Reviewer 1 Responses

The study simulates a frontal rain band over the UK for which extensive radar and in-situ observations exist. Two new secondary production parameterizations are introduced to the COSMO model. The modeling is extensive in that it covers 16 sensitivities (+1 control) with 10 perturbations per configuration. These data are then used to compare to observations and assess the impact of the new parameterizations on ice number concentration and precipitation.

I think the study will be publishable after the following comments are addressed.

Thank you for your thorough reading and suggestions to improve it.

1/ My main issue is the experimental design (or my interpretation of it). To me, the problem is that the study appears to mix one secondary production process into the control model and then looks at the impact of the two new processes and modifications to the rime splintering process as 'the effect of secondary ice production...'. From what I can see in Table 2 there is no sensitivity with all secondary ice processes off. At the same time, the control model is stated as using Seifert and Beheng microphysics. The Seifert and Beheng described in the literature included rime splintering ice production. There are some comments later in the paper that suggest to me that the control does have rime splintering off. Therefore, either I have misinterpreted and some additional description of the control model configuration is needed, or I think we need an additional set of control runs that have no secondary ice production processes included.

Until there is a clean control with no secondary ice production processes it is difficult to interpret the statements about the impact of secondary ice production.

While we had not included the control run (CTRL) in the original version of Table 2, it was indeed done without any secondary ice production processes active; the default rime splintering of the Seifert and Beheng scheme was turned off. Thank you for pointing out that this was not explicit. In Section 3, we have rewritten: *A control simulation [is also run] in which all secondary ice production processes, including the default rime splintering in SB06, are turned off (denoted `CTRL' throughout).* We have also addressed some of the discussion of the CTRL versus non-control simulations below.

2/ The thrust of the paper is to show the impact of the different secondary ice production processes. Given the potential of introducing many unknown parameters into the model I think that it would be really useful for the community to try and identify if certain processes can be ignored. At the moment, the paper is pushing us to try to represent more complexity, but it would be advantageous if simplifications could be identified.

For instance, question that arise as I read through the paper include:

i. What is the relative impact of rime splintering to droplet shattering to collisional breakup? (Something similar was done for primary versus secondary)
ii. Can we ignore any of them or do they interact?
iii. Given the number of unknowns is it possible to write a single parameterization that captures all of the processes with less parameters? (this one would be speculation)

To try and answer i) and ii) the following families of model runs are suggested – some of which you already have.

a) Control: no secondary ice production (CTRL)
b) Control + rime splintering (RS1, RS2)
c) Control + rime splintering + droplet shattering (RS2+DS2)

d) Control + rime splintering + droplet shattering + collisional breakup (ALL)
e) Control + droplet shattering (DS1, DS2)
f) Control + droplet shattering + collisional breakup (DS2+BR2ig)
g) Control + collisional breakup (BR1ig, BR2ig, BR2sg)
h) Control + collisional breakup + rime splintering (RS2+BR2ig)

At both your and the other reviewer's suggestion, we have adjusted the study format. Some of the parameters used for rime splintering or droplet shattering were overly generous and results were not shown from several of the simulations in the first version of Table 2. We had also done no simulations with a single process, but rather with rime splintering *and* either drop shattering *or* breakup because part of our intent was to see feedbacks between these processes. In particular, we had been hoping to confirm or deny the existence of a kind of cascade effect (as proposed by Lawson et al. 2015) in which a few ice crystals formed from an initial droplet shatter or ice-ice collision then kick start rime splintering. To address the relative importance of the processes (your questions i and ii) for various thermodynamic regimes (your question iii in part and noted by the other reviewer), we have reorganized the simulations to be the following (summarized in a new Figure 2 and denoted above):

Rime splintering

RS1:	$\aleph_{RS} = 300,$	$w_{RS} = \mathrm{TR}$
RS2:	$\aleph_{RS} = 300,$	$w_{RS} = \mathrm{UNI}$

Droplet shattering

DS1:	$\aleph_{DS} = 2,$	$p_{max} = 5\%,$	$\sigma = 3$
DS2:	$\aleph_{DS} = 10,$	$p_{max} = 10\%,$	$\sigma = 5$

