

Major comments:

- #1 The central result is that L-band coherence is lower over flooded areas, attributed to a high-dielectric slush layer forming a scattering interface at a new height (Sect. 4.1, Fig. 12). The proposed mechanism is physically reasonable, but I would like to see the alternative explanation addressed more fully than the brief acknowledgement at lines 514–516.

Areas that flood are areas of low or negative freeboard, that is, comparatively thin ice. Thin ice near a landfast boundary is also more susceptible to tidal flexure and small-scale differential motion, both of which reduce coherence over a 16–24 day interval independently of any change in dielectric properties. The observation at lines 486–488 that the flooded zones were the first to open on 9 July is, on its own, consistent with either explanation.

Would the authors consider examining the interferometric phase in addition to the coherence magnitude? If the phase over the flooded zones is spatially flat while the coherence magnitude drops, the dielectric interpretation is considerably strengthened. If instead there are phase ramps or fringe patterns over those zones, that would point toward a motion contribution. This seems to me the most direct discriminator available in the data already in hand, and I suspect it would take relatively little additional processing.

A complementary check would be to stratify the coherence comparison by measured ice thickness, to see whether flooded and non-flooded samples of comparable thickness still separate. If neither is conclusive, I would simply ask that the abstract and conclusions state that the two mechanisms co-vary in this dataset and were not separated.

A smaller point on the same mechanism. A uniform layer at a new height principally changes the interferometric phase rather than causing decorrelation; loss of coherence requires a change in the arrangement of scatterers within the resolution cell. The authors document exactly the relevant condition, namely that slush is patchy at scales below the resolution cell (lines 171–175). Framing the mechanism around sub-resolution patchiness and its evolution, rather than around a uniform interface shift, would make the argument in Sect. 4.1 more robust and is better supported by the authors' own field observations.

Thank you for highlighting this. We understand that, given the scheme as provided, we did not sufficiently highlight temporal decorrelation. In the flooding context, temporal change determines the loss of coherence through changes in scattering properties or in the arrangement of scatterers within the resolution cell. We have modified the text and Figure 12 to make this explicit.

We have checked the phase information, and the low coherence flooded areas are associated with phase noise (Figure R1a), and possibly flat phase. With the data we currently have, we have observed areas that remain flat in phase while showing low coherence, which supports the dielectric interpretation you suggested. For example, see the area surrounding Site 2 (arrows in Figure R1a).

Regarding ice movement, we observed fringe patterns over the landfast ice without losing coherence (arrows in Figure R1b), all associated with typical ice movement, and all with a component parallel to the landfast ice edge. However, we do not observe this in the slush areas because temporal decorrelation occurs with the noise.

To address the alternative explanations more fully in the manuscript, we have added a sentence on the possibility of ice movement in thin-ice areas and included Figure R1 in the Supplement, which supports the discussion above.

