Authors present a case study and associated microphysical modeling study of a case during IMPACTS. This study presents an interesting look at a decaying storm, and how structures – particularly enhanced reflectivity/banding – is impacted by an otherwise dying system. The article is generally well-written, and represents a worthwhile contribution to the body of knowledge. I have a number of issues with the article, that I think addressing would make the article stronger.

The major issue I have is with the overall storytelling of the paper. There are some parts of the paper that jump around, for instance going from lit review, to describing the case, and then back to the lit review. In addition, I think the modeling portion of the paper could use some more tying back to the observations portion. For instance, there's a lot of sensitivity plots (e.g. Fig. 11) for model runs. While the "control" should be close to observations, it would be helpful to ground some of these plots with observations where possible.

Thank you for pointing out this order. There were several iterations of this original manuscript in deciding the best order to tell this story. We had opted to introduce the banding of this case within the introduction of the paper as to fit into this story: introduce banding, talk about it's presence in a case study, understand why then it might be important (from a literature perspective) and what remaining challenges exist that broadly motivated IMPACTS and then specifically our study. We did this because, in the latter part of our introduction, we wanted to highlight the work in our earlier paper that studied another part of this storm that wasn't banded that motivated this comparison. However, it did not make as much sense without previously introducing this case in including a banded area as well.

Regarding tying the model simulations back to observations, it is not clear that there is a specific area of this manuscript where additional observational evaluation is warranted. The foundation of our model runs is observational constraint, from both in situ and remote sensing observations. We prescribe constraints on the initial ice crystal population and the ambient environment in which they are evolved. The simulated "control" PSD is compared to the observations in Fig. 9 and described in the supporting text. Furthermore, the simulated "control" radar reflectivity is directly compared to the observations in Fig. 10 and discussed in the supporting text. Only after we establish the agreement between the "control" simulation and the observations, do we make the comparison of the sensitivity simulations to the "control" simulation. The grounding of the sensitivity simulations is already established by comparison with the "control".

It would also be helpful to think a bit about how the assumptions of the Lagrangian framework are or are not met - are the individual layers moving with the system as a whole? Are the particles sampled in the top leg substantially the same ones sampled in the lowest leg? If you don't capture the same layers/particles, does it matter?

Similar concerns about the Lagrangian framework for this analysis were shared by other referees, highlighting a need for additional clarity. To collect airborne in situ or remote sensing observations from an evolving and translating midlatitude cyclone that adhere to a strict Lagrangian characterization would be exceptionally difficult, or perhaps, impossible. Our original manuscript omitted a clear description of the Lagrangian properties of the measurements available for this case. Nonetheless, we maintain that, as constrained by our analysis, these measurements are a unique collection of data based on temporal and spatial continuity that permit inferences of particle evolution.

Addressing your concern, we have clarified the text in lines 205-215 of the revised manuscript to more clearly describe the semi-Lagrangian framework for this analysis and distinguish our use of the data from a strict Lagrangian analysis,

"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."

Line-by-line comments below:

Ln. 27-28: Are bands a radar feature? That's usually how they are defined. But then they can't produce precipitation – they correspond to intensified precipitation rates.

The word "produce" was replaced with "corresponding to".

Ln 34: remove "recently" – it may not be recent to the reader.

The word "recently" was removed from this sentence.

Ln 52-55: These are a lot of words that may or may not be true, but don't really say anything of value.

The text of these lines was removed.

Ln 58: Are bands always readily diagnosed? You provide one definition, but there are others. Automated band detection algorithms are relatively recent (e.g. Fairman et al. 2016), as the problem has been fairly tricky.

The phrasing of this section was revised to state (lines 50-53 of the revised manuscript),

"These mesoscale bands may be diagnosed from radar reflectivity measurements … For example, Novak et al. (2006) defined a mesoscale band as…"

Ln 61-65: Are you predicting or diagnosing? Different problems.

Here, we are motivating the significance of these events by referencing numerical weather prediction for high-impact precipitation events. The wording in this section was revised slightly to provide clarification, explicitly stating numerical weather prediction.

Ln 66-81: This is out of place, surrounded by lit review

Please see our response to your general comment on the "overall storytelling" of this manuscript. We suggest that the background provided in this section is necessary to introduce the winter storm event broadly in terms of our previous paper and discuss why mesoscale banding is important, giving support to our motivating science questions. For this reason, we prefer to maintain this section as submitted in the original manuscript.

