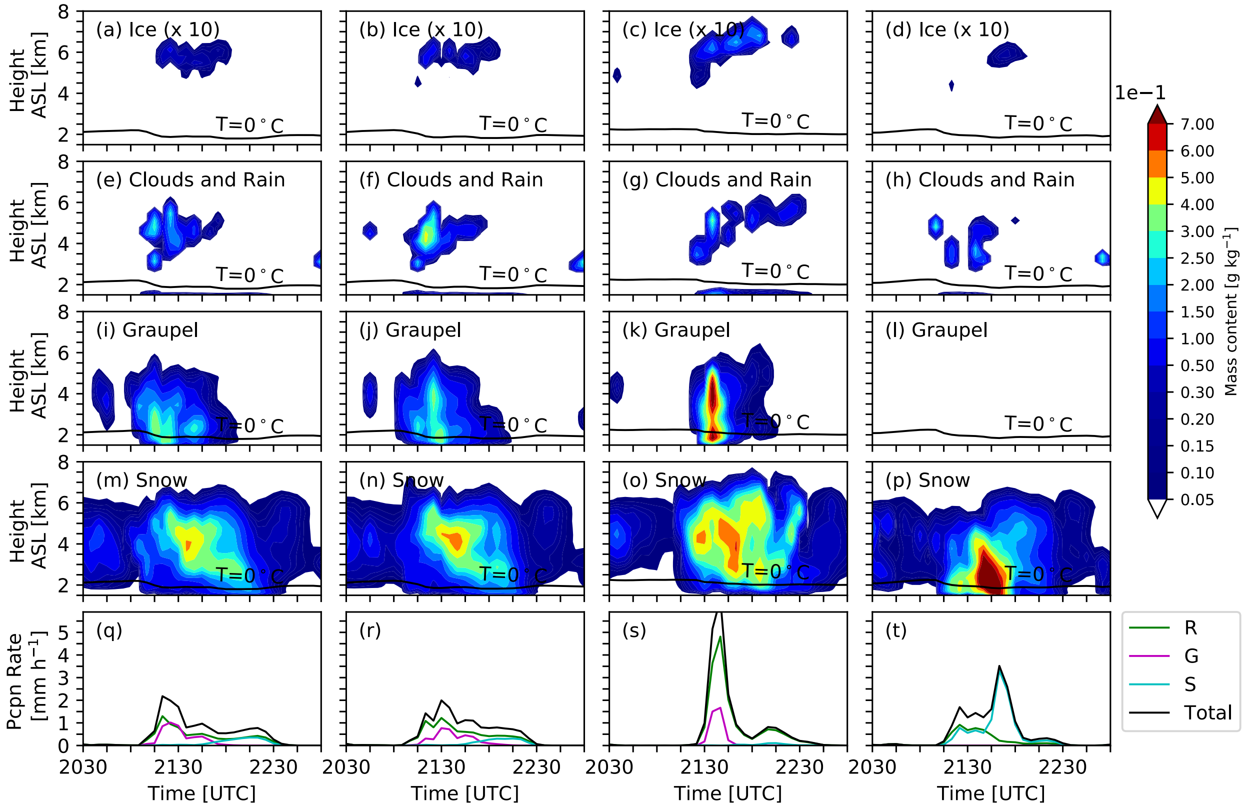


Figure 5 (revised): Comparison of the time evolution of hydrometeors at the surface and aloft at KES during the 4 simulations conducted for CTR, NO_MLT, NO_SBL and NO_GRPL from left to right. (a-d) is ice mass content (x10 g/kg), (e-h) is clouds and rain mass content, where rain is only formed through 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.