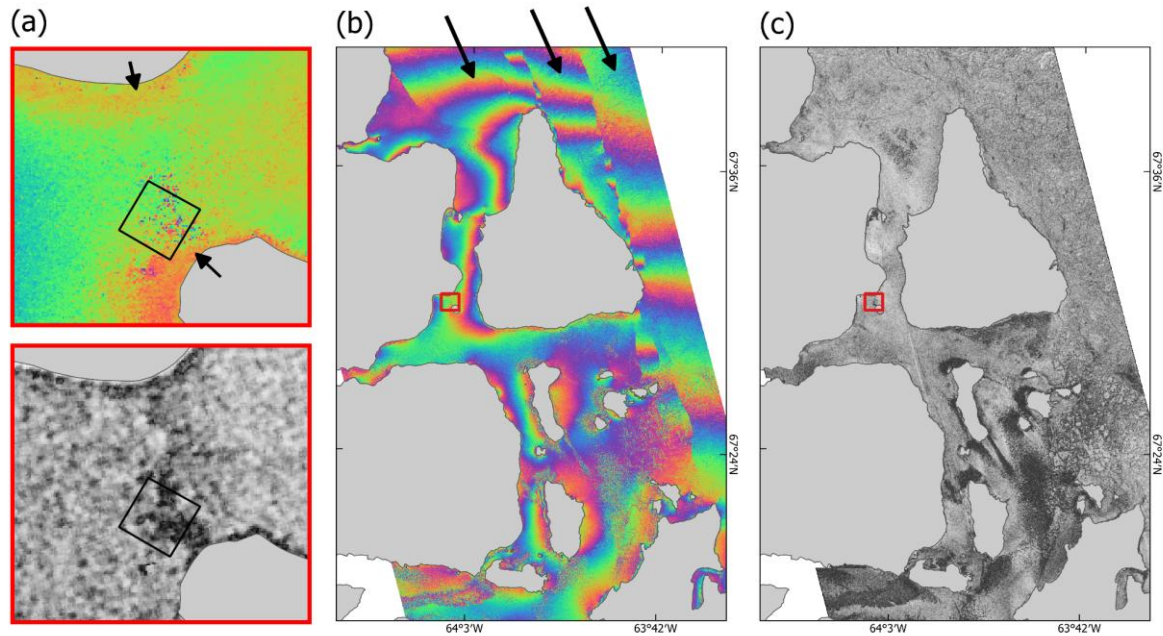


Figure R1. SAOCOM acquisition from orbit 19a. (a) Phase (top) and coherence (bottom) near Site 2 (black square); (b) phase and (c) coherence over the study area. SAOCOM® Product – ©CONAE 2024. All Rights Reserved.

Regarding the complementary check of stratifying the coherence comparison by measured ice thickness, this analysis cannot be carried out with our limited dataset. We only have pre- and post-flooding acquisitions, so in all thin ice areas the change in coherence is associated with the appearance of slush. Any attempt to separate the two effects would therefore be unreliable. The new discussion in the manuscript highlights this limitation. We will consider this analysis in future research, as additional acquisitions during the flooding process would make it possible.

- #2 Given that coherence carries the main result, could the authors add the following to Sect. 2.4?

I have questions about the coherence estimation window and the effective number of looks. The reported class means fall roughly in the 0.25–0.45 range, where the bias of the sample coherence magnitude is not negligible, so the reader needs these values to judge whether the class separation exceeds the estimator's own uncertainty. A short statement of the expected bias at the reported window size would settle this.

I am curious whether topographic or flat-earth phase was removed, and the co-registration approach. The critical baseline and geometric decorrelation contribution for

each pair. I raise this partly because I expect it to help the authors' case: since the critical baseline scales with wavelength, the 480 m perpendicular baseline of orbit 19b is less penalizing at L-band than the comparable figure would be at C-band. Making that explicit would support the temporal interpretation at lines 508–513.

On lines 508–513, the comparison between orbit 19a (16 days, 153 m) and 19b (24 days, 480 m) is used to infer a temporal-baseline dependence, but both baselines differ between the two pairs and there are only two pairs. I would suggest softening this, or supporting it with the geometric decorrelation calculation above.

We agree that we need to add the estimator uncertainty. The coherence window was fixed at a ground distance of $\sim 30 \text{ m} \times 30 \text{ m}$, to better preserve the spatial detail of the phenomenon under study. For L-band (orbit 19), this corresponded to a Range $\times$ Azimuth window of 4×8 , and for C-band (orbit 113), 14×2 (No multilooking step applied). We added this information to Methodology section 2.4.

Additionally, we have calculated the bias using the expression from Touzi et al. (1999). Using the effective number of looks, calculated from the intensity of SAOCOM in homogeneous areas to be more cautious (ENL ~ 16 , with 490 pixels), we obtained the following bias and adjusted values for the class mean values reported for L-band:

Orbit 19a			
	Flooded	Non-flooded	Difference
Measured coherence	0.32	0.45	0.13
Bias	0.06	0.03	
Adjusted coherence	0.26	0.42	0.16
Orbit 19b			
	Flooded	Non-flooded	Difference
Measured coherence	0.27	0.35	0.08
Bias	0.10	0.05	
Adjusted coherence	0.17	0.30	0.13

We added this table to the Supplement, along with a sentence stating the expected bias in Section 3.2, to give readers the elements needed to judge the separation between classes.

Regarding the processing steps, in our original method, we removed the flat-earth phase but kept the topographic phase, since we do not have any DEM for sea ice. The coregistration approach differed between sensors: conventional coregistration was applied for SAOCOM, whereas back-geocoding with Enhanced Spectral Diversity was applied for Sentinel-1, as recommended for this IW mode. We added this information to the Methodology section 2.4.