Ln 100 and elsewhere: "natural particles" are mentioned several times; is this supposed to be in opposition to cloud seeding or something? Is this setting this paper separate from SNOWIE findings?

"Natural" was a poor word choice used throughout the text. We have removed this word throughout. In several cases, "natural" was replaced with a more accurate descriptor of "observed".

Ln 104-114: Again, a description of the campaign seems out of place sandwiched in the lit review.

Please see our response to your general comment on the "overall storytelling" of this manuscript. Given the nature of this analysis, some discussion of the IMPACTS campaign is necessary in this introduction. Here, our IMPACTS introduction is "sandwiched" between background on remote sensing radar methodology and numerical modeling analysis techniques. However, bridging these areas of research was a stated guiding objective of the IMPACTS mission. From McMurdie et al, (2022), see Fig. 3 and the corresponding text stating science goal #3, "apply this understanding of the structures and underlying processes to improve remote sensing and modeling of snowfall". In our view, this is an appropriate introduction to the IMPACTS mission, broadly.

McMurdie, L. A., Heymsfield, G. M., Yorks, J. E., Braun, S. A., Skofronick-Jackson, G., Rauber, R. M., Yuter, S., Colle, B., McFarquhar, G. M., Poellot, M., Novak, D. R., Lang, T. J., Kroodsma, R., McLinden, M., Oue, M., Kollias, P., Kumjian, M. R., Greybush, S. J., Heymsfield, A. J., Finlon, J. A., McDonald, V. L., and Nicholls, S.: Chasing Snowstorms: The Investigation of Microphysics and Precipitation for Atlantic Coast-Threatening Snowstorms (IMPACTS) Campaign, B. Am. Meteorol. Soc., 103, E1243–E1269, https://doi.org/10.1175/BAMS-D-20-0246.1, 2022a.

Ln 168-170: I don't understand the point here. Are you saying a banded snow project selected a band to sample? Would something else ever be selected?

Here, we make the point that the priority for sampling this event was the banded region because in the introduction we discuss a separate region of the storm that was previously analyzed that "lacked well-defined banding". We have added the following sentence in lines 168-169 of the revised manuscript to provide further clarity,

"Therefore, spatial and temporal coordination of sampling from the two IMPACTS aircraft was prioritized near the banded region."

Ln 171-178: How many of these sites are human-augmented? P-type observations from ASOS depend on that – for instance, unaugmented sites can't detect ice pellets.

Also, what is the point of discussing surface p-type? Does it matter to your conclusions? Bands are features aloft.

Given the valid concerns you have raised, we have removed the discussion regarding precipitation type from this section. Consequently, because the KBGR and KPWM ASOS stations were only referenced in discussion of their precipitation types, we have removed these ASOS site markers from Fig. 1, simplifying this figure.

Ln 182-184: Were the passes sampling the bands calculated to be truly Lagrangian – that is, was the actual band/storm motion used to select the location of the next flight leg? Or is it quasi-Lagrangian with just an estimate of motion used/were legs constrained by things like ATC?

Unfortunately, this level of specificity in the flight planning is not known to us. So that our statement is not misleading to suggest that the intent of the IMPACTS mission scientist is expressly known, we have revised this sentence to better reflect that the flight plan (regardless of intent) yielded a semi-Lagrangian sampling favorable for our analysis. Lines 185-187 of the revised manuscript state,

"IMPACTS executed a flight strategy consisting of six flight legs in a "lawnmower-style" arrangement oriented orthogonal to the band that resulted in semi-Lagrangian aircraft sampling from approximately 14:00 to 18:00 UTC."

Ln. 191: Reflectivity maxima in the along-track direction, right? Not the most intense part of the band in the along-band direction? And this is presumably on a composite reflectivity image, right? Not intensity at a particular altitude?

Correct, in the along track direction for composite reflectivity. The wording of this section was revised to clarify.

Ln 202-205: In winter, changes in reflectivity can also be due to brightbanding. Do you know that the drop in reflectivity is due to weakening and not thermal profile changes (e.g. the freezing level moving relative to radar coverage)?

Also, those are very high reflectivites for winter outside of convection!

Indeed, the maximum in composite reflectivity occurs at the bright band. This is evident in Fig. 2 of the original manuscript. These bright band properties may be affected by varied properties and processes. However, it is associated with the downward flux of ice through the melting level and does provide an indication of decay. Addressing your point, we have revised the text in lines 208-213 of the manuscript to provide additional clarity,

"...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."

Ln 214: It would seem that the prior IMPACTS-specific text intermixed in the intro would fit better here.

