Responses to reviewers for *"Implementation of predicted rime mass in the bin microphysics scheme DESCAM 3D: Heavy Snowfall event during ICE-POP 2018"*

By Pierre Grzegorczyk et al.

30 June, 2026

We would like to thank both reviewers for their time and for their constructive comments on our manuscript. Our responses to each of the comments are provided below in red.

REVIEWER #1

The authors have done a thorough and impressive job in significantly modifying and improving the manuscript. From my end, with the exception of one major point, all of my original concerns have been addressed. With regards to the simplified treatment of melting, I think that the authors partially misunderstood what I was suggesting and underestimate the relevance of this point to their primary focus of this study, the impacts of predicted rime density – I have elaborated on this below. I am struggling with what to recommend regarding the manuscript. Strictly speaking, I think this point should really be a deal breaker; however, the authors have already done a lot of work and it is a great paper (and now quite long). But with this oversimplification of melting, some of the conclusions pertaining to the stated scientific objectives are in question. With this, I will recommend major revision and strongly recommend that the authors implement continuous (but still simplified; no liquid fraction) melting, as described below. That said, if the authors opt not to go this route, I will not put up further barriers/delays for this publication – provided that at a minimum this limitation (to interpreting the results on the impacts of rime density) is adequately acknowledged and discussed.

MAJOR CONCERNS

I think the authors are understating the value of simulating continuous melting (with predictive liquid fraction) in arguing that it only affects a relatively thin region (the melting layer). It also impacts the extent to which partially-melted ice can refreeze once it falls back into cold air. This can mean the difference between simulating/predicting precipitation as either ice pellets or freezing rain at the surface. This complex situation is quite common in winter storms in eastern North America, e.g. – and the societal impact is significant. Of more direct importance for this paper is that the oversimplified representation in DESCAM probably results in an incorrect understanding of the impacts of predicted rime mass – so melting cannot just be dismissed as "beyond the scope of this study". For example, line 694 states that "the melting of massive rimed ice particles also produces larger drops … leading to an earlier onset of rainfall and in increase in total precipitation by 25%" for the squall line simulation and also that "large drops lead to … slowing squall line propagation". These are significant changes and are clearly related to rime density, but they are clearly also strongly related to the way melting is treated. This cannot be overlooked unless you can argue/show convincingly that the melting treatment does not impact the signal of the impact of predicted rime density (which you cannot without a model that treats melting in more detail).

Also, the authors partially misunderstood my concern about the oversimplified melting. While it is true that this (predicted liquid fraction) is the best approach, as previously done in DESCAM, as described, I was more concerned the instantaneous melting of ice in all size bins, which I believe is what is done now. A cheaper alternative, with no extra variables, is to melt ice gradually but implicitly shed all melted mass to drops on each time step. This is what has been done for years in bulk schemes – the "ice" (tiny crystals) category may melt instantly to rain in some schemes but most compute gradual melting for "snow" and "graupel". With no liquid fraction you cannot represent mixed-phase particles, so this too is a simplification, but a much less egregious and problematic one as instant melting of all ice.

While I hate to insist on making big model changes (though this would be easy to implement and, as I am arguing, a much better approach), the melting point cannot be casually dismissed within the context of studying the impacts of predicted rime density.

We agree that the instantaneous melting treatment of ice particles can have an important influence on our results and that a continuous treatment of melting is particularly important in this study, as we also investigate the liquid phase. Therefore, major modifications have been made to DESCAM. We reintegrated the prognostic melting scheme developed by Planche et al. (2014) into the current version of DESCAM and run again both simulations (idealized squall line and ICEPOP snowfall event).

Implementing this scheme required substantial code modifications (for microphysics and its link with the dynamical core) because the current version of DESCAM includes a much more detailed representation of ice particles than the version used by Planche et al. (2014). Nevertheless, the Planche et al. formulation of the melting scheme remains fully compatible with the present microphysical framework. We considered more appropriate to reintegrate the existing prognostic melting scheme rather than introduce a simplified parameterization, providing a more complete framework for future studies and developments of DESCAM.