Control

CTRL:	$\aleph_{*} = 0$	$F_{BR} = 0$

Ice-ice collisional breakup

BR1ig	*graupel_breakup_ice*		
	$F_{BR} = 180,$	$T_{min} = 256,$	$\gamma = 3$
BR2ig	*graupel_breakup_ice*		
	$F_{BR} = 360,$	$T_{min} = 249,$	$\gamma = 5$
BR2sg	*graupel_breakup_snow*		
	$F_{BR} = 360,$	$T_{min} = 249,$	$\gamma = 5$

Combinations

RS2 + BR2ig:	$\aleph_{RS} = 300,$	$w_{RS} = \mathrm{UNI}$	
	graupel_breakup_ice		
	$F_{BR} = 360,$	$T_{min} = 249,$	$\gamma = 5$
DS2 + BR2ig:	$\aleph_{DS} = 10,$	$p_{max} = 10\%,$	$\sigma = 5$
	graupel_breakup_ice		
	$F_{BR} = 360,$	$T_{min} = 249,$	$\gamma = 5$
RS2 + DS2:	$\aleph_{RS} = 300,$	$w_{RS} = \mathrm{UNI}$	
	$\aleph_{DS} = 10,$	$p_{max} = 10\%,$	$\sigma = 5$
ALL:	$\aleph_{RS} = 300,$	$w_{RS} = \mathrm{UNI}$	
	$F_{BR} = 360,$	$T_{min} = 249,$	$\gamma = 5$
	*graupel_breakup_**		
	$\aleph_{DS} = 10,$	$p_{max} = 10\%,$	$\sigma = 7$

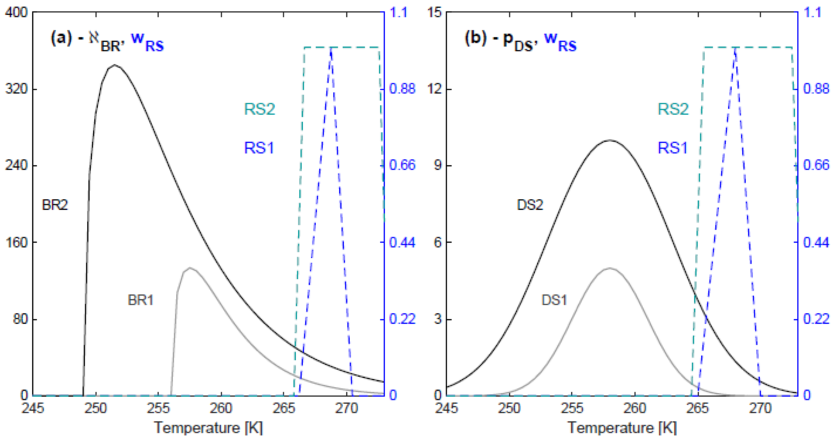


Figure 2 Fragment numbers, weightings, and probabilities from the secondary ice production parameterizations. In panel a, we show N_{BR} from both ice-ice collisional breakup simulations (BR1 and BR2) as well as the triangular and uniform w_{RS} (T). In panel b, we show p_{DS} from both droplet shattering simulations (DS1 and DS2) and w_{RS} once again.

To address your questions about relative importance and interaction, we have added a Figure 6 which compares the contributions from rime splintering, droplet shattering, collisional breakup, and all in combination in a 2 x 2 panel, as the old Figure 4 did for the primarily and secondarily formed ice crystal numbers.