Please see our response to your comment on this referenced text and our general comment on the "overall storytelling" of this manuscript. We prefer to maintain a brief background on the IMPACTS mission in the introduction of the paper, which we suggest is important to provide appropriate motivation for our science questions.

Ln 230-232: You have the data you have. But what does this mean for the reliability of measurements you wanted to use the 2D-S for? If the HVPS were reliable in the 2D-S' size range, why have the 2D-S at all?

A similar concern was raised by Referee 2 regarding the use of HVPS-only OAP imagery to derive IWC. In this study, we do not attempt to use the HVPS at the 2D-S size range. Rather, we are working with the data we have and using a partial PSD described by the HVPS measurements of the particle population with D > 0.5 mm. Addressing this concern, we have revised the text pertaining to quantitative estimates of IWC throughout the manuscript to be more accurately described as the partial IWC. This is consistent with the original analysis, but adds clarity based on the limitations of the data available. Lines 407-411 of the revised manuscript provide this clarification,

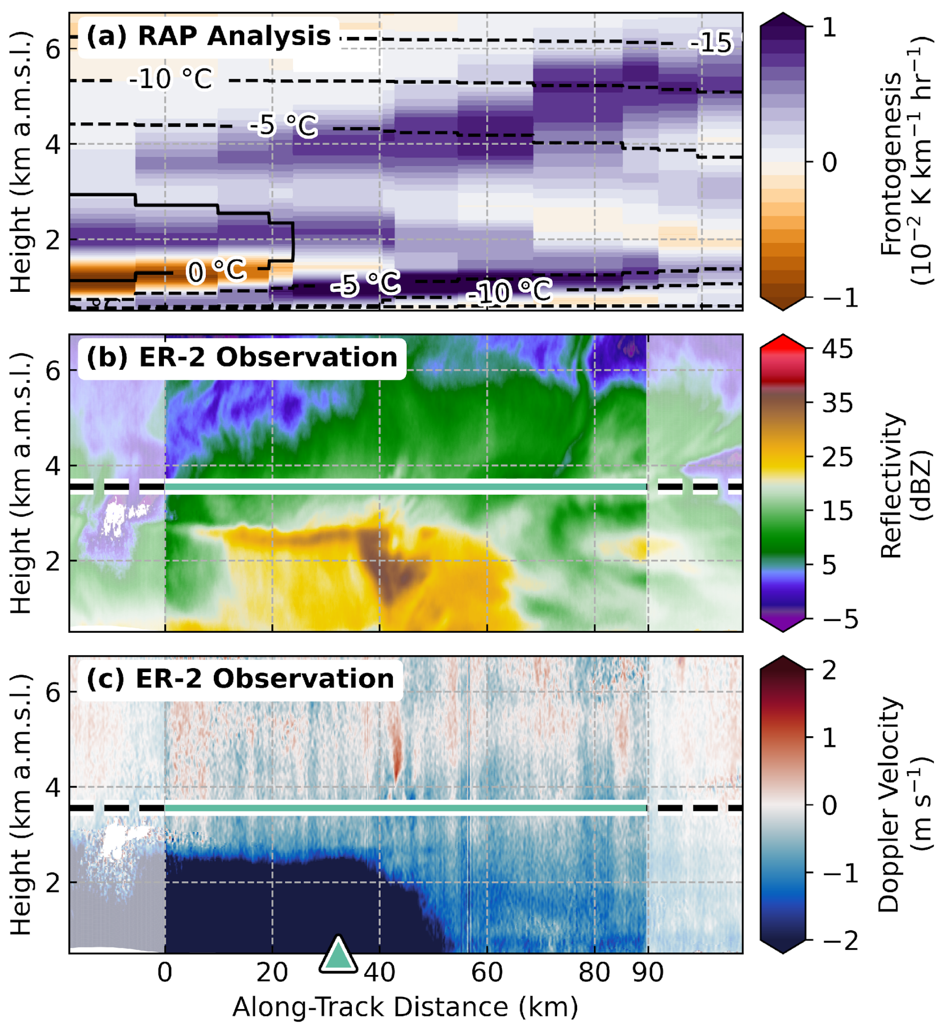
"Because measurements of ice at small particle sizes are unavailable for the 4 February event, the total ice water content (IWC) is not completely defined by measurements from the HVPS. Thus, throughout the present study, we define the characteristic IWC as the IWC computed at D > 0.5 mm, applicable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

Ln. 283-285: Does the frontal position correspond exactly to the freezing level?

