

# 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 Grzegorzczak et al.

We would like to thank the editor and the two reviewers for their time and their constructive comments on our manuscript. In this document, we first provide a general response addressing the main points raised by the reviewers and summarizing the major changes made to the manuscript. In a second time, we present a detailed, point by point response to each reviewer’s comments (our responses are in red).

As noted by Reviewer 1 in our initial submission, the manuscript directly focuses on the ICE-POP case study with model to observation comparisons without fully examining the impact of predicted rime on microphysical properties. Similarly, Reviewer 2 also emphasized that the analyses of the ICE-POP case did not focus enough on the effect of predicted rime on microphysics properties. Consequently, as recommended, we choose to add a new case study which is more suitable to analyze in detail the effect of the predicted rime implementation. This case consists of a 2D idealized squall line case (run in a 3D framework). This type of idealized simulation has been widely used to evaluate developments in various microphysics schemes and to perform model intercomparisons. To summarize, this case focuses on the amount and location of rime mass in the cloud system, how it is distributed across different ice particle sizes, and the effects of the rime mass on microphysics and precipitation as well as on the dynamics of the squall line system. The results of this case are included prior to the section describing the results of the ICE-POP case, similarly done in [Park et al. \(2024\)](#).

Because the addition of this simulation increased both the length of the manuscript and the number of figures, parts of the ICE-POP analysis were shortened. Some results such as the comparisons between DESCAM and the X-band radar were moved to supplementary material (analyses for W-band radar gives similar information and were retained in the paper)

Both reviewers also questioned why continuous melting is not represented in DESCAM. In a previous version of the model ([Planche et al., 2014](#)), continuous melting was implemented by prognosing the water-ice ratio (i.e. the liquid water fraction of ice particle) for the 30 largest bins of the 39 used in DESCAM to describe ice crystals distribution. However, this approach was not retained in the other versions of the model for computational efficiency. Indeed, it required introducing 30 additional prognostic variables to describe a melting layer that is only a few hundred meters height, while these variables had to be prognosed for the entire cloud system. Consequently, continuous melting has not been included in the model for over 10 years now.

Furthermore, this implementation assumed a fixed ice particle density of  $0.9 \text{ g cm}^{-3}$ , which corresponded to the constant density of ice used in DESCAM at that time. Reintroducing this continuous melting scheme would no longer be appropriate, as ice particle density is not  $0.9 \text{ g cm}^{-3}$  in the current version of the model. Although density can now be diagnosed, doing so would require substantial modifications to the melting parameterization of [Planche et al \(2014\)](#), which is not the purpose of this study.

In our study, the vertical resolution near the melting layer is 250 m for the idealized squall line simulations (the new case study) and about 150 m in the ICE-POP case. Such resolutions are not the most appropriate to properly represent the continuous melting process. Indeed, [Planche et al \(2014\)](#) used a vertical resolution of 40 meters layer and reported a melting layer of roughly 200 m height which is close to the vertical resolution of the squall line and ICE-POP simulations of this study).

Even if instantaneous melting can influence precipitation on the surface and is important for accurately representing solid precipitation at the ground (e.g. hail or graupel) in convective clouds, we think that it is beyond the scope of our study. However, we have added a new section before the conclusions, titled "Limitations and Perspectives for DESCAM," where we discuss the missing elements in DESCAM and potential future developments such as prognosing the particle density, continuous melting or even ice particle habit. Other models, such as the P3 scheme, have followed a similar chronological development, first focusing on rime mass prediction ([Morrison and Grabowski, 2008](#)), secondly on ice density ([Morrison and Milbrandt, 2015](#)) and finally on melting prediction ([Cholette et al. 2019](#)).

## **Detailed comments**

### **Reviewer #1**

This manuscript describes the modifications to the DESCAM bin microphysics scheme whereby the predicted rime fraction for ice is added. The authors then conducted high-resolution real-case mesoscale model simulations, using the original and modified scheme, for a heavy snowfall event that was well observed during the ICE-POP 2018 field campaign in South Korea. The authors conclude that the modified scheme improves the simulated precipitation and other fields compared to observations. This is a potentially valuable paper in a few ways. First, although the introduction of prognostic rime mass is not novel in the microphysics modeling community, it is new for DESCAM and it represents an important development to that model. Second, the use of microphysical data from field campaigns such as ICE-POP 2018 is very useful and interesting for examining detailed microphysics schemes. In that and other regards, the manuscript is nice in lots of way, however, it has some major shortcomings (described below) that must be addressed in order for this to be considered for publication.

Given that I am recommending that a DESCAM be further developed (with explicit melting) in order to properly study the effects of predicted rime mass and that idealized tests/demonstrations be added, then to be followed by a modifications to the real-case simulation examination section, one possible path forward would be to re-cast this as a two-part paper: 1) description of new developments + idealized tests; 2) Real-case simulation of the ICE-POP case. It may simply be too long as a single paper and I believe that the additions I am recommending are important. I will leave that to the authors to decide, but the comments below must be addressed. With that, I will recommend major revision.

#### **SPECIFIC COMMENTS**

1. The implementation of predicted rime mass in the DESCAM scheme is a major development. The authors go from describing the new method to attempting to illustrate the impacts through a full real case 3D simulation. This is a big leap. Understanding and evaluating the changes to a microphysics

scheme is complicated enough; the authors have gone directly to the most challenging approach. Microphysical pathways in a 3D model are very complicated and evaluation based on comparison to observations is inherently challenging. For a major development of the type presented in this study, the authors should really start by illustrating the behaviour of the modified scheme in a very simple context, such as a 0D or 1D model framework, in order to provide the reader a basic understanding of how the new scheme works and what it does, as well as to illustrate that the changes do indeed do what they are supposed to do. I strongly recommend adding a section on idealized tests and demonstrations, even if it means reducing the amount that is presented for the 3D case.