Regarding the critical baseline and geometric decorrelation, we calculated these using the expressions from Ulaby (2014) and the results are:

Orbit	Perpendicular Baseline (m)	Critical baseline (m)	Geometric decorrelation
19a	152.58	11906.26	0.99
19b	480.26	11906.26	0.96
113	145.12	2130.70	0.93

We included the table in the Supplement and added a sentence in Section 3.2 and in the Discussion to strengthen the treatment of the perpendicular baseline penalty at L-band, as suggested.

- #3 Section 2.6 describes IDW interpolation of the field slush measurements, generation of 100 random points within each interpolated area, thinning to a 30 m minimum separation, and a 70/30 split stratified by class.

Could the authors confirm whether the split was random or spatially blocked? If it was random, then training and evaluation points are drawn from the same interpolated surfaces at separations well below the scale of the SAR features themselves (the GLCM kernels are 100–135 m per lines 269–270), and the reported ROC-AUC and F1 values would represent something closer to in-sample than out-of-sample performance. With four field sites, a leave-one-site-out scheme would be straightforward and would give a figure that transfers. I would expect the resulting scores to be lower, and I want to say clearly that this would not weaken the paper. A defensible number is more useful to the community than a high one.

Two related requests:

The class labels are themselves interpolated, with reported MAE of 2.2 cm and RMSE of 3.5 cm (lines 314–315), against a class boundary at 5 cm. Could the authors report what fraction of the extended points lie within one interpolation RMSE of the threshold, and show how the results change if those are excluded?

Feature selection by KS statistic and model evaluation currently use the same data. Selecting features on a subset of sites and evaluating on the held-out site would remove this.

Thank you for this suggestion, it has substantially improved the manuscript. We have replaced the random split with a leave-one-site-out (LOSO) cross-validation scheme. In each fold, one site is held out entirely for testing while the model is trained on the remaining three. Hyperparameter tuning is also performed via nested cross-validation restricted to the training sites only, so that no information from the held-out site informs any stage of model fitting.

Because Site 3 contains only flooded observations, ROC-AUC and F1 are not defined for that fold in isolation (i.e., there are no true negatives from which to compute a false positive rate). We considered per-site metrics to have limited statistical power in isolation. We therefore pooled the predicted probabilities from all four held-out folds and computed ROC-AUC and

F1-Score on this pooled set as our primary measure of out-of-sample performance. We added this modification to the methodology in Section 2.6.

During the above modifications, we also observed that selecting the threshold that maximized F1-score among the previously tested fixed values (0.00, 0.25, 0.50, 0.75) produced poor results for feature sets F2 and F3 when applied to predict the full image. To address this, we now select the classification threshold via cross-validation restricted to the training data within each LOSO fold. We have incorporated the pooled test metrics into the revised Table 6 of the manuscript:

	Threshold	ROC- AUC	Precision	Recall	F1
1-Coherence	0.60	0.84	0.79	0.89	0.84
Model 1	0.45	0.84	0.86	0.79	0.83
Model 2	0.40	0.85	0.85	0.86	0.86

We have additionally included precision and recall addressing Comment 8.

To address the point on extended points and interpolation RMSE, we quantified the fraction of extended points falling within one interpolation RMSE (3.5 cm) of the 5 cm classification threshold. This corresponds to 21.5% of the extended dataset. As a sensitivity check, we repeated the analysis after excluding these points. F1-scores remained comparable to those obtained on the full dataset (0.81/0.86/0.87 vs. 0.84/0.83/0.86 for 1-Coherence/Model 1/Model 2, respectively). ROC-AUC, which is independent of the classification threshold, showed a modest improvement across all three feature sets (0.87/0.89/0.91 vs. 0.84/0.84/0.85 for 1-Coherence/Model 1/Model 2, respectively). We note that this improvement should be interpreted alongside the corresponding reduction in sample size. Overall, we consider the stable F1-scores and the ROC-AUC gain as supportive evidence that the results are not driven by label ambiguity near the threshold. A table with this information has been added to the Supplement.