The discussion about the freezing level at the surface was initially included here with context of the changing precipitation types at the surface in Section 2.1. Per your earlier critique of the unnecessary discussion of surface precipitation type in this analysis, that context was removed. Similarly, we have also removed the text specific to the surface characteristics in this section.

Ln 308:313: You’re discussing a frontal boundary quite a bit, but don’t actually show where you are saying it is. Can you add your analysis on Fig 2 or a new figure here?

Although we did calculate frontogenesis at flight level from the in situ data, which was shown in Fig. 3 of the original manuscript, it is difficult to interpret the spatial context from these data. Adding information about the frontal boundary to the original Fig. 2 is a good suggestion. To address your concern, we have included a plot of frontogenesis and ambient air temperature from the 17:00 UTC RAP analysis following the ER-2 position as a new panel to Fig. 3 of the revised manuscript. The sloping frontal boundary aloft is evident in the frontogenesis maximum, which is consistent with our analysis from the in situ observations. There is small displacement in the frontal maximum assessed from the RAP analysis relative to the in situ analysis, which may be a result of either spatial or temporal differences, however, this similarity gives useful context supporting our in situ analysis.



Ln 314-315: Is this a coincidence? I'm not sure what you're saying here. There is a maximum at the north? Or the north end was chosen because of the maximum?

The latter suggestion was our intended message. We have revised this text in lines 329-330 to now read,

"To constrain our analysis to the air mass trailing the sloping front aloft, we define the northern extent of the horizontal observational domain by a frontogenesis maximum."

Ln. 319-326: I like this analysis. However, I do need to ask: how steady-state is the aircraft motion? That is, when you calculate derivatives, are your measurements actually on the same x/y plane – or close enough? I think 1 Hz measurements take care of it, and I don't think the underlying fields vary that much even if turbulence change your altitude.

These are constant altitude flight legs with a standard deviation in altitude of 5 m (leg 2) to 10 m (leg 5) and steady heading. While uncertainty in the frontogenesis calculations derived from these 1 Hz data due to aircraft motion is possible, there is considerable noise in the calculation from inherent variability in the ambient environment. However, such variations should not affect this analysis since we only assess the frontogenesis after applying a substantial 60 second smoothing to the computed values. This 60 second smoothing was stated in lines 311-312 of the original manuscript.

Ln 329: I think I get it – "observational domain" here is the analysis domain for this work, not observational domain for IMPACTS. But when talking about a field campaign, observational domain often means the latter.

The phrase "observational domain" is first used in lines 303-304 of the original manuscript, where we refer to the "observational regions indicated in Fig. 1". Rather than introduce a different naming convention for this purpose, we have revised this sentence to state "observational regions indicated in Fig. 1 by different colors for each of the four flight legs".

Ln 332-334: Why these points vs other points in the leg?

We use all points across the flight leg. This sentence was revised for clarity and in lines 351-253 of the revised manuscript now reads,

"All further in situ observations used for analysis in this study were obtained from within the complete segments spanning the flight-leg specific cloud-edge-assessed southern points and the frontal-boundary-assessed northern points."

Ln 344 (and elsewhere): Are the observed habits unusual for this temperature? If so, what does this mean? If there's nothing truly remarkable or unique, avoid those words in formal papers.

The word "remarkable" was removed.

Ln 352: Presumably RH w.r.t. liquid water? Just checking since the paper is discussing ice phase processes.

Yes, correct. We have revised this sentence to state "with respect to liquid water".

Ln 352-353: Advected from other higher RH areas outside the flight track perhaps?

This is a hypothesis that is difficult to test with the measurements we have available. We do not address this point directly here in the text, but we have provided a comment on a similar effect towards your point in our overall remarks at the end of this section, in lines 482-487,

"Intermittent updrafts may have provided a source of supercooled liquid water within the subsaturated ambient background environment. It is further plausible that supercooled liquid water transport occurred through horizontal advection. However, because of the minimal evidence for directional shear aloft, it appears more likely that supercooled liquid water transport, and more importantly, particle evolution and fallout occurred within the vertical plane sampled by aircraft."

Ln 359-360: Is this the southern region in this study? You may want to name your regions and indicate them on a figure.

This was a helpful suggestion. The differing references to "southern" regions throughout the text were confusing. Here, and elsewhere when referring to the region evaluated in our previous study, we have revised the text to state "offshore" region. The figure appropriate to include such labeling would be Fig. 1 which is already a rather busy figure. Because we reference this separate offshore region infrequently we would prefer to not further crowd this figure with additional labeling. We hope the text revision provides sufficient distinction between the two regions for the reader.