Our response to this comment can be found in the first part of the document. To address this point and strengthen the manuscript, a new 2D idealized simulation focusing on the effects of predicted rime on cloud microphysics has been added.

2. The setup of sensitivity experiment is backwards. Normally in a control experiment one takes a baseline configuration, which is the control, and then sets out to conduct one or more sensitivity tests to examine the impact of one or more changes. In this case, the most logical setup would be to prescribe the control (or control simulation), which in this case would be the simulation with the unmodified DESCAM scheme, the control configuration, and define the experiment simulation to be the one with the modified code. Following from point 1, the one could define the CTRL (baseline DESCAM) and MOD (DESCAM with rime fraction) configurations and apply them to the idealized simulations/demos and then to the ICEPOP case simulations.

Yes, we agree, the names of the experiments were not very appropriate. In the revised manuscript, the simulation including the new predicted rime implementation is called 'pRIME', while the unmodified DESCAM simulation is now named 'CTRL'. As requested, for both the idealized simulation (squall line) and the ICE-POP case, the manuscript now includes the CTRL simulation as a baseline for assessing the changes of the pRIME experiment.

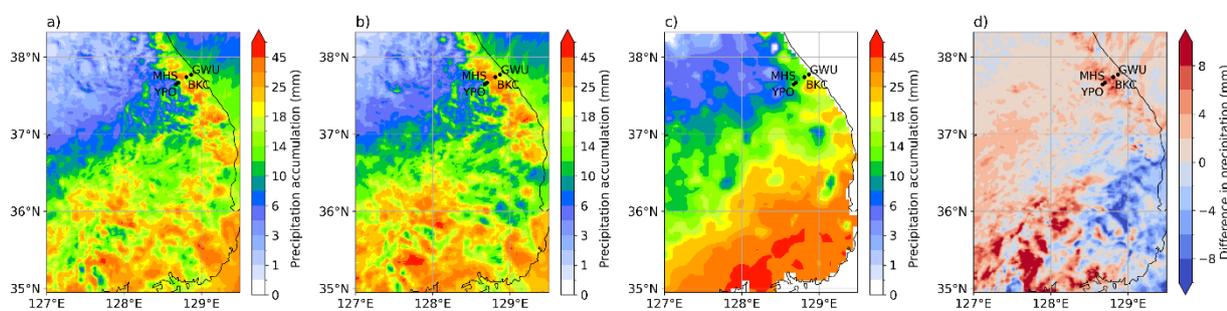
3. When one uses a real-case simulation and comparison to observations to illustrate the benefits (or any kind of impact) of a particular set of changes to a model, one first needs to demonstrate that the control (unmodified) simulation is sufficiently realistic that one may proceed to use the modeling framework to meaningfully examine the impacts of sensitivity tests using the modified model. This is why it is necessary to start off with the baseline control. When jumping straight to comparisons between observations and simulations with the modified model, as the authors have done, one cannot tell if discrepancies between the observations and the model are due to limitations of the model set up (which could include a number of things, starting with the initial conditions) or negative impacts resulting from the changes. Figures 5, 6, 7, 8, 10 compare observations and the simulation with the modified code, but no comparison to what should be called the control run (with no rime). Every comparison here should include simulations from both the original and modified microphysics scheme.

We agree that displaying both simulations improves the clarity of the analysis and helps to better illustrate the changes induced by the implementation of predicted rime. The CTRL simulation has now been added to all figures where it was previously missing. Most of the issues described for the pRIME simulation are also present in CTRL. This indicates that, as mentioned initially in the paper, they are due to the model setup rather than the implementation of predicted rime, which is the main purpose of this study.

4. In the illustrations of model precipitation (Fig. 11), the observations are conspicuous in their absence. There was a dense network of surface precipitation observations in that region for ICEPOP. This needs to be added.

We have added precipitation accumulation products based on observations from 494 Automatic Weather Stations (AWS) operated by the Korea Meteorological Administration (KMA) in panel (c) of the figure 15. It should be noted that this product only includes operational AWS and does not incorporate the Parsivel disdrometer deployed during the campaign. This explains why near the measurement sites, the values of this product differ from those reported by the Parsivel measurements. Nevertheless, despite these local differences, the AWS observations provide a useful reference to evaluate the global precipitation field simulated by DESCAM. Since this rain product has some limitations to describe local variations of precipitation accumulation, the measurements from the Parsivel instrument deployed during ICE-POP are complementary and more appropriate for evaluating DESCAM in mountainous areas.

The Fig 11 (now Fig 15 in the revised version of the manuscript) have been modified as follow :



*Precipitation accumulation (ice + rain) within the innermost domain from 07 March 00:00 to 09 March 00:00 UTC: a) DESCAM CTRL, b) DESCAM pRIME, c) Rain accumulation product from Automatic Weather Stations (AWS) operated by the Korea Meteorological Administration (KMA) interpolated using the method of Cressman (1959) and d) difference between DESCAM CTRL in a) and DESCAM pRIME in b).*

The content of the paper was modified according to this new figure.

5. The explanation of particle density needs to be expanded upon in section 2.2, not just summarized by a reference to Heymsfield et al. 2018 (line 107). What is the density of ice with a rime fraction of 1? Is there no distinction between graupel and hail? I gather from line (“... it is currently not the case in DESCAM [variable density]”).

