This paper presents a reasonable case study of microphysical evolution within a winter storm supported by ample observations and some well-considered idealized modeling. The authors do a good job at not over generalizing the observation evidence or reading too much into their simulations. I think this paper well highlights the prevalence of sublimation in this winter storm. IMPACTS as a project well showed how common sub saturated conditions really are.

I have concerns that the flight legs from this flight do not really create a good Langrangian view of the band of interest. I also think the idealized microphysical modeling experiments do not investigate variations in riming the way they should. However, because the authors do not over generalize and do not over interpret their evidence, the ultimate impacts are minor, and the investigation of sublimation stands as good and interesting work.

I recommend this paper be published with minor suggested changes that I trust the authors to consider without a subsequent round of review.

Thank you for your comments, which we address specifically below.

Below are suggestions in no particular order:

- The authors highlight the intended semi-Lagrangian nature of the flight sampling for this storm. However, there's an implied assumption of near steady state conditions and consistency in the vertical structure along the storm's direction of motion. There's a deeper implied assumption that the banded feature of interest is contained within the extend of the flight legs. I think volumetric MRMS data or perhaps 3D EXRAD data (if available for this flight) as well as an examination of vertical wind profiles from soundings (or, as a last resort, HRRR or reanalysis) can demonstrate that the storm fits close enough to the implied structure and that shear isn't confounding the analysis.

Your concerns about the Lagrangian characterization of this analysis, as well as similar comments shared by other referees, requires some revision. As you suggest, the Lagrangian characterization implies a steady state, which is an inaccurate description of the case presented in this analysis. Indeed, in our original manuscript, we describe the storm's decay during the period of aircraft sampling. Thus despite a flight strategy that favored sampling in a Lagrangian manner, the nature of the storm was not permissive to a strict Lagrangian characterization. For this reason, we describe the collection of observations as providing semi-Lagrangian context of the evolving particles (lines 193-194 of the original manuscript). Nonetheless, we have provided additional context in lines 205-215 of the revised manuscript to distinguish the limitations of our semi-Lagrangian analysis from any implied assumption of a strict Lagrangian analysis, which is exceptionally difficult to obtain from airborne measurements,

"Obtaining precise Lagrangian observations from temporally and spatially evolving winter storms is exceptionally challenging and, from this strategy, requires that the storm maintain steady state during sampling. However, measurements collected between sampling along flight leg 2 (~14:45 UTC, Fig. 1a) and flight leg 5 (~17:15 UTC, Fig. 1d) occurred as the maximum composite reflectivity within the banded region weakened from 54.5 to 47.5 dBZ, which occurs at the radar bright band due to melting of the particles aloft. The reduced intensity at the bright band suggests a change in the particle properties or the downward mass flux from ice aloft, indicating a decay of storm intensity, consistent with the reduction in precipitation rate during the same time period. Thus, we suggest that flight legs 2 through 6 provided semi-Lagrangian context for coincident in situ and remote sensing measurements of the cloud and precipitation particles during evolution between 5.5 and 3.0 km a.m.s.l."

We greatly appreciate your suggestion to investigate the environmental context for this event using the EXRAD retrieved wind product. Indeed, this would be an excellent asset to this manuscript. However, unfortunately, the EXRAD radar failed during the operations one day prior (03 February 2022)! While we maintain that our analysis should be fundamentally based on the in situ observations collected, other datasets are indeed useful for context. Addressing an additional critique of Referee 3 regarding the assessment of the frontal region, we have revised our original Fig. 2 to include an analysis of the frontogenesis and temperature fields from the 17:00 UTC RAP analysis coincident with the aircraft sampling on flight leg 5. While a small displacement, either temporal or spatial, is found for frontal cloud structure between the RAP analysis and our in-situ derived analysis, our characterization of the northward sloping frontal boundary above the melting layer remains consistent with our analysis.

Considering the effects of sheared surfaces affecting our analysis, operational soundings at Portland, Maine during the duration of sampling provide minimal evidence of directional shear aloft. We provide some remarks on this consideration in lines 300-307 of the revised manuscript,

"Operational rawinsondes launched at 12:00, 15:00, and 18:00 UTC from Portland, Maine (KGYX), which was approximately overflown on flight leg 4, demonstrated a southwest flow with minimal directional wind shear above the frontal boundary (not shown). The lack of directional wind shear aloft suggests that the primary transport of ice particles aloft occurred within the vertical plane sampled by IMPACTS aircraft and minimally through horizontal advection. Thus, we assume that successive collections of observations from descending P-3 flight legs cohesively translated with the storm motion and were minimally affected by horizontal advection along sheared surfaces aloft."