Overall, the new simulations confirm the main conclusions of the previous manuscript version for both the squall-line and ICE-POP cases. However, prognostic melting reduces the influence of prognostic rime mass on surface precipitation. We attribute this to the smoother transition from ice particles to rain, which makes the impact of predicted rime less abrupt than in the previous simulations. For example, in the squall-line case, the increase in accumulated precipitation decreases from 25% in the previous simulations to 15% when prognostic melting is included. For the ICE-POP case, the corresponding change remains similar since it changed from 6.5% to 7.2%. The conclusions regarding the differences between CTRL and pRIME remain unchanged with the continuous melting scheme, although some minor quantitative differences are observed. Therefore, we have revised some of the descriptions of the results section and updated Section 5 (Limitations and Perspectives) accordingly. All modifications are highlighted in the pdf diff file.

It is important to say that the completion of the ICE-POP simulations has been delayed by technical issues affecting French supercomputing facilities during the recent historical heat wave, during which parts of the system were temporarily shut down (due to cooling issues). Fortunately, both the CTRL and pRIME simulations have already reached the end of the shallow precipitation event (15:00 UTC on 8 March), after which no further precipitation occurs. Since the main conclusions are based on the period preceding the end of the shallow precipitation system, our conclusions are not affected by the few hours missing for the ICEPOP case. We will for sure complete the pRIME and CTRL simulation for ICEPOP before the next revision round so that the final manuscript shows the full two days simulation period.

MINOR POINTS:

1. With regards to my comment on aerosols (CCN) on the impact of rime density, laboratory experiments (e.g. Heymfied 1983 [I think]; Cober and List 1993) show that the droplet sizes impact the density of the resulting riming since smaller droplets freeze quickly on impact, leaving gaps, while larger drops “splat” and the liquid spreads out more evenly before freezing. Since CCN concentrations affect droplet sizes, aerosols ultimately affect rime density, albeit indirectly. (e.g. see Jouan and Milbrandt, 2019).

In DESCAM, droplets are activated from aerosol particles following the resolution of the Köhler equation, which means that the number, size of aerosol particles and supersaturation (for droplet activation) directly influence the drop size distribution. We therefore agree that aerosol particles considered as CCN can indirectly affect rime density. Studies such as Cober and List 1993 or even the study of Williams and Zhang (1996) provide relationships for rime density as a function of temperature, impact velocity, and mean droplet radius, which could be useful.

We have updated the discussion on particle density in the paper accordingly (see section 5 lines 625-628) with « *The rime volume can be determined from the rime density, which has been observed to depend on temperature, droplet size, and impact velocity (Cober and List, 1993; Williams and Zhang, 1996), thereby reflecting the droplet size distribution predicted by DESCAM and indirectly the influence of aerosol particles.* »

2. Line 127: I missed this the first time, but those m-D parameters for hail seem odd. I would think one could just assume spherical high-density particles, thus ALPHA = (pi/6)*rho_i and BETA = 3. Just curious. Perhaps a reference could be added and/or a brief description of the physical implications of those m-D parameters.

We used the values given by Heymsfield et al. (2018) for this m-D relation, but other relation could be relevant. Of course, in absence of rime density, the choice of such distribution can have important implications. These implications are summarized as follows in lines 146-150 *to* aware the reader of this critical point: « *However, the choice of the graupel properties in this study is quite arbitrary, given the fact that graupel properties are highly variable in nature (e.g. see Heymsfield, 1978). Furthermore, the choice of the graupel properties is critical, as it can strongly influence terminal velocity, collision kernels for riming and aggregation, SIP processes, and ultimately precipitation rates. Therefore, as discussed in Section 5, future developments will focus on predicting ice density directly to remove the dependency on a fixed m–D relation and better represent processes such as wet growth that would lead to much denser rimed particles.* »

3. Line 170: It should just be “2015”; no “a”. Same with line 252. (Part 2 in that series was Morrison et al. (2015); Part 3 was Milbrandt and Morrison (2016).