2) *Figure 6 is similar as Fig. 5 but for the vertical cross-section.*

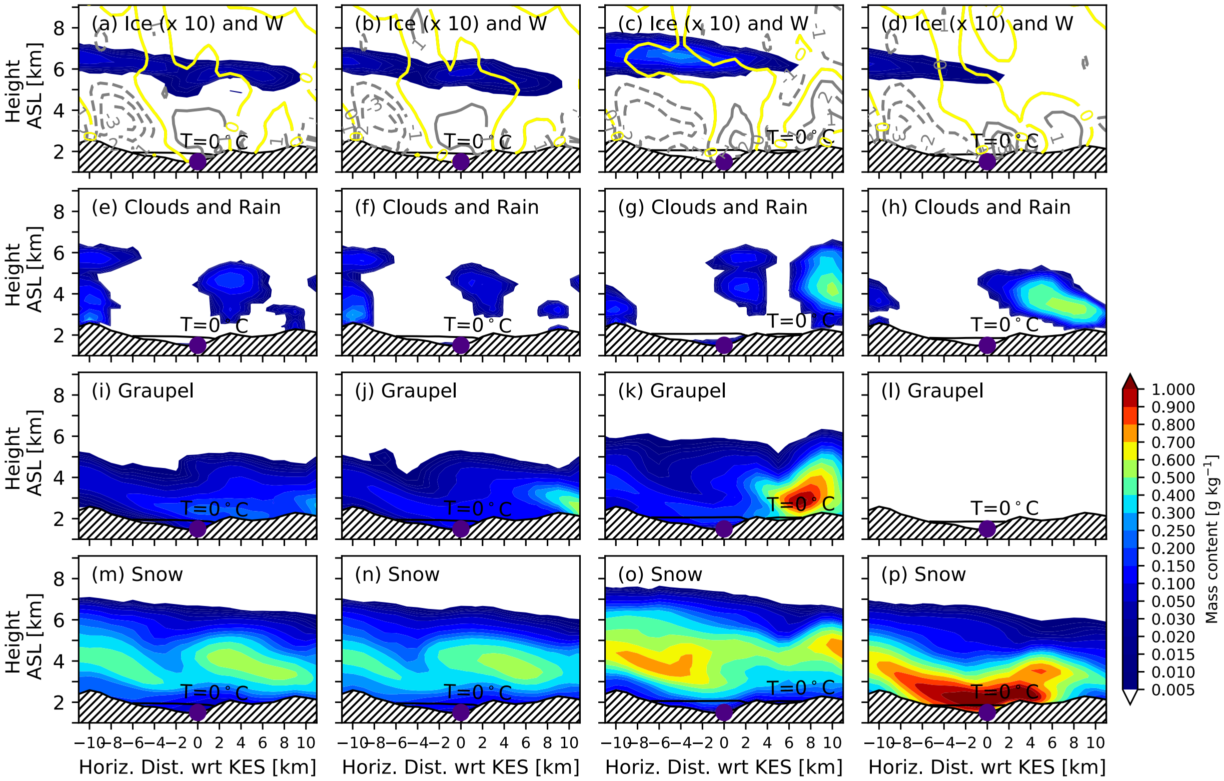


Figure 6 (revised): Comparison of the vertical cross-section across the Kananaskis Valley along the red line in Fig. 1 showing the mass content of hydrometeors during the 4 simulations conducted for CTR, NO_MLT, NO_SBL and NO_GRPL from left to right. (a-d) is ice mass content (x10 g/kg) with vertical 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.

3) *In this case we deleted Figs. 7-9 but kept Fig. 10 (plotted mass content instead of mass mixing ratio).*
4) *Clouds and rain were kept on the same panel because rain is only located below the 0°C isotherm and the clouds are aloft. There is no rain aloft in those experiments. A comment was added to the text.*
5) *The temperature fields were not added to the new Figs. 5 and 6 because it was too busy so we decided to keep Fig. 10. However, so you can see the temperature fields in detail, we added a 4-panel figure only in the responses:*