We have added the following description in section 2.2 “*In DESCAM, the density of ice particles can be diagnosed from the mass–diameter relationships presented in Fig. 1, resulting in rimed particles being denser than unrimed ones. Fully rimed particles, typically classified as graupel, exhibit densities of around 0.2 g cm<sup>-3</sup> for millimeter-sized particles. This density is comparable to the graupel generated in the laboratory experiments of Grzegorzczuk et al. (2023) under dry growth conditions (i.e., liquid water content of 0.7 g m<sup>-3</sup> at -20 °C), meaning that fully rimed particles in DESCAM correspond to low-density graupel. This density results from the graupel mass–diameter relationship adopted from Heymsfield et al. (2018) (Table 3, “Graupel and small hail”). Future developments will focus on predicting ice particle density and accounting for wet growth to produce denser rimed particles, as discussed in Section 5.*”

6. Line 349, "... in DESCAM [it is assumed that] ice particles melt instantaneously at the 0C isotherm". This is a huge weakness in the DESCAM microphysics scheme in terms of modeling ice – it is not just a minor simplification. That might be fine for tiny crystal, but certainly not for ice that would be considered to be "snow" (large crystals or aggregates) or graupel. Presumably it would make more sense to address this deficiency in the scheme before adding predicted rime fraction. I definitely think the calculation of explicit melting should be added (it should be added anyway) in order to examine the impacts of rime fraction in the context of a case like ICE-POP. The rime fraction will affect the ice fall speed, which will affect the horizontal distance ice is transported before completely melting, which will therefore affect the spatial distribution of precipitation, particularly in the mountainous regions. So in order to understand the impact of predicted rime fraction, melting has to be treated more rigorously.

Our response to this comment is presented in the first part of the document. We agree that incorporating continuous melting in DESCAM is an important perspective for future model development as mentioned in Section 5 of the new manuscript version *"A melting scheme considering the water ice ratio of melting ice particle was implemented in a former version of DESCAM 3D (Planche et al., 2014) but was later removed for computational efficiency, as it required approximately 30 additional prognostic variables relevant for a few vertical levels. However, several studies have highlighted the importance of ice particle continuous melting for both microphysical processes, precipitation and dynamics of convective cloud systems (Phillips et al., 2007; Cholette et al., 2019, 2023; Milbrandt et al., 2025). As DESCAM now aims to predict particle properties, the melting scheme of Planche et al. (2014) could be reintroduced and extended to account for particle density, which strongly influences melting behavior. However, this would first require a prognostic treatment of ice particle density mentioned in the previous point. Consequently, the scheme should be reformulated to depend on particle density from low density snow particles (Mitra et al., 1990) to heavily rimed particles (Theis et al., 2022). Additionally, shedding effects associated with large rimed particles should be considered (Rasmussen and Heymsfield, 1987a, b, c), as they are currently not represented."*

#### MINOR POINTS

1. The title could be improved. "Heavy snowfall event during ICE-POP 2018" by itself does not mean much; it is just a noun.

We tried to improve it by including the new numerical experiments of the squall line system. We propose the following title *"Implementation of predicted rime mass in the bin microphysics scheme DESCAM 3D: Evaluation for an idealized squall line system and a heavy snowfall event during ICE-POP 2018"*

2. Line 310, "The overproduction of these small ice particles likely originating from a numerical artifact." This sounds like a guess and is not very satisfactory, particularly given the negative impacts on the simulation.

This sentence may have been a bit unclear. In fact, we clearly identified that this issue is a direct consequence of a numerical artifact (related to boundary condition issues). We wanted to exercise caution in interpreting this, which may have led to a different understanding of the sentence.

This sentence and the corresponding paragraph have been rephrased (also considering Reviewer #2 comments). See line 446. *"We identified that the overproduction of these small ice particles originates from a numerical artifact. Indeed, in the south-western and northwestern corners of the outermost domain, mountainous terrain near the boundaries (Fig. 4a) appears to influence incoming air masses*

*by generating artificial updrafts at the upper levels of the domain. These updrafts lead to an humidity enhancement at the western lateral boundary, triggering the production of a large number of ice particles by heterogeneous and homogeneous ice nucleation. It is important to note that this artificial upper cloud layer may contaminate the shallow cloud system through a seeder–feeder mechanism, potentially influencing its microphysical properties. A thorough evaluation of the microphysics of the shallow system against ICE-POP ground-based observations will be presented in the following sections. .”*

3. The discussion on the impact the fixed rime density could be expanded upon. If variable density were to be added in DESCAM, this would exploit the predictive aspect of aerosols since the liquid droplet size is important for the rime density.

We are not sure to understand the link between “the predictive aspect of aerosols” and the fixed rime density. Regarding drop size, we agree that it may influence rime density. However, to our knowledge, there is currently no observations describing the impact of drop size distributions on ice rime density that could be implemented in microphysics scheme.

We added some discussions about the perspective of predicted density for ice particles in Section 5 *“In the current implementation based on Morrison and Grabowski (2010), the size of rimed particles is constrained by a mass diameter ( $m$ – $D$ ) relation for graupel based on Heymsfield et al. (2018), which effectively limits ice density to graupel growing by pure dry growth. A future development would be to extend the predicted rime mass toward a prognostic treatment of ice particle density by introducing a prognostic rime volume, as proposed by Morrison and Milbrandt (2015) and implemented in other microphysics schemes (e.g. Brdar and Seifert, 2018). Given that in DESCAM collision kernels and terminal velocities of ice particles are dependent on rime mass, implementing a prognostic density could be straightforwardly integrated into the model.*

## **Reviewer #2**

This article describes the implementation of a prognostic rime fraction in the bin microphysical scheme DESCAM, and demonstrates its performance for a simulation at kilometric resolution of one event from the ICE-POP 2018 field campaign. Although already available in some other microphysical schemes, this new development for DESCAM is an important improvement, as it enables a better representation of ice hydrometeors characteristics. The real-case evaluation and comparison to observations is also very interesting, highlighting impacts of the prognostic rime fraction on cloud composition, but also on the cumulated precipitations at the ground and their type. However, I think this paper does not put enough emphasis on the microphysical processes on the one hand, and I have some concerns about the evaluation in comparison to the selected ICE-POP case on the other. It felt a bit awkward, somewhere between a classical demonstration of model abilities and a dedicated case study. Thus, I believe this article requires substantial modifications before it can be accepted for publication.