Regarding the feature-selection requirement, we repeated the KS analysis within the LOSO scheme, computing the statistic independently for each of the four folds. Because the test was applied per fold, we defined a fold-aware criterion in advance: a variable was retained as a candidate only if its mean KS statistic (D) across folds exceeded 0.5 and it was statistically significant in at least three of the four folds. This criterion is more demanding than the original analysis, as it additionally requires consistency across folds, and fewer variables met it (five features instead of seven). We then applied the same visual redundancy inspection to this reduced candidate set, which led to the same final features as in the original analysis. We incorporated this into the revised Methods and Results sections, with the per-fold results reported in revised Table 5.

- #4 The abstract states that L-band coherence is optimal for this application and that the lower frequency is less influenced by the overlying snow (lines 22–25). I do not think either statement is separable from the acquisition configuration in this dataset.

The L-band observations come from a single mission, in one mode, at a single incidence angle and pixel spacing, in quad-pol. The C-band observations span two missions, four modes, incidence angles from 20 to 55 degrees, and dual-pol only. More importantly, C-band coherence is available from one interferometric pair. I do not think a conclusion about C-band coherence for flooding detection can rest on a single pair, and I would suggest restating this as the observation that the one C-band pair available here did not separate the classes, together with a short list of the confounding differences.

The snow influence statement is not tested anywhere in the manuscript. In cold, dry, low-salinity snow, C-band is also close to transparent, so the frequency contrast under these conditions may be smaller than implied. If the authors wish to keep an argument about snow, the salinity profiles they measured in the pits (lines 121–123) are the natural evidence, since a saline basal layer would raise the effective C-band scattering horizon while affecting L-band less. Those measurements are collected but not used anywhere in the analysis, which seems to me a missed opportunity.

The same issue affects Fig. 9a. The mean KS statistic is computed over different orbit subsets for different parameters: coherence exists for three orbits, two of which are the L-band pairs acquired on the latest and most strongly flooded dates, while sigma nought is averaged across all twelve orbits including earlier dates and wide-swath scenes at high incidence. Because this ranking underpins the headline sensitivity result, I would ask that it be restricted to a common subset of orbits and dates, or presented per-orbit only.

We agree the statement is too strong to support with the data available. In the revised manuscript we will report the results more objectively, stating that L-band performed well under these conditions, while C-band did not yield conclusive results. We will also highlight the multiple confounding parameters involved.

We acknowledge that the snow influence statement is not tested, and that we also have snow pit data that can be used for this. We removed the snow influence statement from the abstract and we are pursuing the penetration depth differences between C- and L-band as part of a modelling study that is an extension of this work.

Regarding Figure 9a, we agree it is incorrect. We have removed the subplot (a) and revised the conclusions drawn from this figure to be more specific.

- **#5** Flooded is defined as slush thickness of at least 5 cm and Non-flooded as zero, with the 0–5 cm interval excluded (lines 285–287). From Table 1, mean slush thickness is 2.0, 3.5, 10.1, and 1.0 cm at Sites 1 to 4, so only Site 3 has a mean above the threshold.

Could the authors report the number of Flooded and Non-flooded samples per site, per orbit? If the Flooded class is drawn predominantly from one site, then the reported separations are partly a between-site contrast, and sites differ in snow depth, ice thickness, and date of visit. It would be reassuring to see whether the separation also holds within sites.

Separately, the Table 1 caption states that slush thickness includes the surface snow ice layer where present. This places liquid slush and refrozen snow ice in the same class, whereas Sects. 3.1 and 4.2 argue that the two states differ substantially in their

backscatter and log-ratio response. Could the class definition be made consistent with that argument, either by separating the two states or by reporting a sensitivity test with snow-ice records excluded?

We decided to conservatively exclude 0-5 cm slush thickness because we found that, when slush ≥ 5 cm, we had a clearly flooded area and not, for example, only 0.5-1.5 cm of basal brine that had naturally wicked up from the snow-ice interface and into the snowpack. With this in mind, we ended up, after the cleaning, with this proportion of the data:

Site	1	2	3	4	R1	R2	OR
Raw	105	56	43	77	21	22	78
Non-flooded	52	34	0	39	0	0	22
Flooded	15	9	38	17	4	14	13

We acknowledge that there is some imbalance, for example site 3, an extended flooded site, has more *Flooded* samples, but the other sites have samples from both classes. The number of samples for each class when analyzing each orbit was further reduced by excluding duplicate samples within the same image pixel (e.g., lower resolution images provide fewer independent samples) and removing orbit specific noise, where present. This is why we are analyzing the statistics jointly with their significance for each feature. For example, the HV channels were not significant because of the low number of samples in the orbits. We added a table in Supplement with this information, indicating samples per site, per orbit, per class and per SAR parameter.