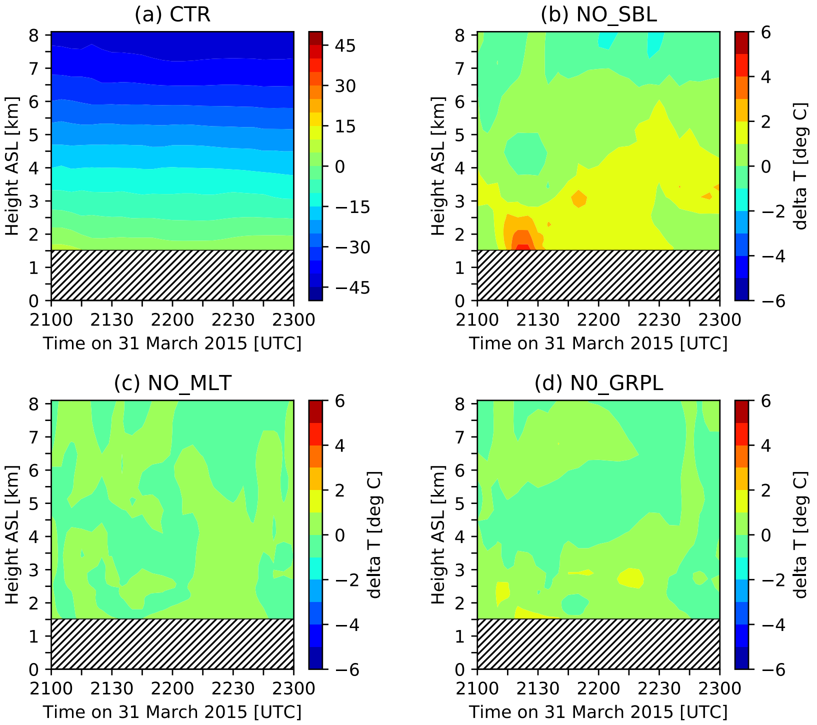


Figure D: Shows the (a) temperature fields time-series at KES for the CTR, (b) NO_SBL-CTR, (c) NO_MLT-CTR and (d) NO_GRPL-CTR. We see most of the difference between NO_SBL-CTR, which is the reason why we kept Fig. 10 in the manuscript.

Minor Comments:

Comment 1: 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).

The term "snow pellets" has been changed to graupel throughout the text.

Comment 2: 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?)

The modifications made on the original Milbrandt and Yau (2005a) scheme are now detailed in the paper. The bug has been corrected into more recent versions of WRF. Section 2 was updated to: "Given that graupel can sublimate at temperatures >0°C, the same equation was used for snow, which is

$$QVD_{vs}=\frac{1}{AB_i}\left[\;2\pi(S_i-1)N_{0s}VENT_s-\frac{L_sL_f}{K_aR_vT^2}QCL_{cs}\;\right]$$

where

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

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 vapour, T is the temperature of air, ρ is the density of air, q_{is} is the saturation vapour mixing ratio with respect to ice and ψ is the diffusivity of water vapour in air.
The sublimation rate equation was moved in the microphysics scheme so that snow and graupel sublimation are computed in the same conditions, at all air temperatures. The function polysvp was also corrected in the microphysics scheme to calculate the saturation vapour pressure properly at all temperatures. This bug was fixed in the following version of WRF."

Comment 3: p. 7/ln 8, "accreted particles". Unclear. I assume this means "rimed crystals". Degree of riming? Partially rimed or bona fide graupel?
The term "accreted particles" has been changed to "rimed particles".

Comment 4: 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.
Since we only have 3 experiments; we described them in bullet points in section 3.3. The name of the runs have been changed throughout the text as suggested. The details on how the graupel was shut off have been added as "The control simulation (CTR) is conducted using the modified microphysics and model configuration described in section 3.1 5 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 temperature tendency equation is

$$\frac{dT}{dt}=\frac{1}{\Delta t}\left\{\begin{array}{l}\dfrac{L_f}{c_{pd}}\left(\begin{array}{c}\Delta QCLcs+\Delta QCLcg+\Delta QCLch+\Delta QCLri+\Delta QCLrs\\+\Delta QCLrg+\Delta QCLrh+\Delta QFZci+\Delta QFZrh\\-\Delta QMLir-\Delta QMLsr-\Delta QMLgr-\Delta QMLhr\end{array}\right)\\+\dfrac{L_s}{c_{pd}}\left(\begin{array}{c}\Delta QNUvi+\Delta QVDvi+\Delta QVDvs+\Delta QVDvg\\+\Delta QVDvh\end{array}\right)\end{array}\right.$$

where L_f is the latent heat of fusion, L_s is the latent heat of sublimation, c_{pd} is the specific heat of dry air and Q is for mixing ratio. The types of mixing ratios are noted by CL for collection, FZ for freezing, ML for melting, NU for nucleation, VD for diffusional growth (positive) or *sublimation (negative) and the subscripts (c, r, i, s, g, h, v) represent cloud droplets, rain, ice, snow, graupel, hail and water vapour.*

