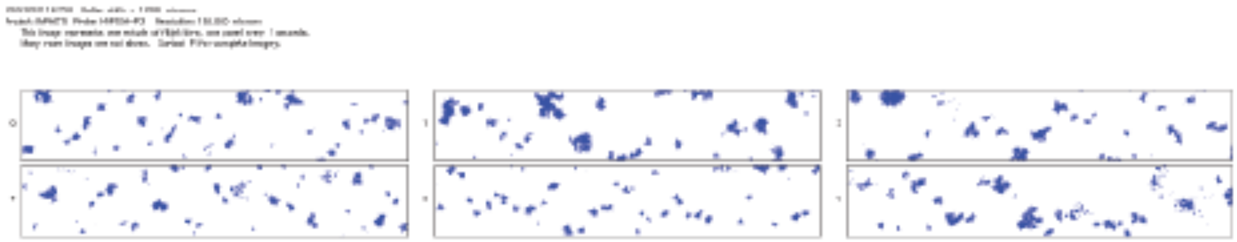
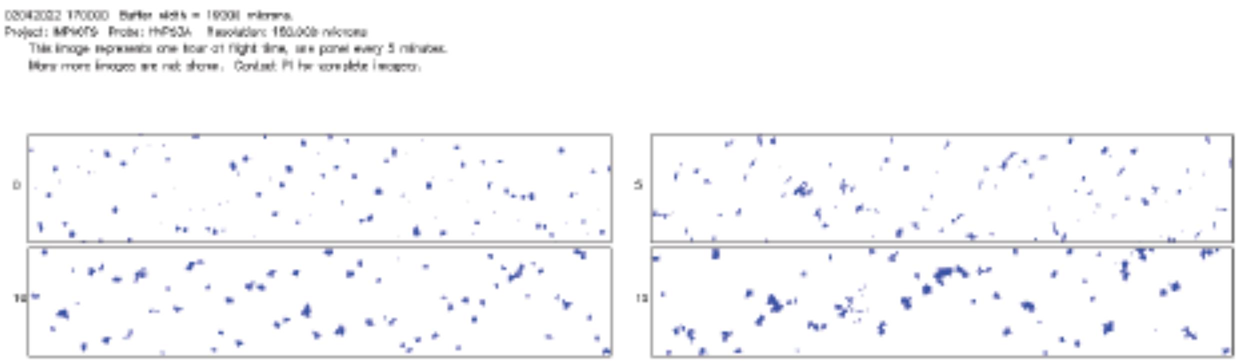
This case study leverages a Lagrangian particle model (McSnow) along with data from an HVPS cloud probe in order to understand the evolutionary pathway of snow properties and various microphysical processes in a midlatitude band. The authors constrain their model using environmental data from the P3 aircraft in situ instruments and the initial PSDs derived from the HVPS. The particular snowband that the authors targeted was subsaturated such that the estimated IWC decreased with decreasing altitudes. This behavior, along with the authors' arguments using PHIPS probe imagery, suggests that rapid sublimation was a driving factor for the decaying band. The authors conclude from their suite of simulations that sublimation provided greater than a 70% reduction of IWC and that small perturbations of RH can vary IWC by as much as 29%. The authors’ figures are simple, high quality, and I think they provide a cohesive and easily understandable narrative regarding their interpreted evolution of this decaying winter band.

I have three overall major concerns with this study that I think the authors need to address before publication. First, I think that there needs to be some discussion regarding how steady the band was during all four legs and how the location of the band changes at each of the P3 leg altitudes. The authors use MRMS composite reflectivity to show the P3 aircraft location with respect to the location of this decaying band. However, fallstreaks are often sloped in these winter storms. I’m not convinced that the composite reflectivity shown in Figure 1 properly illustrates the location of where the band was at each of the P3 levels. This information is quite important because the PSDs can vary quite a bit throughout each level which can be seen from the IMPACTS website quicklook figures; this PSD variability would naturally impact the authors’ constrained modeling results. Second, the authors rely on the HVPS to estimate ice water content (IWC) and the authors assume that the IWC predominantly results from the "prevalence of single crystals" that were "larger than 0.5 mm." The authors state on lines 347--350: "From imagery collected at all heights, relative to the prevalence of single crystals, very few aggregate particles were observed. This near absence of aggregation significantly contrasts with the high prevalence of aggregate particles observed within the southern region of enhanced reflectivity that lacked well-defined banding (DeLaFrance et al., 2024b)." I took a look at some of the example HVPS images and distributions available for the 04 Feb 2022 IOP on the IMPACTS 2022 website available publicly at https://catalog.eol.ucar.edu/impacts_2022. I don't really agree with the authors that aggregates were not common during these short time periods. You can see various pockets of moderate (mm sized) and even large sized (approaching cm sized) aggregates for each of these regions in the example HVPS distributions and images available on the IMPACTS website. It seems like there were moderate to large aggregates at some time periods such as 1617 UTC and 1710 UTC. I've added some HVPS buffer strips below to illustrate this.