Regarding the class definition separating flooding states, the limited sample size makes it impractical to treat each snowpack condition as a separate class for statistical testing. Rather than partitioning ground-truth flooded records into slush and refreezing/snow-ice subclasses, which the limited sample size per site precludes, we instead exploit the temporal structure of the SAR acquisitions. The analysis therefore separates *Flooded* from *Non-flooded* conditions without distinguishing between flooding states. We acknowledge this limitation and made it explicit in the revised manuscript (Section 3.1 and 4.2). The Table 1 caption was also revised for clarity. In addition, we adapted Fig. 3b from the original manuscript (now Fig. 4b), in response to another reviewer, to show the snowpack conditions observed chronologically. The figure illustrates how slush progressively refroze over the course of the fieldwork, which supports our use of the temporal structure of the acquisitions. Finally, we recognize that a more detailed intra-site analysis would be valuable, and we expect future work to provide the additional data needed to address this gap.

- #6 Table 2 indicates that between 61 and 96 percent of L-band HV and VH samples fall below the NESZ, and 30 to 37 percent for two of the Sentinel-1 wide-swath orbits. Lines 355–356 then conclude that L-band showed almost no difference between classes in the cross-polarized channels. I would suggest revising this to state that these channels are noise-dominated and therefore uninformative here, since a null result cannot be

established from data below the noise floor. Moving the affected panels to the supplement with an explicit note would be one clean way to handle it.

Thank you, we have revised the sentence to clarify the noise dominated nature of the L-band cross-polarized channels and their limited utility, rather than interpreting the single-date result as evidence of no class difference.

- #7 The KS tests are applied to samples spaced 25 m apart, using features derived from kernels of 100–135 m, so the effective sample size is substantially smaller than the nominal one and the associated p-values are difficult to interpret at face value. The tests are also applied across 138 features without correction for multiple comparisons.

The D statistic itself is a perfectly reasonable effect size and I would keep it and the associated figures. My suggestion is simply to present D as an effect size, and to caveat or remove the significance flagging (including the -1.00 sentinel in Fig. 9) rather than to redo the analysis.

We agree with this suggestion. As noted in our response to Comment 3, the KS tests are now implemented under a leave-one-site-out framework, which reduces the effective sample size concerns arising from spatial autocorrelation of the 100-135 m feature kernels. Following this recommendation, we think it is appropriate to retain the D statistic as the effect size measure and remove the -1.00 value in Fig. 9. We flagged non-significant results with an asterisk (*) rather than remove them.

- #8 The paper is motivated by ice travel safety, which I think is the right motivation and one of its strengths. The random forest is trained on a balanced set, and ROC-AUC is correctly noted to be insensitive to class balance. However, along a given travel corridor the prevalence of thick slush is likely to be low, and at low prevalence precision degrades even for models with good ROC-AUC. The two error types also carry very different costs for someone on the ice.

Because the intended users would rely on this, it would be valuable to report precision and recall at a plausible operating prevalence, and to state the expected false-alarm and missed-detection behaviour. I raise this to make the product more dependable in use rather than to question the application.

We thank you for this comment. We agree that ROC-AUC alone is insufficient given the class imbalance expected in real operating conditions. We have now reported precision and recall jointly with F1 for all models and explained their implications for flooding detection in the context of ice travel safety in Section 3.4.

We acknowledge that we have not estimated performance at a specific field operating prevalence, as we currently lack the data to support a realistic prevalence estimate along a travel corridor. We thank you for this relevant comment and flag this as a direction for future work.

Minor comments:

- Lines 22–28 (Abstract) "is found to be optimal" reads more strongly than the evidence supports, given two L-band pairs at one location in one season. Similarly, "due to the occurrence of a high-dielectric slush layer" states a causal mechanism that the study associates but, as discussed above, does not isolate. Some qualification would bring the abstract into line with the body.