### **Major comments**

#### *1. Paper structure*

As this paper presents the newly available prognostic rime fraction for icy hydrometeors in DESCAM, I expected a more thorough demonstration of how this impacts not only the general characteristics of the cloud, but also microphysical processes. I think the paper should have a full section dedicated to

the differences between the two simulations (with and without the predicted rime fraction), rather than some elements of this comparison here and there in what seems to be more a case study. This section could go into more details about microphysical processes, even without involving observations. For example, how are the secondary ice production mechanisms affected? How different is the melting layer (there is a mention of the melting layer, although it does not appear on the displayed radar observations)? How different is the supercooled liquid water? What about the vertical structure of clouds? The cross sections in fig. 16 are very interesting but the discussion remains really short. For example, they suggest that the rime fraction is strictly limited to regions where there is liquid water and below, are there no updrafts in the deep cloud able to lift rimed particles a bit higher up?

All elements of our response to this comment can be found in the first part of this document, where we address common reviewer comments regarding the general structure of the manuscript. In summary, the impact of our implementation on microphysical properties is now better presented with the new squall line idealized case. This includes Figures 5 to 8, which illustrate, for example, the effect of predicted rime on cloud water content (ice, liquid, and rime) and the impact on secondary ice production (SIP), as suggested.

Regarding the observations, no bright band corresponding to the melting layer is visible in the radar data because the radars used in this study were deployed at the MHS station, where nearly all precipitation consisted of ice particles.

Regarding the rime fraction, for the squall line, Figures 6a and 7a in the revised manuscript illustrate the predicted rime mass as well as the supercooled liquid water content. From Fig 6a, we can conclude that rimed particles are indeed transported to higher altitudes, as the rime mass content remains strong up to 9 km, while the supercooled liquid water content becomes very low above 6 km.

## 2. Evaluation in comparison to ICE-POP observations

Overall, I would like to have more information about the studied case, I give some examples of missing things in the minor comments below. But I also have specific concerns about some parts of the comparison:

- The simulated W band reflectivities in fig. 5d show that the deep and shallow clouds in the shallow system are not separated by a layer of cloud-free air. This configuration typically leads to contamination in the low cloud from particles precipitating from the cloud above, and/or strong radiative influence. This is never discussed in the article, and may largely disturb the shallow cloud composition. Does that invalidate all or some comments about the shallow clouds? This should at least be discussed in the conclusion.

It is indeed possible that the artificial upper cloud layer could contaminate the lower shallow cloud system by a seeder-feeder mechanism. However, overall, the ground-level properties simulated by DESCAM do not exhibit large discrepancies compared to the observations. The modeled and measured particle size distributions of the shallow cloud system, as well as the rime fraction tendency shown in Figs. 13 and 15, are in quite fair agreement. In Fig. 14, the modeled number concentration of ice particles at the surface remains below  $10 \text{ L}^{-1}$ , as in the observations. While these microphysical properties at the surface appear to be reasonably well represented by the model, it is nevertheless difficult to assess whether the presence of the upper cloud layer significantly affects the shallow cloud system, particularly at higher levels. Therefore, we cannot rule out the possibility that the upper cloud layer may influence the shallower clouds.

A first sentence is now added when discussing the origin of the upper cloud layer (see line 450): *"It is important to note that this artificial upper cloud layer may contaminate the shallow cloud system*

through a seeder–feeder mechanism, potentially influencing its microphysical properties. A thorough evaluation of the microphysics of the shallow system against ICE-POP ground-based observations will be presented in the following sections.”

Secondly, another discussion is now added in the ground-based to model comparison section (see Line 555) “Furthermore, even if the overall properties of the shallow system agree well with ground-based observations, a potential contamination by the artificial cloud layer produced by DESCAM (see Fig. 9) on the present results cannot be ruled out.”

- In section 4.3, the differences between control and no-rime in fig. 13 are not clear enough to state that there is an improvement with control regarding the overestimation of large ice particles?

The Fig 13 (Fig 16 in the revised manuscript) can maybe give the impression that differences in mean mass diameter are small due to the fact that the y axis is going up to 10 mm. However, for the shallow system, by comparing the values of the mean mass diameter themselves, we demonstrate an improvement with the predicted rime. As mentioned in line 518 “In the shallow cloud phase, PIP measurements in Fig. 16d, show smaller ice particle than in the deep system with a mean diameter around 0.5 mm. While the pRIME simulation overestimates the size of the ice particles with a mean diameter around 1 mm, DESCAM CTRL generates even larger particles, of about 1.5 mm before 12:00 UTC (March 8). This shows that the implementation of the predicted rime distribution helps to mitigate the overestimation of large ice particles given by DESCAM CTRL ”.

The mean diameter of DESCAM CTRL and pRIME are confronted to the PIP observations in panel d) of Fig. 13 (Fig. 16 in the revised manuscript). This helps to see the differences between the two simulations.

- In section 4.3 again, the comparison of observed riming index and simulated rime mass fraction (fig. 15) is used to demonstrate the improvement with with a predicted rime fraction. However, looking at this figure and fig. 14, I would argue that some data of fig. 15 should be removed when the number of ice particles is too low (specifically, observations around 6 UTC, and simulations after around 12 UTC). Then, I wonder if the argumentation about increasing rime index with diminishing number concentrations) remains valid?

In fact, a threshold for the rime mass fraction was already applied, based on an ice water content of  $0.01 \text{ m}^{-3}$ . However, to focus on the number of ice particle, the following figure (Figure R1, not included in the manuscript) focuses on the period during the shallow cloud phase when both DESCAM and the observations indicate strongly rimed particles.