This is corrected.

4. Line 296: “topography” (which refers to all land surfaces) should be “orography”. Same with caption for Fig. 4.

This is corrected.

5. Fig. 7: For c) – f) it would be helpful to have the simulation configurations indicated in the figure panels themselves. Same comment for other figures – in principle one should not even need the caption. Also, personally it seems more intuitive to put CTRL first (left panels) – as is done in Fig. 8. Regardless, be consistent with the order from figure to figure. There are other inconsistencies like that in the paper which, while not at all wrong, are a but off-putting. For example, Line 395 reads "DESCAM pRIME and CTRL simulations" and then on line 399 it reads "DESCAM CTRL and pRIME". Just a suggestion. Also, in my opinion it is unnecessary to keep repeating "DESCAM" since "CTRL" and "pRIME" have been clearly defined to be DESCAM configurations. It is even slightly misleading since writing "DESCAM CTRL" seems to suggest that there may be one or more other "CTRL" configurations.

Yes we agree with that, we have corrected the inconsistencies regarding the order and naming of the simulations.

6. Line 414: I suggest "…model simulations reproduce the presence…"

Thank you, this is corrected.

7. Fig. 15. d) could be "pRIME – CTRL" so that positive amounts mean increase values for the modified scheme.

Yes, this is indicated in the figure now.

8. Line 654: Why would continuous melting require a prognostic treatment of ice particle density? (e.g. P3 unrimed ice). One can simply diagnose the density of the mixed-phase particle using mass-weighting of the dry part (from Fig. 1) and the liquid part. Prognostic density is better, but it is not a show-stopper to predict the liquid fraction and improve the representation of melting.

Yes, this is true. Since we have reintroduced prognosed melting, we have deleted the paragraph (in Section 5) where it was mentioned.

9. Lines 663-672: The second sentence "However, the behaviour of ice … remains poorly understood" is an overstatement and incorrectly conveys the message that the benefits of improving the prediction of ice particle properties are unknown in general. Certainly this is true for some processes, such as the ones mentions, but the benefits are very well understood for other important processes. For example, the impacts of density on fall speeds – and therefore in turn on other processes (e.g. accretion) -- are well understood. I recommend rephrasing that section to reflect this point. Also, I agree with your last sentence – you may wish to add that this point highlights the continued need for improved field studies, laboratory research, and theoretical research in order to improve the representation of microphysics in models (and you may consider adding the reference to Morrison et al. 2020; JAMES "Confront the challenges of modeling cloud and precipitation microphysics").

The purpose of this sentence was to say that the refinement of model parameterizations is challenging due to the complex nature of ice particle processes, and the lack of information for some processes. We didn't intend to imply that improving ice particle models is fundamentally unknown. Therefore, this sentence was reformulated in line 635-637 as follows *« While models advance toward predicting detailed properties of ice particles, providing important benefits, some unknowns still remain about* *their behavior. This is particularly true for aggregation since the sticking and collision efficiencies for different properties of ice particles are not well known... »*

We also added the reference to Morrison et al. 2020.

10. Line 757: Should be "We also thank…" (not "thanks").

Corrected.

REVIEWER #2

p. 14 l. 332 "effect of the new prediction of rime scheme" → "effect of the new predicted rime scheme"

Corrected.

fig. 6 Is IWC the total ice mass in pRIME or only the unrimed ice mass? If needed, also plot the sum of RIWC and IWC for pRIME, to facilitate the comparison with IWC CTRL?
Yes IWC is the total ice mass, including the rime mass. The IWC in pRIME is just lower compared to CTRL which could make the impression the RIWC is not included in IWC. We have added this clarification *'(note that rime mass (RWC) is included in the IWC)'* in line 350.

p.15 l. 336 "is also a results from" → "results from"

Corrected

fig 7 In the caption I would move "y direction averaged" immediately after "Vertical cross section". In panels c to f, I suggest to add "CTRL" and "pRIME". Why is the color palette different in panels a and b?