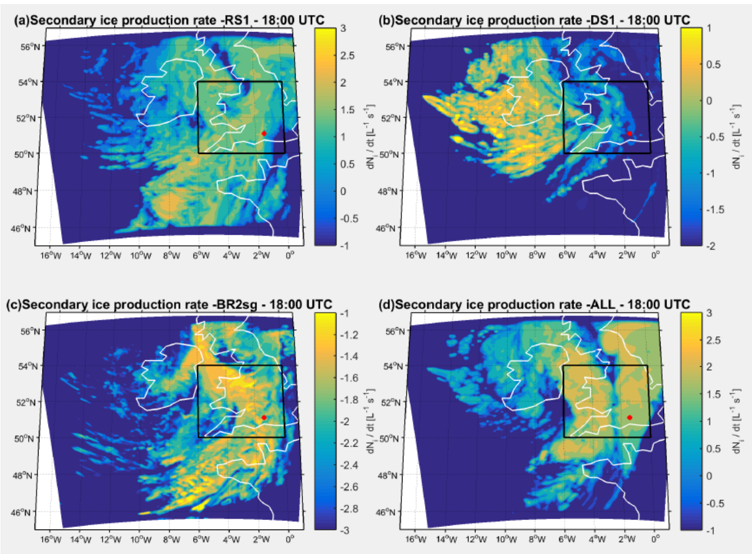


Figure 6 Maps to compare $N_{i,sec}$ between 18:00 UTC and 18:30 UTC from (a) rime splintering (RS1 at 3 km) (b) frozen drop shattering (DS1 at 4.5 km) (c) collisional breakup between snow and graupel (BR2sg at 1.5 km) and (d) all secondary processes occurring simultaneously (ALL at 4.5 km). Note the different logarithmic colorbars for each panel.

We have also added discussion of this to Section 4.2:

Next, we consider the relative ice crystal number concentrations produced by different processes in Figure 6. The largest $N_{i,sec}$ magnitudes, up to 1000 L^{-1} over the half hour, come from the RS1 and ALL simulations. These are followed by about 10 L^{-1} (half hour)$^{-1}$ generation rates from frozen droplet shattering and 0.1 L^{-1} (half hour)$^{-1}$ from collision breakup of snow and graupel. There is also an altitudinal hierarchy. Contributions from droplet shattering are largest at the highest altitudes of 4.5 km where raindrop number concentrations are still relatively high and the temperature (T in [237 K, 262 K] with a median of 249 K) is cold enough for non-negligible shattering probability. The rime splintering contribution is next at an altitude of 3 km, and the breakup is largest at a lower altitude of

1.5 km because the graupel mixing ratio is highest here. If graupel were present at higher altitudes, $N_{i,sec}$ from breakup could increase significantly, as both the snow mixing ratio and fragment number parameter increase at colder temperatures.

Finally, we have included fields of graupel and snow mixing ratios and rain drop number concentrations in the supplemental information, as well as a new section on dynamical intercomparisons – of wind speed, updrafts, and radar reflectivites – to make the discussion more thorough.

Additional points:

p. 5 eqn. 7. qrim is not defined. Is it the rate of change of ice due to riming?
q_{rim} is the rime mixing ratio. We add this to the description proceeding equation 7.

p. 6 table 2. SY not defined (typo on p. 5?)
We have eliminated the simulations that employed this symmetric temperature weighting for rime splintering (SY was for symmetric.)

p. 6, 13. CTRL = Seifert and Beheng – this has rime splintering secondary production as standard? [see main comment above]
You are right that in the default set-up Seifert and Beheng includes rime splintering. But we turn it off for our CTRL simulation and have made this clearer throughout the rewrite.

p. 8, 20. Maybe change secondarily -> secondary, primarily -> primary
We would prefer to leave the terminology as "secondarily-produced ICNC" / "primarily-nucleated ICNC" or "ICNC from secondary production" / "ICNC from primary nucleation".

p. 8, 28-29. Is COSMO able to capture this sort of mixing process?
We went ahead and removed this comment, as there is not extensive discussion of how COSMO represents vertical mixing in the model description.

p. 8, 30-p. 9, 3. 1Ag and 1Ac contain 2 changes. It's difficult to say which change is most important. Including 1An would provide a way to decide the relative importance of the changes.
Given the relatively large amount of observational evidence that suggests that the fragment number per milligram of rime should be on the order of 10^8, we have limited the rime splintering simulations to two with the standard triangular temperature weighting and a slightly extended one. So we will not discuss different fragment numbers.