These aggregate regions also seem to show up in HVPS distributions themselves from what I can tell from the available figures on the IMPACTS website. While I don't think that the presence of aggregates necessarily changes the authors' results or interpretation, I do think that the presence of aggregates as seen in the HVPS buffer strips should be mentioned and some HVPS strips should be shown as a figure. Finally, I also think that the authors need to demonstrate that the 2D-S is not needed here to fully resolve the ice water mass distribution. I suggest that the authors utilize a separate IMPACTS case such as the 07 Feb 2020 IOP to investigate how much ice mass, on average, would be from particles smaller than 0.5 mm. Since many of these winter systems have bimodal particle distributions, it’s possible that the IWC values from these smaller size particles are appreciable and could change the interpretation of the McSnow sensitivity tests. The authors could easily calculate IWC using the HVPS and the combined 2DS-HVPS size distributions (which are already available online) for another IOP.

Thank you for the careful review of our manuscript. You provide three main criticisms of this analysis.

1. You raise concerns about the characterization of Lagrangian sampling of this storm. We recognize these concerns and, in our original manuscript, made attempts at careful wording regarding any Lagrangian sampling. In introducing the storm and flight strategy, we explicitly characterize the observations as having *semi-Lagrangian* context, stated in in lines 193-194:

   “This Lagrangian strategy requires that the storm maintain steady state during sampling. Thus, we suggest that flight legs 2 through 6 provided semi-Lagrangian context”.

Moreover, we noted that the storm is not in a steady state but is decaying in intensity during sampling. For example, lines 72-73, 198-201, 417-419, 817-818 and in the title of the manuscript, we describe this as a *"decaying winter storm"*.

Nonetheless, addressing your comment and those from the other referees, we have revised the text in lines 205-215 to provide additional clarity on our semi-Lagrangian characterization of this storm.

"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."

In addition, we find minimal evidence that the storm system sampled was substantially affected by the effects of shear. Rather, flight legs translated northeast within a cohesive system advected by southwesterly flow aloft. Remarks on this characterization of the storm system are provided 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."

By design, the McSnow model has Lagrangian properties. However, we apply this Lagrangian model to observations of this case study in a semi-Lagrangian manner. We provide additional clarity on this distinction in lines 499-504 of the revised manuscript:

"we use the Lagrangian particle-based McSnow model" … " Constrained by the semi-Lagrangian in situ and remote sensing measurements, this McSnow model permits simulations of the likely evolution of a population of ice crystals."

2. Your second point regards the use of HVPS imagery strips, and our assessment of the aggregation process. We appreciate your evaluation of this IMPACTS data, which was an oversight in our analysis. We agree that remarks on the particle habit variability inferred from the HVPS image strips are warranted in this manuscript. However, we do not think that these details justify the inclusion of an additional figure owing to our focus on sublimation. To acknowledge your point, in lines 380-384 of the revised manuscript we have added a remark on the HVPS image strips and updated the corresponding description of aggregation throughout this cloud:

   "However, transient occurrences of aggregate particles were evident among HVPS images collected on flight legs 2 through 5 (not shown). This modest evidence of aggregate particles differs from the high prevalence of aggregate particles observed within the southern region of enhanced reflectivity that lacked well-defined banding (DeLaFrance et al., 2024b)."

3. Your third point regards the use of HVPS-only OAP imagery to derive IWC. An oversight of our initial manuscript was any implication that these measurements describe the total IWC. Although we expect that a significant fraction of the total IWC is described by the HVPS measurements, as you point out, we do not show this. While we appreciate the suggestion to compare the relative IWC contributions derived from HVPS or 2D-S size ranges from another IMPACTS event, such as 07 February 2020, we have concerns about drawing such relationships, given the inherently unique properties of PSDs which may significantly differ between diverse storms. To address your valid critique, we suggest a resolution that more accurately describes the IWC throughout the manuscript as the partial IWC among the particle population of D > 0.5 mm. This is consistent with the original analysis, and therefore, does not change any of the numerical values describing IWC, but is more clearly stated as such. Correspondingly, we have revised the text in lines 373-376 of the original manuscript to now state (lines 407-411):

   "Because measurements of ice at small particle sizes are unavailable for the 4 February event, the total ice water content (IWC) is not completely defined by measurements from the HVPS. Thus, throughout the present study, we define the characteristic IWC as the IWC computed at D > 0.5 mm, applicable to both observations and model simulation output."

Specific concerns/comments:

- Figure 1: The authors use composite reflectivity from the MRMS product here. However, wouldn't it make more sense to use the MRMS reflectivity at levels corresponding to the rough P3 altitudes for each leg? I don't think the composite reflectivity is really appropriate if we are to be convinced that the P3 is truly following a decaying winter band in a Lagrangian or semi-Lagrangian sense.