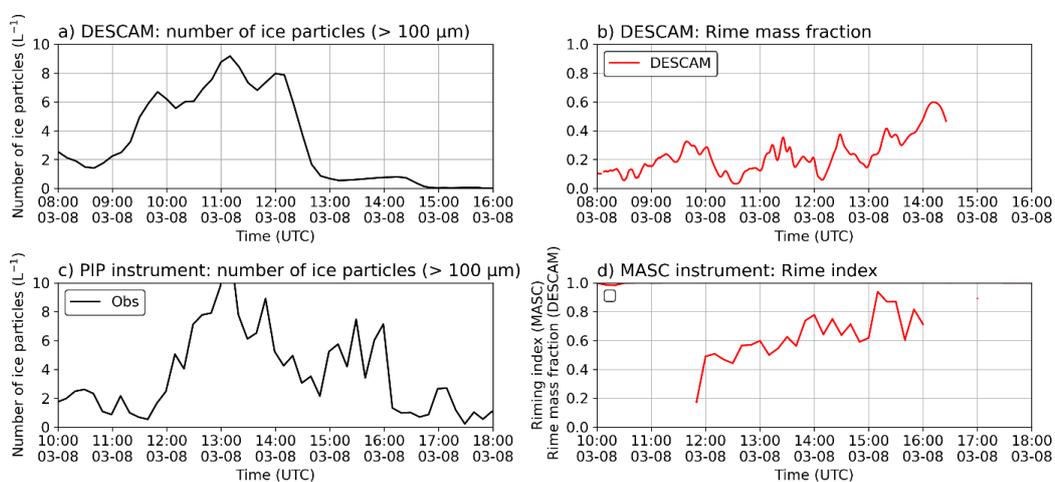
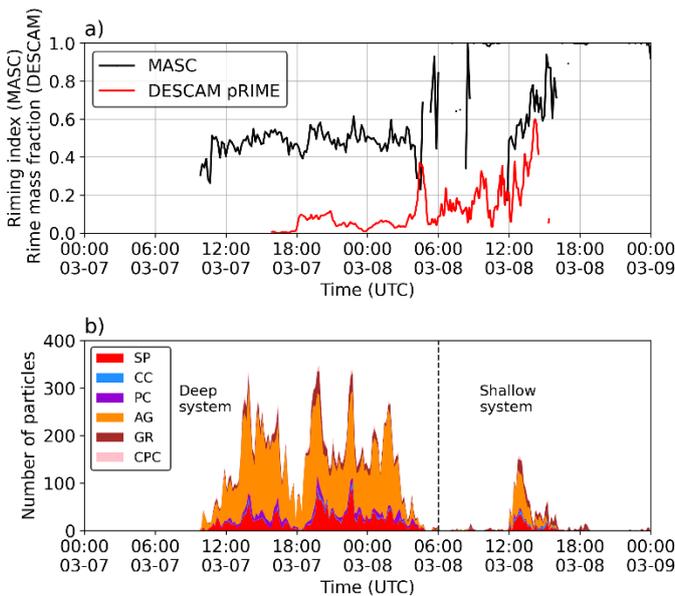


Figure R1: Number concentration of ice particles at ground at and rime index for DESCAM in ab-b and for the MASC instrument c-d.

Figure R1a shows that during the period when the rime mass fraction is highest in panel b), the number concentration of precipitating ice particles is about  $1 \text{ L}^{-1}$ . This concentration is still characteristic for snow precipitation, and we therefore consider it relevant to display the rime mass fraction. After 14:30, the ice number concentration decreases, leading to a drop in ice water content (IWC), and consequently the rime mass fraction in DESCAM is no longer displayed. Nevertheless, a threshold based on ice particle number concentration could also be applied using a value such as  $0.1 \text{ L}^{-1}$ ; however, as shown in Fig. R1b, this would lead to results very similar to those obtained in Fig. R1a using the IWC.

Regarding the observations, the ice particle number concentration measured by the PIP in Fig. R1c remains between 1 and  $10 \text{ L}^{-1}$ , which is also representative of snow and for the analysis of the rime index. However, no threshold on ice particle number concentration of the PIP was applied to the MASC observations. We therefore propose to modify Fig. 15a by applying a threshold of  $0.1 \text{ L}^{-1}$  to both the MASC rime index and the DESCAM rime mass fraction. This threshold seems relevant since the field observations of [Mignani et al. \(2022\)](#) in their Fig 4 pointed out that a threshold of  $0.076 \text{ L}^{-1}$  occurs between snow and no snow events.



With this updated figure, the same conclusions made in the original manuscript remain relevant.

- In section 4.4, I do not understand why these times were chosen (00 UTC, 14 UTC). According to fig. 5, 00 UTC corresponds to an “anomaly” in the deep cloud, when reflectivities are much lower than at other times. And 14 UTC is the very end of the shallow cloud event at MHS (figs. 5 and 14). The discussion about the change in large scale winds probably remains valid, but what about the cloud composition?

The main purpose of this figure is to highlight differences in wind patterns and their impact on cloud composition, particularly on the rime fraction. For this reason, we selected 14:00 UTC (08-03) as it corresponds to the period when the rime mass fraction reaches its maximum, allowing us to analyze the conditions leading to this increase which was discussed previously. The choice of 00 UTC (08-03) is a bit arbitrary and was chosen to illustrate a contrasted situation which corresponds to the deep system, but other times could also be chosen. We didn't pay attention to the fact that 00 UTC (08-03)

corresponds to the exact time when a decrease of radar reflectivity occurs. We therefore propose to update the figure by new panels e) and f) corresponding to 18:00 UTC, which corresponds to a more typical situation of the deep cloud system, characterized by higher radar reflectivity values. However, this does not change our conclusions that we made previously concerning the wind pattern and the contrasted situation compared to the shallow system.