Paragraph starting at Line 164: A map and some time series plots would be of real benefit here. Moreover, the claim that the studied band produced locally high surface precipitation accumulations is never demonstrated to the reader.

A meteogram-style time series was added as Fig. 2 showing the ASOS observations at the Wiscasset, ME (KIWI) site that is situated very near flight leg 4. We have plotted surface temperature, liquid-equivalent accumulated precipitation, and mean sea level pressure to visually highlight our point here.

- The authors stated that the two dominant microphysical processes at play for the bulk of the depth of the storm were sublimation and riming. I think the sensitivity tests for relative humidity were well considered. The variations in vertical velocity mainly influenced the fallout time of the particles and subjected them to more or less time to sublimate. Missing from the sensitivity tests was a variation in riming. The authors stated that they used a fixed LWC value to affect riming. I think that value should be subject to variation as well. The observed vertical velocity distributions (as well as time series of vertical velocity from which those distributions were derived) suggest that upward vertical velocity is brief and suggests that riming should happen in bursts. I don't know if the McCool model allows for time-varying changes in ambient conditions, but I suggest investigating the possibility in order to better characterize the nature and role of riming. If the authors can reasonably fit some riming variations into the manuscript, they should. That said, I think the results of such variations should be obvious to most experts.

In present form, using the McSnow model, the LWC is prescribed and does not vary in time, thus permitting the steady-state outcome of the simulations. However, we agree that riming is a microphysical process that, when subject to varied environments (i.e., ambient LWC), can substantially modulate the evolution of ice-phase particles. Indeed, we directly evaluated the riming process and its sensitivities to ambient LWC for this 4 February 2022 IMPACTS winter event using the McSnow model in DeLaFrance et al. (2024). Notable from that study, we found that riming within a modest ambient LWC of 0.05 g m^{-3} yielded a dominant 55% of the precipitation mass at the surface. Furthermore, we found that the particle morphology during riming substantially modulated radar remote sensing profiles of reflectivity and Doppler velocity. Because riming was previously directly evaluated for this winter event, albeit in a separate region of the storm, we do not feel that a complementary evaluation would yield novel insights consistent with the scope of the present study.

DeLaFrance, A., McMurdie, L. A., Rowe, A. K., and Heymsfield, A. J.: Simulated particle evolution within a winter storm: contributions of riming to radar moments and precipitation fallout, Atmos. Chem. Phys., 24, 11191–11206, https://doi.org/10.5194/acp-24-11191-2024, 2024.

- Paragraph starting at Line 557: The authors are reading too much into the precipitation accumulation numbers here. Remember that a single rain gauge bucket tip is 0.254 mm. The authors state that 4 bucket tips in an hour is more than their simulations and 3 in an hour is more in line when the truth is that their numbers all fall within a rather large ballpark of measurement uncertainty.

You raise a reasonable critique here. While we value the direct observations of precipitation as verification of the model simulation, limitations of the model and, as you point out, limited precision of the observations provide constraints on the strength of this verification. With these limitations in mind, we have revised the text of this section (lines 611-619 of the revised manuscript) to state,

"...the control simulation produced a liquid-equivalent rain rate of 0.66 mm h^{-1}. Surface measurements at KIWI were reported as 1.02 mm during the 17:00 UTC hour, at the time of aircraft overpass on flight leg 5 (Fig. 3) and reduced to 0.76 mm h^{-1} during the following 18:00 UTC hour. To the extent that the control simulation precipitation rate can be compared to the available ASOS tipping bucket measurements with a resolution of 0.254 mm, the simulation is in general agreement with observations. Moreover, any underestimation of the precipitation rate by the control simulation may be a result, in part, of the absence of simulated liquid-phase precipitation formation below the melting level."

- The radar colormaps don't work at all in black and white.

While we appreciate suggestions to improve figures, we are confused on how to address this point. On our end, all the figures showing radar data display that data in color so we are not sure how to address this comment.

- Table 1: Please include the obs for reference.

The median observations, initially provided in the text of Section 3 (lines 376-379), have been included as a new column in Table 2 of the revised manuscript.