p. 10, 7. 'There are no heating...' – do you mean because the structures are outside of the traditional temperature range for rime splintering?
We have moved this paragraph to the discussion section. The idea was that because rime splintering is a mechanical process, simply the shedding of fragile protuberances from rime, there is no latent heat release or consumption when it occurs. We have clarified: "*As mechanical processes, rime splintering and collisional breakup do not have direct latent heating effects.*"

p. 10, 8-10. 'Zhu et al...' I don't know if this can be inferred. I would imagine the melting differences affect the strength of downdrafts and cold pools leading to changes in subsequent convection which would be substantially different to the dynamical coupling due to latent heating in updrafts.
Yes, thank you. A more relevant study is that of Willison et al. 2013 *The importance of resolving mesoscale latent heating in the North Atlantic Storm Track* in which they discuss the latent heating effects on mid-latitude cyclogenesis and their spatial resolution dependence. We write:
Additional latent heating aloft can intensify the upper-level anticyclonic potential vorticity (PV) [Willison et al. 2013]. This PV generation may suppress further cyclogenesis by disconnecting the

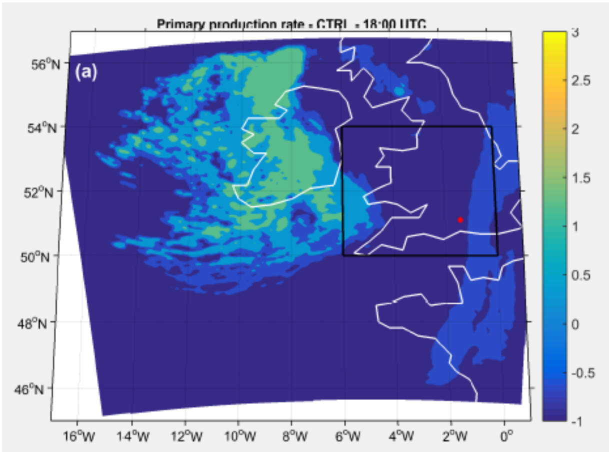
developing system from surface potential temperature anomalies. On the other hand, additional cyclonic diabatic PV may slow system progression and maintain favorable levels of shear.

p. 10, 13-14. '...smaller droplets form, diminishing the riming...'. I think making smaller droplets could lead to increase cloud liquid water that would lead to increased riming.

Yes, this is true, but we were thinking of a case of fixed cloud LWC. In this case, more and smaller droplets should decrease the collisional efficiency between these and ice hydrometeors. We have made the fixed cloud LWC assumption explicit.

p.10, 16. Ni,sec – does this include all rime-splintering too? How does it compare to Ni,pri in a control model with no secondary ice production?

In the updated figure, $N_{i,sec}$ is indeed the secondary ice produced from rime splintering. Instead of showing its ratio relative to the primarily nucleated ice $N_{i,pri}$ we have chosen to show the absolute $N_{i,pri}$. This removes some numerical concerns (where one or the other tendency was absent and division created unrealistic values). Although we do not include it in the updated manuscript, here is an $N_{i,pri}$ field from the CTRL simulation:



It has similar structure but somewhat smaller magnitude than the same field shown for the RS1 and RS2 fields in Figure 5.

p. 10, 27. This is a large domain that includes some of the warm front too?

Yes, this is true. It is the subdomain shown in black boxes in the new Figures 5 and 6, so an area that should also contain some of the warm front. We mention this in reference to some of the underestimation: *A final contribution to these too low ICNCs may be inclusion of parts of the warm front in the subdomain of analysis.*

p. 10, 30. '...filtered out'. These are in-cloud values then. Is it same thresholding for the observations?

The measurement accuracy does not extend to this low level, so we did not filter the observations in the same manner.

p. 11, 2. Control simulation – does this include rime splintering?

No, there is no secondary ice production in the control simulation.

p. 11, figure 4. How many points go into the 10^{-3} and lower probability for the observations? If it less than ~10 it might be good to ignore them?