### 3. Discussion

The discussion could be extended. For example, is the fill-in hypothesis valid for all drop/droplet sizes, or is there a dependence on drop size? Since a parametrization of particle liquid fraction and progressive melting is available in DESCAM and mentioned in the article, why was it not used in this study? Possible biases in the aggregation are mentioned, how is aggregation influenced by the predicted rime mass?

We have included Section 5 in the paper which discusses the current perspectives and limitations of DESCAM.

We expect the fill-in assumption to be valid when ice particles are much larger than the collected droplets. However, its validity across different particles and drop size need to be better established through dedicated laboratory studies, which are still lacking to date. In Section 5 validity of the fill-in mechanism is discussed as follows *“while the fill-in hypothesis may apply when ice particles are much larger than droplets, its validity across different particle and droplet sizes remains unknown”*.

Regarding the melting, the reasons why DESCAM does not include anymore a continuous melting treatment were mentioned in our general response at the beginning of the document. The reintroduction of continuous melting in DESCAM is also a future perspective, to be considered after implementing a predicted ice particle density, which will allow melting to be represented more realistically based on particle properties.

The new section 2.5 ‘Aggregation collision kernels’ is added after the section dedicated to the riming collision kernels to describe the representation of aggregation in DESCAM. This new section includes a detailed description of aggregation in DESCAM, sticking efficiency and the underlying assumptions.

#### Minor comments:

- I.59: DESCAM could be introduced before, citing some recent works, instead of appearing here in this sentence giving the article’s purpose.

We agree that more reference to previous works can be included, we have revised the paragraph accordingly (line 59) as follows *“This study aims to evaluate the implementation of a predicted rime mass distribution in the bin microphysics scheme DESCAM (DEtailed SCAvening and Microphysics model, Flossmann and Wobrock, 2010; Planche et al., 2010) based on the approach of Morrison and Grabowski (2010). Historically, DESCAM developments have mainly focused on cloud-aerosol interactions (Leroy et al., 2007) and their impacts on cloud and precipitation properties, including anvil characteristics in deep convective systems (Leroy et al., 2009), marine stratocumulus clouds (Flossmann and Wobrock, 2019), precipitation and drop size distributions (Planche et al., 2010; Kagkara et al., 2020), or even case studies in mountainous regions (Planche et al., 2013; Arteaga et al., 2020). Only a few developments have addressed the ice phase, for example the melting scheme of Planche et al. (2014), which is no longer used due to its high numerical cost. More recently, efforts have focus on the refinement of the ice phase microphysics of DESCAM, with the implementation of secondary ice production processes (Grzegorzczuk et al., 2025a, b, e). In previous versions, the ice particles in DESCAM were simply represented by a single predicted number distribution divided into 39 mass bins. Building on these recent advances, we introduce rime mass prediction in DESCAM to*

*better represent ice particle characteristics in each bin mass (i.e. size, fall speed, and collision kernels)* .”. However, we found it was somewhat difficult to introduce DESCAM model in a paragraph since a general overview of ice particle representation in microphysics schemes is presented. Furthermore, that background helps explain the reasons for the present developments in DESCAM.

- I.81+: give slightly more details about ice nucleation parametrization. Since aggregation is discussed in the article, briefly present its implementation, and should it be modified based on ice particles density? Also, is there a water shedding mechanism for particles experiencing wet growth?

The new version of the manuscript is already quite long, and the primary formation of ice particles is not the main focus of this study. We prefer to simply refer to the study done by [Hiron and Flossmann \(2015\)](#), which provides detailed information on these mechanisms.

Theoretically, higher ice density reduces surface asperities, producing more spherical particles which likely decrease aggregation. However, quantitative information on collision or sticking efficiencies is missing in the literature, which limits how these mechanisms can currently be represented in models. For instance, as presented in our new section 2.5 “Aggregation collision kernels” we account for a reduction in sticking efficiency between  $-10^{\circ}\text{C}$  and  $-20^{\circ}\text{C}$ , where dendritic ice particle branches can be filled by rime, inhibiting their interlocking.

While moving toward a more detailed representation of ice particle properties, it should be noted that the lack of laboratory or observational data on aggregation and riming among different particle types introduces significant uncertainties and limitations. These limitations are discussed in a new Section 5 presented before the conclusion of the manuscript (Section 6).

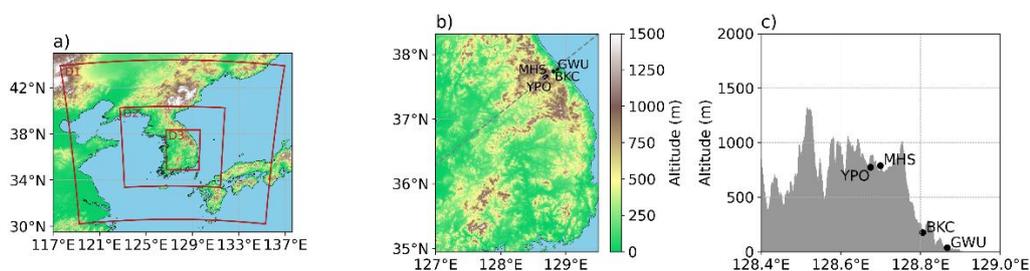
No shedding is currently represented in DESCAM but this is a perspective which is also discussed in Section 5.

- I.92: what type of crystals / clouds were sampled by Fontaine et al 2014?

Fontaine et al. (2014) sampled mid latitude convective cloud systems.

- I.198-206: Since the paper is built around a case study, more information should be provided here. Maybe a weather map? I also missed a cross section, as that from fig. 16, to show the relative altitude of all sites more clearly than on fig. 4.