We modified the caption with *'Vertical cross section (y-direction averaged)'* and added CTRL and pRIME to panel c) to f) as suggested.

We use a different color palette in panels a) and b) because they display the rime fraction, whereas the other panels show the total ice and liquid water contents, which are different variables. We chose a distinct colormap to emphasize this distinction.

p. 17 l. 357 altitude → altitudes

Corrected.

p. 17 l. 362 Rime → pRIME
Corrected.

p. 19 l. 418 applies → apply

Corrected.

fig. 10 & 11 & associated text: state/check hours considered for the plots (missing for fig 10, fig 11 says after 9 UTC but in fig 9 the shallow system starts around 5 UTC)

After your first comment which was *'fig. 7: I think you could enhance the figure by restricting the time period considered a bit more, to avoid the signal above 2km in the observations (only use observations after 8 UTC for example). And in the simulations, maybe only consider the reflectivities between 10 and 13 UTC, when the "gap" between the shallow and high clouds is the most important'*, we choose to consider the shallow system only after 9 UTC in our analysis (for both the radar data and the model) to avoid the transition zone between the deep and shallow system in our analysis (e.g. avoid radar signal is in observations above 2 km for the shallow system before 9 UTC in the observations). Regarding the deep system, we consider values up to 06:00 UTC to avoid the transition zone in our analysis. We have added "(i.e., before 06:00 UTC on March 8)" to the caption of Fig. 10 for the deep cloud system, and "(the transition between the deep and shallow systems between 06:00 and 09:00 UTC is excluded from these figures)" when mentioning Figs. 10 and 11 for the first time (line 443 - 444).

For further clarity, we also adjusted the position of the dashed line in Fig. 9 to 06:00 UTC instead of 04:00 UTC. This line represents the end of the deep system and was corrected to match the time limit specified in the text and used in the analysis presented in Fig. 10.

p. 21 l. 442 show → shows

Corrected.

p. 21 l. 443 similar as → similar to
Corrected.

p. 22 l. 462 underestimates → underestimate

Corrected.

p. 22 l. 469 I would have kept "velocities" instead of "velocity". Or change appear → appears?

We changed 'apper' to 'appears'.

p. 23 l. 473 reflectivity → reflectivities?
Corrected.

p. 24 l. 497 leads → lead

Corrected.
p. 26 l. 527 "see 5" → "see section 5"

Corrected.

p. 27 l. 547 "precipitation peak" → "precipitation peaks"

Corrected.

p. 28 l. 569 displays → display

Corrected.

p. 28 l. 571 remove unnecessary ","

Corrected.
fig. 17 mistake in caption stating that CTRL is in solid line (exchange CTRL and pRIME)
Caption corrected.

p. 33 l. 646-647 check sentence (starting from “implementing”).

We corrected the repetition of “implementing” and “integrated.”

p. 33 l. 648 “the water ice ratio of melting ice particle” → “liquid fraction of melting ice particles” ?

As we reintroduced the predicted melting in DESCAM, we removed the point where this sentence was written. However, a new sentence similar to this one was added to describe the model in lines 109 – 111: ‘*The melting of ice particles is considered to be continuous based on the predicted liquid mass fraction implemented by Planche et al. (2014)’*

p. 34 l. 664 require → requires

Corrected.
p. 35 l. 693 “leads to further promote” → “further promotes”

Corrected.

p. 35 l. 712 decrease of → decrease in

Corrected.
p. 35 l. 724-725 “a substantial effect” of “substantial effects”

Corrected with *“substantial effects”.*