We have revised the sentence to use more cautious wording.

- Lines 93–97 and 595 The study domain is a coastal area within roughly 30 km of the community, informed by four sites. I would suggest "local" or "sub-regional" rather than "regional." Line 595, which pairs "regional scales" with "spatial resolutions of less than 50 m," reads a little awkwardly as a result.

We agree and have replaced regional with “local” as suggested. In addition, we reformulated line 595 to improve clarity and better reflect the spatial scale and resolution of the proposed approach.

- Line 130 The sensors were reinstalled adjacent to their original locations on 2 May. Could the authors state the displacement, and whether the record in Fig. 2c is treated as continuous across the interruption?

We agree that the information provided was not sufficiently clear. The sensors were reinstalled less than 1 m from their original location, in an adjacent undisturbed area, because the original site had been disturbed during the instrument inspection. Snow conditions at both locations were similar (snow depth 23.5 cm vs. 24 cm; slush thickness 8.5 cm vs. 9 cm). We have added this information to the revised manuscript. The record in Fig. 2c is not strictly continuous. Although the verification and reinstallation took less than one hour, so that the gap is short and not visually apparent, the data before and after the interruption should be considered two consecutive segments rather than a single uninterrupted series. The vertical dashed line indicates the timing of the interruption. We have clarified this in the figure caption and added the displacement and procedure to Section 2.1.

- Lines 137–141 Millero and Leung (1976) concerns seawater thermodynamics and seems an unusual citation in support of the absence of snow melt. Separately, the Site 1 sensor reading at or above 0 °C from 5 May sits somewhat awkwardly with the statement that melt was absent; a sentence reconciling the two would help.

The previous use of Millero and Leung (1976) was not necessary to support this statement. We had used this reference to account for the salinity dependent freezing point of the saline basal layers. However, we agree that the key point here is the possible onset of snowmelt indicated by the temperature sensor at Site 1. We therefore removed the reference and revised the sentence to clarify this point and reconcile the two observations.

- Lines 156–161 and Table 1 The treatment of snow ice thickness is difficult to follow. The text states it was not considered owing to measurement uncertainty, while the Table 1 caption states that slush thickness includes it. Please make the convention explicit and consistent.

The confusion arose because “snow ice” was used both as a layer and as a snowpack condition. We adapted the terminology for clarity. When the snowpack ice condition is “Refreezing,” we now refer to the transition layer as “slush → snow ice” and have adapted this terminology in Figure 3:

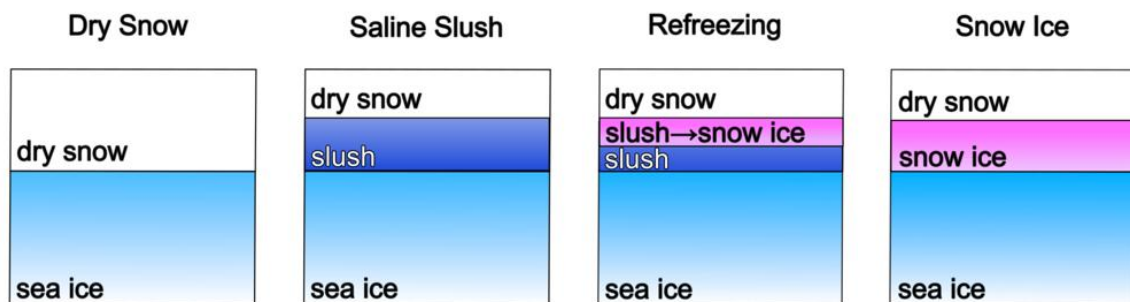


Figure R2. Schematic representation of the observed snowpack conditions.

When the entire slush layer had refrozen, we classified the snowpack condition as “Snow ice”. In this condition, we could not reliably determine the thickness of the snow ice layer (which had previously been slush). Therefore, we did not include this thickness in the slush thickness measurements. We have revised both the text and the Table 1 caption to make this convention explicit and consistent.