The manuscript now includes a substantial number of figures, we don't want to make it too heavy, then, we propose to add another panel on Fig 4 (panel c) which better represents the sites on a cross section:



- I.226-236: how many vertical levels do you use? Why do you assume that aerosols are sulfate, so close to the sea? Maybe mention the chosen parametrizations for other processes, such as radiation and turbulence? These can also largely influence the production of liquid droplets and the availability of supercooled liquid water.

We forgot to mention the number of vertical levels, the sentence line 291 is modified to “ *The vertical grid is non-equidistant, with 82 vertical levels ranging from  $\Delta z = 40$  m at ground level and  $\Delta z = 230$  m at 9 km. .* ”

Regarding aerosol particles, this is a prescribed input in the model and therefore requires assumptions. We chose ammonium sulfate as it is representative of marine conditions, consistent with the oceanic origin of the air masses during the ICEPOP case. We therefore modified line 298 to “*We assumed that aerosol particles are composed of ammonium sulfate, as for this case study, the air masses mainly originated from the ocean (the East Sea for the shallow system and the China Sea for the deep system), although some influence from more polluted conditions cannot be excluded* ”

More details about our model have been added in the following paragraph “*DESCAM (DEtailed SCAvening and Microphysics model, Flossmann and Wobrock, 2010; Planche et al., 2010) is a bin micro physics currently included in the 3D cloud scale model of Clark (Clark et al., 1996; Clark, 2003). In this dynamical framework, the horizontal and vertical subgrid scale mixing is calculated using a first-order turbulent kinetic energy (TKE) closure following Smagorinsky (1963) and Lilly (1962) formulation with the Blackadar (1962) mixing length. Surface atmosphere exchanges are represented using a Monin-Obukhov similarity scheme, with daytime-dependent sensible and latent heat fluxes. Our implemented microphysics scheme predicts five distribution functions, each divided in 39 mass bins with namely, interstitial aerosol particles, liquid drops, ice particles as well as aerosol mass inside drop and ice particles*”

- I.237-240: Usually, the “control” simulation uses the original model configuration, and an “improved” simulations shows the new model version. I understand the choice here because the article focuses much more on the simulation using the predicted rime fraction, but it was slightly disturbing at first, and should be changed back if a section of the paper is dedicated to the differences between the two simulations.

Yes the names were not appropriate, and this was also noted by Reviewer #1, we have renamed the numerical experiments: CTRL’ refers to the initial model configuration with rime prediction, while ‘pRime’ refers to the updated model version including predicted rime.

- I.250: Do you need to change the particle properties in the radar simulator depending on the rime fraction? Does it account for attenuation?

The effect of the rime fraction on the radar reflectivity signal arises because, for the same mass, filling in the ice crystals leads to smaller particles. This is visible close to the ground in the shallow system, where rimed ice particles are predicted. While a more sophisticated approach could be applied, it remains challenging to accurately define crystal properties for a radar simulator based on a continuous variation of the rime mass fraction.

- fig. 5 (and in other places): I feel that some information is missing, mostly regarding the temperature. It is essential to have that information, as it will largely influence the availability of supercooled liquid water, ice production (heterogeneous nucleation or secondary production), etc.

We propose to add temperature isotherms and liquid water content to Fig. 16 (Fig. 19 in the revised manuscript), where the cross sections and the ice and liquid water contents are analyzed. This would provide valuable information for interpreting the vertical structure of both the deep and shallow cloud systems. The previous version of Fig 5 is less suitable for this purpose, as it is dedicated to comparisons between the model and radar observations, and no temperature measurements are available there.

- I.274: can this be due to attenuation?

The W-band radar data are corrected for attenuation, so we do not think that this can explain the differences between modeled and observed W band reflectivity.

-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?

To simplify the analysis of the shallow system, we have restricted the observations as well as the simulations to data after 9 UTC. With this update, no signal is present in observations above 2 km.

-l.304: Since you discuss particle sizes (and it is important indeed), it would be nice to have a cross section of some measure of particle sizes, compared between control and no-rime?

We prefer to focus on the particle sizes at ground since there are some ground-based measurements which help to show if the predicted rime implementation leads to more consistent results with the observations. With the new considerations, the paper starts to be heavy, a discussion of the distribution of the drops and ice particles is made in the new experiments of the idealized squall line system.

-l.324-325: delete this sentence (duplicate from the one before)

The repetition is deleted, thank you.

-fig. 9: why are the simulation curves blue and red? Use the same colors for all figures

This was done to distinguish between snow and liquid precipitation stations. We have now updated the figure by using a uniform color for the two simulations.

-l.340: DESCAM is consistent with the observations

We have modified the sentence accordingly.

-l.348: the melting layer is not visible on any of the figures. It would be a nice addition.