The three key following sensitivity experiments were performed:

1. *NO_MLT: The diabatic cooling of melting snow (QMLsr) and graupel (QMSLgr) were set to zero in the temperature tendency equation. Hence, snow and graupel were allowed to melt into rain but no energy was extracted from the environment to melt the particles.*
2. *NO_SBL: The diabatic cooling of sublimation of snow (QVDvs) and graupel (QVDvg) was set to zero in the temperature tendency equation.*
3. *NO_GRPL: Since 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 by turning the production of graupel off (grpl _ON = false) . 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."*

A new section, section 3.4 named "Data analysis" has been added and the second paragraph of section 3.3 has been moved. Section 3.4 is as follows:
"The data are analyzed in a systematic manner. First, the CTR simulation is compared to available observations such as wind speed and direction, temperature, relative humidity, height of the rain-snow transition as well as precipitation amount and types collected during the field project. The time evolution of mass content of ice crystals, cloud, rain, snow pellets and snow are analyzed at the grid point closest to the KES site. To analyse precipitation aloft at KES and across the Kananaskis Valley, a vertical cross section is plotted and the mass content of hydrometeors as well as the vertical air motion are investigated. Second, the CTR simulation is compared to the three sensitivity experiments: the simulation without the temperature change from melting of snow and snow pellets (NO_MLT), the simulation without the temperature change from sublimation (NO_SBL) and the simulation without graupel (NO_GRPL). Finally, the impact of wind direction and precipitation types formed aloft on the precipitation amounts and types reaching the surface is investigated."

Comment 5: 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.
A panel showing the simulated radar reflectivity has been added as suggested in Fig. 3 (see specific comment #3). Indeed, figures showing simulated results are for the 1-km domain. This is now stated in section 3.1 as "The following analysis of simulated results will focus only on outputs from the high-resolution domain.".

Comment 6: 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.

As suggested, accumulated snow and graupel are now presented separately on Fig. 4a and b (see specific comment #4).

Comment 7: 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.)

The reduced size plots make them look less choppy. Hope that it is satisfactory.

Comment 8: 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.

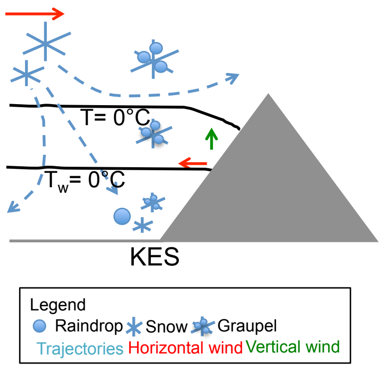
*The referee is right: the simulated hail mixing ratios are zero in all the simulations. A comment was added to section 4.2: "*Note that no hail was produced in the simulations."

Comment 9: 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.

The sentence has been rephrased for clarity as: "Less ice crystal and cloud water mass-mixing ratios are produced aloft compared to CTR. This could be explained by the lack of warming from accretion resulting in colder temperature, which leads to less water vapour depositional growth for ice crystals and cloud droplets and less ice nucleation aloft (e.g. Meyers et al., 1992)."

Comment 10: 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)

Yes, T_wet is associated with melting of ice. We tried plotting it on the revised Fig. 6 (comment #5), which replaced Fig. 8, but it is too close from the surface and, therefore, hard to see. We decided to plot T=0°C instead. It is, however, added to Fig. 12 (see below) since that the schematic does not extend as high vertically.



Comment 11: p. 12, lines 24-26. Suggest omitting paragraph or relocate this as an intro to section 6.

This paragraph has been moved to the beginning of section 6 and rewritten as: "This section will assess the role of sublimation and snow pellet formation on the vertical and horizontal evolution of precipitation intensity and types in the Kananaskis Valley."