We would prefer to keep the low-probability tail in Figure 7b to indicate the high degree of skewedness in the distribution. These instances, even if very few, are the ones most likely to reflect secondary ice production since N_{ice} is so high.

p. 12, 26. 'The control simulation without secondary ice...'. Is this the additional secondary ice processes or all secondary ice including rime splintering? If it is the latter then that needs to be made clear that the control has no secondary ice processes in it (see main point, p. 11,2, p. 6,13).

There is no secondary ice production in the control simulation. To remove confusion we delete "without secondary ice" here. In Section 3 on the Simulations, we write out: *"a control simulation in which all secondary ice production processes are turned off, including the default rime splintering in SB06 (denoted CTRL throughout)."*

p. 12, 32. How does figure 5 compare to observed accumulations?

The UK NIMROD radar data only offers rainfall rates. If we look instead at the CFARR ground site measurements of precipitation rate (Crosier et al. 2014 Figure 4b) we can very roughly integrate it. Say from 18:00 to 20:30 UTC there is a rate of 1.5 mm h^{-1} (= 4.5 mm) and another 1 mm h^{-1} from 21:00 to 22:00 (= 1 mm). If the rainband passage happens over a half hour with 60 mm h^{-1} intensity then we have a total accumulation of 4.5 + 1 + (0.5)(60) = 35.5 mm in the regions that saw the maximum precipitation intensity. This is not far off from the simulated values.

p. 13, 3. Please could you also give the domain mean precipitation change?

Yes, this is a good idea, thank you. We have written "*The sum of the deviations over the whole subdomain is an additional 23.9 m of precipitation for the RS1 simulation, 25.6 m for RS2, 16.9 m for DS1, and 16.6 m for ALL.*"

p. 13, 8. The red (positive) regions are also correlated with a combination of the location of the front and the orography, where convection may be enhanced.

Indeed. In reference to the location of the front, we write that *"Banding [in the P_{tot} deviations] reflects convective structure: vertical motion is strongest in the rainband leading edge, but also preceded and proceeded by downdrafts."* We have shown some of this dynamical structure in the new Figure 3c.

We had not considered the effect of orography; thank you for this suggestion. It could be that the particularly high P_{tot} seen in Figure 8a around 50.5°N and 4°W is due to orographic lifting by Dartmoor. We add the following description of topography to Section 4.3:

Orography in this region also has an impact. The spot of particularly large P_{tot} around 50.5°N and 4°W corresponds to the Dartmoor with a maximum elevation of 621 m. Slightly elevated P_{tot} is also present over the Exmoor and Bodmin Moor at (51°N, 3.5°W) and (50.5°N, 4.5°W).

p. 14, 4. The structures in Crosier et al are on the scale of ~5 km, whereas these are much bigger ~50 km?

Yes, you are right. A direct comparison of banded structure in the differential reflectivity and accumulated precipitation fields does not make total sense, so we have removed this particular comment. In general, however, we see "broadening behavior" in the simulations relative to the observations. For example, when we compare observed versus simulated radar reflectivity, we see that Z_{DH} values have a too-low magnitude over a too-great extent:

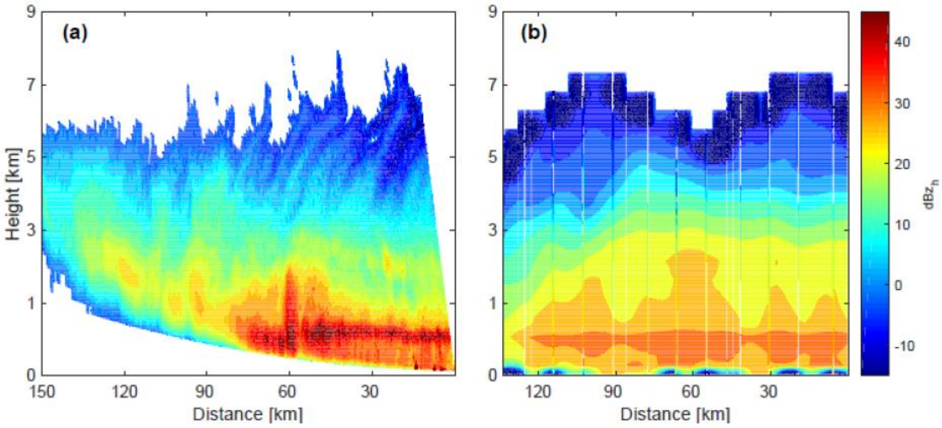


Figure 4 Model-measurement intercomparison of range-height indicator scans of radar reflectivity Z_{DH} along the 255 degree radial out from CFARR. CAMRa Doppler radar measurements are shown in panel a for the scan taken between 19:22:07 and 19:23:07 UTC, and modelare shown from the CTRL simulation at 19:00:00 UTC, both in dBZ_h.

Or in a qualitatively similar manner, we see too-low precipitation intensity magnitudes over too-great a time period in Figure 9.

p. 14, 9-11. 'suggesting that rime splintering is responsible for much of the change in P_{tot}.' Does this mean that the control did not have rime-splintering?

Yes, there is no secondary ice production in the control simulation. In Section 3 on the Simulations, we write out: *"a control simulation in which all secondary ice production processes are turned off, including the default rime splintering in SB06 (denoted CTRL throughout)."*

p. 14, 14. NCRF not defined?

Yes, thank you for pointing this out. We have added *narrow cold frontal rainbands (NCFRs).*

p. 14, 34. The variation in the means is now within 10% of the mean of the ensemble of results.

The differences in precipitation intensity vary less between simulations and deviate less from the CTRL simulation than those in precipitation accumulation. You are right that the difference in precipitation intensity from one simulation to another is not statistically significant. To the discussion in Section 4.3, we make explicit that "*In neither case does the evolution of the mean precipitation intensity for different simulations vary significantly from one to the next."*

p. 15, 1-6. Why not use the model to provide the diagnosed rates?

Yes, this is a good point. We opted to drop this analysis since it had significant numerical noise. Instead we have replaced these with temporal evolutions of the N_{ice} profile over time (in part to investigate the impact of seeding):