- Lines 288–291 Sigma nought is normalized to 28° using a single slope for C-band HH (−0.23 dB/°, $R^2 = 0.67$) and for HV (−0.02 dB/°, $R^2 = 0.01$). With $R^2 = 0.01$ the HV normalization has little basis and might simply be dropped or flagged. More substantively, the angular dependence of flooded surfaces is itself reported in the earlier literature as a discriminator, so applying a single pooled slope across both classes could suppress or introduce class differences. Could the authors derive the slope separately per class, and show the sensitivity of the results to that choice?

We re-estimated the incidence-angle slope separately for each class (flooded/non-flooded) for both HH and HV. The results are:

Polarization	Class	Slope	R2
HH	Non-flooded	-0.22	0.69
	Flooded	-0.23	0.65

HV	Non-flooded	-0.02	0.02
	Flooded	-0.01	0.00

The per-class slopes are essentially identical to the prior estimates reported in the manuscript, indicating that the pooled normalization does not mask or introduce class-dependent effects. This is consistent with the per-orbit mean visual inspection reported later in the manuscript, which similarly shows no discernible incidence angle effect on sigma nought.

- Line 291 "Wiebke et al., 2020" appears to use a given name, and I could not find a corresponding entry in the reference list.

Thank you for pointing this out. The correct reference is Aldenhoff et al. (2020). We corrected the citation and added the reference to the reference list.

- Lines 348–349 versus 425–427 and Fig. 6 Line 349 states that the HH class difference was not dependent on incidence angle, while Sect. 3.3 reports increasing separability with incidence angle for GLCM Variance and differences confined below 37° for HV. These appear inconsistent.

We believe the reference is to Lines 348–349 and 398, since incidence angle is not mentioned in Section 3.3. We agree that the sentence in Line 398 was confusing and incorrectly stated. We have therefore deleted it, as it does not accurately reflect the plots.

- Lines 402–404 and 452–462 Treating 1 – coherence as a probability of flooding is convenient, but it is an unscaled decorrelation measure rather than a calibrated probability, so comparing its F1 at a 0.50 threshold against random forest probabilities is not quite like-for-like. Reporting it as a coherence threshold, with the ROC curve and the chosen operating point, would be cleaner and would not weaken the finding.

We agree with this comment. We addressed this issue by modifying our methodology so that, instead of a fixed threshold, we now select thresholds via cross-validation restricted to the training data within each LOSO fold, as explained in Comment 3. We've reported ROC-AUC alongside F1 for all three models.

- Lines 465–474 The polynya observations are described in the text as subjective indicators, which I think is right, but the abstract and conclusions read as though they validate the regional predictions. Since polynya formation is also associated with thin and dynamically active ice, I would suggest describing this consistently as a qualitative consistency check.

We revised the wording in the Discussion and Conclusions to describe the polynya observations consistently as a qualitative consistency check rather than validation of the model predictions. The polynya observations are not mentioned in the abstract.

- Lines 501–503 The suggestion regarding upward capillary transport and volume scattering is plausible but appears speculative in this context; marking it as an inference would be clearer.

The statement has been revised to explicitly mark the proposed mechanism as an inference rather than a stated fact.

- Lines 505–507 The proposed C-band explanation involving snow volume change between passes is closely related to work already in the reference list on C-band coherence and snow thermodynamics. Engaging with it here would strengthen the interpretation of the single C-band pair.

We have added this discussion to the revised manuscript. In addition to snow volume changes between acquisitions, we have also incorporated the potential effect of snow redistribution, as discussed in the cited study, as another possible mechanism contributing to the observed C-band coherence changes.

- Line 469 8 May to 11 June is 34 days.

Thank you for pointing this out. Corrected.

References

- Aldenhoff, W., Eriksson, L. E. B., Ye, Y., & Heuzé, C. (2020). First-Year and Multiyear Sea Ice Incidence Angle Normalization of Dual-Polarized Sentinel-1 SAR Images in the Beaufort Sea. *Ieee Journal of Selected Topics in Applied Earth Observations and Remote Sensing*, 13, 1540-1550. <https://doi.org/10.1109/JSTARS.2020.2977506>
- Touzi, R., Lopes, A., Bruniquel, J., & Vachon, P. W. (1999). Coherence estimation for SAR imagery. *Ieee Transactions on Geoscience and Remote Sensing*, 37(1), 135-149. <https://doi.org/10.1109/36.73914>
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