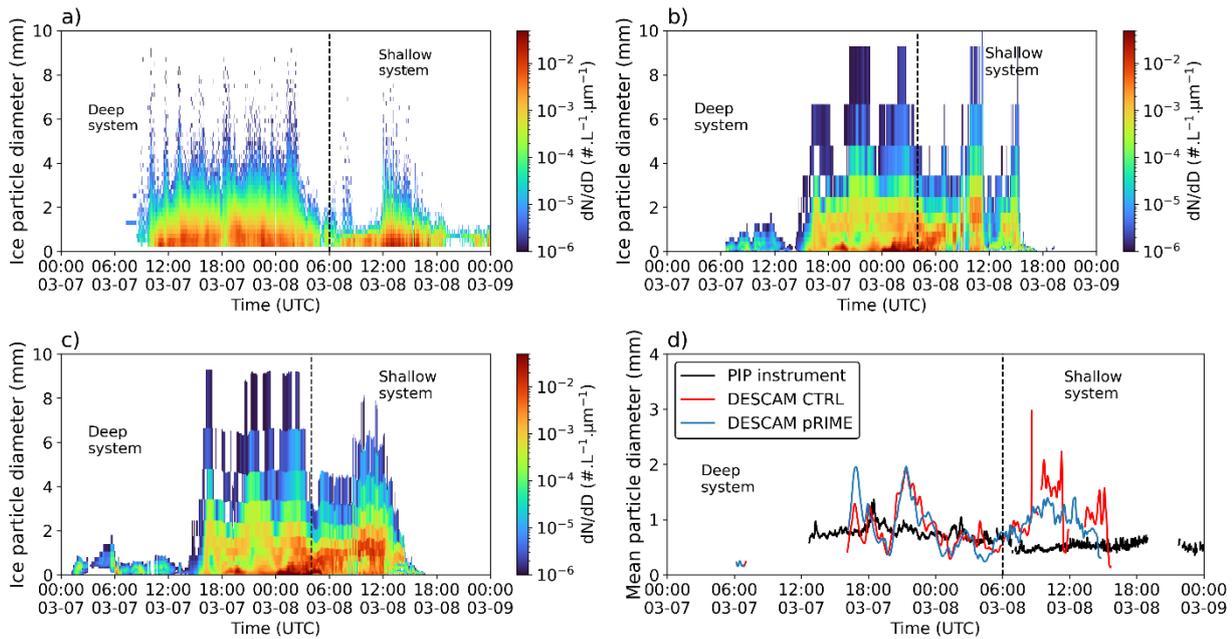
The 0°C isotherm is now presented in Fig. 16 (fig 19 in the revised manuscript) in panels a) and e). This helps to illustrate the presence of a warmer and more humid air mass originating from the ocean that forms the shallow system, in contrast to the deep system.

-fig. 11: Is it really necessary to have 3 panels, as the liquid and ice precipitations are mostly exclusive of each other? Also, maybe merge with fig. 12?

We agree with this suggestion. Figures 11 and 12 have been merged into a single figure presenting only the total precipitation. In addition, observations of precipitation accumulation have been added to this panel, as requested by Reviewer #1.

-l.377: the bimodal size distribution is also apparent in the shallow system. Is it also a feature of the no-rime simulation? Could the numerous small crystals (often largely overestimated when we compare to the observations) result from (secondary) ice production, or is it really due to aggregation as suggested?

With the updated version of the figure 13 (fig 16 in the revised manuscript) we clearly see that it is also apparent in the “CTRL” (i.e. no rime simulation).



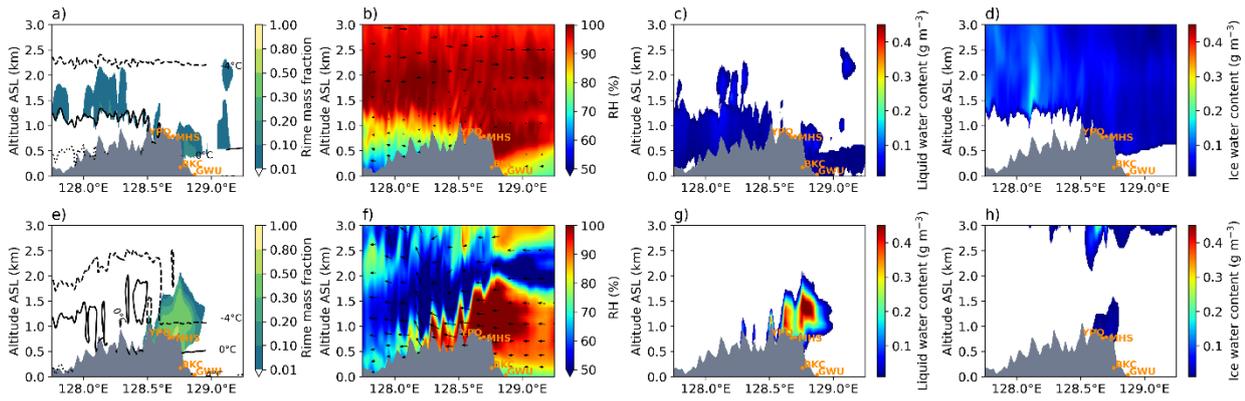
We are not sure what is meant by 'often largely overestimated when compared to the observations.' The secondary ice production (SIP) mechanisms implemented in DESCAM mainly generate ice particles smaller than about  $300 \mu m$ . Therefore, we doubt that the observed feature is related to SIP, since the separation between the two modes occurs at sizes between  $0.5$  and  $1$  mm. In our previous studies, we did not observe any bimodality at such sizes associated with SIP. For example, Fig. 4 in [Grzegorzczuk et al. \(2025\)b](#) shows the evolution of particle size distributions influenced by SIP in a deep convective cloud, where bimodality appears only for small ice particles below  $100 \mu m$  (due to heterogeneous ice nucleation or Hallett-Mossop processes). Consequently, we rather think that bimodality is due to aggregation.

-fig. 14: Can you explain, or try to explain, why the control and no-rime simulations are almost identical until around 2UTC, and then so different? What happens then?

It is a bit difficult to interpret. After 3 UTC, the rime mass fraction shows some peaks, indicating that particles are becoming rimed. This would typically increase the sedimentation flux of ice particles and thus decrease their number in cloud, as observed in our new idealized squall line simulation. Consequently, this may lead to a rise in precipitating ice particle number at ground, as visible in Fig 14.

-fig. 16: the IWC and LWC contours are hard to read. At first, I even thought that there was ice between the two dotted lines in fig.16c, and almost nothing at MHS or above.

We have removed the IWC and LWC contours from Fig. 16. Instead, four additional panels (c, d, g, and h) have been added to explicitly show the liquid and ice water contents.



-1.424: the rime fraction barely exceeds 0.5 at MHS in fig.16c, and does not exceed 0.5 at YPO, if I read the colors correctly.

*Yes that is true, we have updated this sentence by “around the mountainous stations (YPO and MHS) the rime fraction generally exceeds 0.3, with maximum values close to 0 ” in line 564.*