We appreciate your concerns regarding the best radar reflectivity product to describe a mesoscale band. We suggest that a clearer description of the intention of Fig. 1 in this manuscript was needed. Here, we are not defining a mesoscale band from WSR-88D composite radar reflectivity. Rather, we use the WSR-88D composite as illustration of a prominent mesoscale feature within this winter cyclone. Presented in a two-dimensional form in such a way to be consistent and comparable across several times during aircraft sampling (i.e., Fig. 1a-d), we suggest that composite reflectivity is appropriate. As discussed in Section 2.3, the bounds of the observational analysis are derived from the ER-2 observations. Thus, those methods are unique to the collection of observations available for this event. We have revised wording in attempts to provide clarity on the intention of Fig. 1 in lines 169-172 of the revised manuscript

"Composite radar reflectivity from the NWS MRMS product shown in Fig. 1 provides spatial and temporal context of the intended sampling objectives of IMPACTS flights relative to the prominent banded enhancement elongated southwest-to-northeast near the Gulf of Maine coastline."

- I like how the authors show PHIPS images for each leg. However, it would be better from an analysis perspective to show the HVPS buffer strips as well throughout each leg. This would be a better way to demonstrate whether or not the particles were generally unaggregated and also whether the HVPS is good enough to resolve the mass distribution. Also, the authors should consider using CPI images as well as those were also available throughout the IOP.

Please see our response to the similar remark on the HVPS imagery made in your major concerns. We do not see the HVPS image strips as having sufficient value to warrant an additional figure in this manuscript. However, we have added a comment on the utility of this data, in comparison with the assessment made from the PHIPS imagery. It is also not clear that this manuscript would benefit from CPI imagery similar to that already shown from the PHIPS, which has substantially greater image quality. Therefore, we prefer to maintain the single figure of particle imagery from the PHIPS instrument.

- Section 4: It isn't obvious to me how McSnow was configured. The authors say that the model is a "columnar model" where there were 500 grid cells. This is giving me the impression that the model is 3D and that each grid cell represents some 3D column in space. However, I think this is actually referring to the vertical spacing alone. The authors should specify here what they mean by each grid cell.

Indeed, the model description in this manuscript is brief which is because, as stated in lines 456-457 of the original manuscript, “Model selection and its design follow DeLaFrance et al. (2024b).” Therefore, we do not feel it is necessary to repeat many of these details in this paper, except where they notably differ from the prior study. However, to address your critique, we revised the text in lines 479-480 of the original manuscript to now include the following statement in lines 522-526 of the revised manuscript,

“In McSnow, modeled microphysical processes are largely independent of Eulerian grid constraints (see the discussion in Brdar and Seifert, 2018, Sect. 2). However, a gridded columnar domain provides a suitable framework for analysis of the evolved bulk particle properties. As in DeLaFrance et al. (2024b), we specify a column of 500 grid cells…”

- Table 1: It wasn’t clear at all to me what the bracketed percentages represent. I tried reading the caption and the text and I couldn’t find that information. From the text, I was able to infer that this probably refers to the change from the control simulation, but this should be explicitly stated in the table caption.

Thank you for identifying this oversight. The caption for this table (Table 2 in the revised manuscript) now includes a statement explaining the bracketed values,

“Percentage differences in IWC from the control simulation are provided for each perturbed-state simulation in brackets.”

Suggestions:

- Figures 1,2,3: It would be really beneficial if the authors provided the times as well, in addition to the along-track distances. I like to utilize the NASA IMPACTS field catalog to verify some of the authors' claims and so I can understand the authors’ arguments a bit better. For example, readers could be pointed to the IMPACTS website for additional images from, for example, the CPI measurements. However, it wasn’t clear to me throughout what the time periods were in each figure. I think that the start/end periods are represented in the first and last PHIPS image panels in Figure 4 but this wasn’t stated anywhere in the text from what I can tell. The authors should provide these time details somewhere in either the text or the figures.

This was an oversight in the original manuscript. For both aircraft, we have added the start and end times for each flight leg in a new table (Table 1 of the revised manuscript) and the following comment in lines 356-358 of the revised manuscript, *"A summary of the start and end times for ER-2 and P-3 flights along each flight leg traversing the objectively assessed, banded region of cloud is provided in Table 1."*

flight leg	ER-2 start	ER-2 end	P-3 start	P-3 end
leg 2	14:50:46.5	14:54:16.0	14:48:35	14:54:17
leg 3	15:35:12.0	15:40:41.5	15:34:41	15:42:13
leg 4	16:17:47.0	16:24:40.5	16:11:25	16:23:23
leg 5	17:08:13.0	17:16:11.5	17:04:33	17:16:49

Also note that in addressing this suggestion we identified an error in our time window that the example PHIPS imagery was obtained, for flight leg 2 only. Consequently, Fig. 4 of the original manuscript and minor sections of the accompanying text in Section 2.3 have been revised to correct this error. Our interpretation of the data and our analysis were not affected.

- Figure 8: I think the authors should make the histogram stairs dashed or dotted for their simulated particles less than 0.5 mm. This would help readers better compare panels a and b while maintaining the simulated PSD at smaller sizes.

We think this is a good suggestion. Because of the "stair-step" plotting style used, a dashed or dotted line is difficult to distinguish from a solid line. However, we have revised the figure applying transparency to the simulated profile at sizes below 0.5 mm. This revised plotted style should permit a more direct comparison between the observations and the simulation.