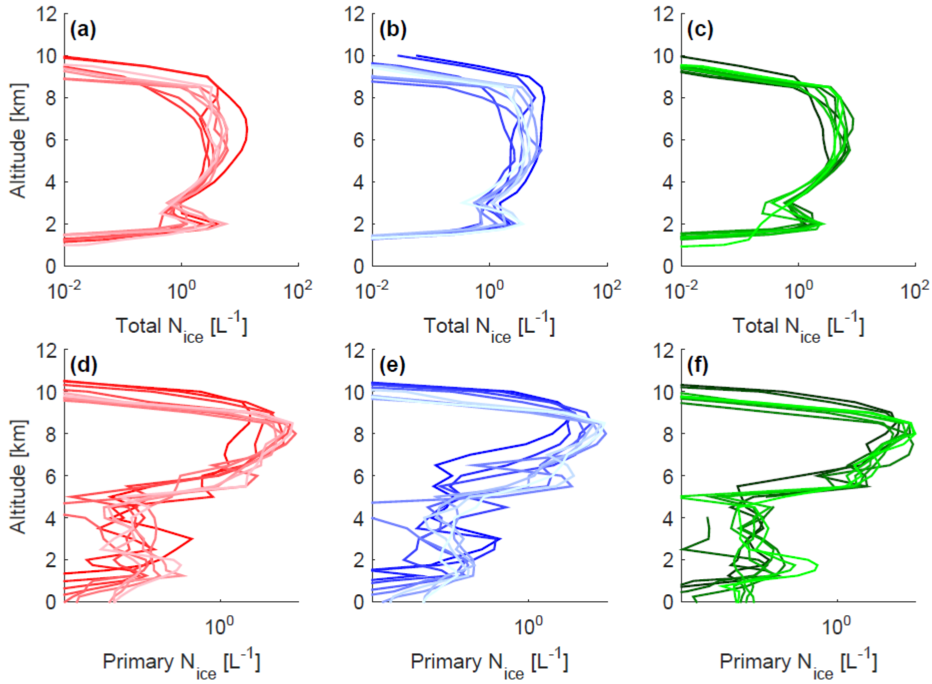


Figure S3 Temporal evolution of N_{ice} (panels a, b, c) and N_{pri} (panels d, e, f) profiles in the RS1 simulation from three, randomly-sampled latitude / longitude locations in the vicinty of CFARR. Eight profiles are shown for each location, one for each half hour from 18:00 UTC to 21:30 UTC with the darker colors representing earlier times and the lighter ones later times.

p. 17. It would be good to see answers to the points raised in 2/ above. It would also be good to state what the overall domain mean change in precipitation is due to secondary production processes.

We have added to the enumerated conclusions to address relative importance and parameterizability of these processes. To the first conclusion on ice production rates from primary nucleation versus secondary production, we build off the new Figure 6 and add:

"In this case, we saw that rime splintering was the most important process in line with the conclusions of Crosier et al. 2014; however, underestimation of vertical velocities in the cold front also led to underestimation in simulated radar reflectivity relative to observations. If this Z_{DH} difference was caused by additional graupel at higher altitudes, contributions from collisional breakup could have been much higher than the 0.1 L^{-1} per half hour found here. A low bias in updrafts also generates fewer raindrops at altitude and limits the contribution from frozen droplet shattering (in this case to an intermediate production rate of 10 L^{-1} per half hour)."

In regard to how the processes can best be parameterized we refer to the supplemental figure that shows large hydrometeor number concentrations or mixing ratios. There is a strong relation, for example, in the structure of the $N_{i,sec}$ from collisional breakup and the graupel mixing ratio. In the conclusions, we reiterate that: "*Underestimations stem in part from low biases in the updraft velocity. If the vertical velocities can be brought into agreement with observations, then criteria in these values as well as temperature could be used together to parameterize secondary production in appropriate thermodynamic zones. For two-moment schemes, graupel, snow, and raindrop criteria could be implemented for these processes.*"

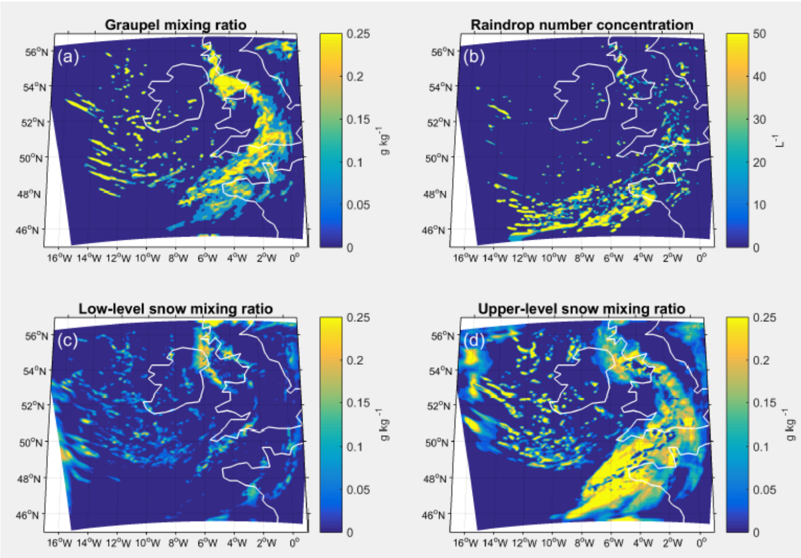


Figure S5 Graupel mixing ratio (a), snow mixing ratio (b), large-scale graupel quantity (c), and rain drop number concentration (d) in the simulation domain at 18:00 UTC for the RS2 simulation.

In regard to the second point, as mentioned in the response for p. 13, 3 above, we have written "*The sum of [P_{tot}] deviations over the whole subdomain is an additional 23.9 m of precipitation for the RS1 simulation, 25.6 m for RS2, 16.9 m for DS1, and 16.6 m for ALL.*"