Ln 369-370: This is basically stating the next section header.

This text was removed.

Ln 375: Liquid equivalent precipitation rates, right? Snow rates depend on more than mass.

Correct. We have revised this sentence to state "liquid-equivalent precipitation rates".

Ln 378-380: These are presumably IWC values averaged over a certain time frame (the whole leg), right? That's where the distribution in Fig. 5 comes from? Would shorter averaging change the story for parts of the legs?

Yes, this is correct. We have made a minor revision of this sentence to state,

"ice mass PSDs from all HVPS measurements of particles with D > 0.5 mm collected during P-3 flight legs 2 through 5."

While it is possible that averaging over a shorter period would yield quantitative changes, the motivation for our analysis was to identify the principle microphysical pathways that describe the particles evolving within the cloud associated with the mesoscale banded radar feature. Towards this objective, we find it appropriate to include the largest data sample available from our objectively-identified relevant region of cloud.

Ln 386-388: Probably should have a cite here to support this claim, particularly in light of trying to use HVPS to get around the lack of 2D-S.

This specific text was removed and this intent clarified in lines 407-411 of the revised manuscript to state,

"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 fraction of IWC computed at D > 0.5 mm, applicable to both observations and models simulation output."

Ln 414: Is it really unique?

In our view, the concurrence of sublimation and riming within a winter storm is unique. However, we have revised the word "unique" to "notable".

Ln 437-439: Did you sample these supersaturated environments? Do you think these SLW drops are being advected in from off the cross-section? If you didn't sample them, where are the supersaturated environments?

No, we do not have strong in situ evidence for determination of the source region of supercooled liquid water. We have revised our wording of this section to offer the updrafts as one plausible explanation of the supercooled liquid water. We further provide a comment on the potential for lateral advection, which we suggest as a less probable explanation for the supercooled liquid water given the minimal evidence of directional wind shear aloft. The following text was added in lines 482-487 of the revised manuscript,

"Intermittent updrafts may have provided a source of supercooled liquid water within the subsaturated ambient background environment. It is further plausible that supercooled liquid water transport occurred through horizontal advection. However, because of the minimal evidence for directional shear aloft, it appears more likely that supercooled liquid water transport, and more importantly, particle evolution and fallout occurred within the vertical plane sampled by aircraft."

Ln 455-456: If the uncertainty in the instrument is 0.2 m/s, these values are indistinguishable from zero – that is, the sign (ascent/decent) is unknown, right?

This is correct, however, in the absence of a more precise measurement, we are dependent on the data available to us. Addressing your point, we have included a comment on this limitation in lines 477-479 of the revised manuscript,

"the ambient median wind field was characterized by weak descent with w = ~ -0.06 to ~ -0.10 m s^{-1}*, which is within the estimated uncertainty (of 0.2 m* s^{-1}*) of the TAMMS instrument."*

Ln 460-461: Did you measure this colocation of updraft and supersaturation?

This was not found by our analysis. Thus, the text was revised to offer this as a plausible explanation for the presence of rimed particles, and therefore, SLW, within these flight legs. The following sentence in lines 460-462 was revised to state,

"It is plausible that supercooled liquid water droplets were formed in locally supersaturated environments outside of the observationally-constrained domain of our analysis, such as regions of sustained updrafts."

Ln 496-499: Is there a chance that an important layer (e.g. sloping frontal inversion/theta-e max) is washed out with this methodology of averaging and interpolating?

Of course, this possibility exists. However, the motivation for our carefully constrained selection of data described in Section 2.3 was that the observations used in the analysis are from a cohesive region of the storm. Indeed, avoiding averaging over important layers, such as a frontal inversion, is the reason we invested the effort to directly assess the frontal boundary from the in situ observations. Consequently, our observations are constrained behind this important boundary. The addition of the RAP analysis of the frontogenesis and temperature fields to Fig. 3 and the supporting text in Sections 2.2 and 2.3, which was motivated by your prior suggestion further supports that we are sampling a cohesive region of cloud that was not significantly affected by embedded important layers.

Ln 743-745: Given the slow V_t, do we know that the generating cell particles are the same ones in the band, or are they advected elsewhere during their long fall?

Although the effects of horizontal advection cannot be expressly quantified by the available observations, there is minimal evidence that the measured particles were sampled within an environment subject to substantial lateral shear. Conversely, the sampled flight legs translated along the mean southwesterly flow aloft within an apparent cohesive system. 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."