Response to Referee Comment RC1

Ground ice distribution and degradation rates in the near-shore coastal zone

Warren et al.

We thank the referee for a careful and constructive review, and in particular for identifying the 2018 velocity model as the point requiring the strongest justification. We agree it is the most consequential methodological choice in the paper, and we set out below both the evidence supporting it and the additional material we will add at revision. We respond to each point in turn, with the referee's comments in italics.

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1. Justification of the 2018 velocity model

"The difference in velocity between 2018 and the later years is significant, and the velocity models were built using different approaches… I recommend the authors provide a more rigorous justification for the 2018 velocity model, including a comparison with theoretical values or literature benchmarks."

This is a fair request and we are happy to meet it directly.

Converting the three velocities to relative permittivity and then to volumetric water content using the empirical relation of Topp et al. (1980) gives:

Year	v (m/ns)	ε_r	θ_v	Thawing index (°C days)
2018	0.084	12.7	0.24	574
2024	0.075	16.0	0.29	882
2023	0.073	17.0	0.31	1,103

All three values fall within the standard range for partially to fully saturated sand ($\varepsilon_r \approx$ 10–25); none is anomalous. The implied water contents are ordered consistently with the thawing indices for the three campaigns: 2018 has by far the lowest thawing index, a correspondingly shallower and less-developed thaw, and the driest inferred overburden. The 2018 velocity is therefore not an outlier requiring explanation but is in the direction the site conditions predict. The difference between years is modest in permittivity terms (12.7 against 16–17) even though it appears larger expressed as velocity, because velocity varies with the inverse square root of permittivity.

We will add this comparison to Section 2 at revision.

We would also note that the manuscript already carries the consequences of this choice quantitatively rather than qualitatively. The standard error of 0.007 m/ns on the mean of the eight hyperbola fits is propagated as a *systematic* uncertainty, reported separately from the per-point terms precisely because a single velocity scales all 2018 depths together and displaces the whole relationship rather than adding scatter to it. It propagates to ±0.6 cm yr^{-1} on the mean degradation rate and ±9 cm on the fitted decay length, and the 7–27 day forcing-period band quoted in the abstract and conclusions already incorporates it. Section 3 also records that the fitted velocities decline with two-way travel time ($r = -0.74$, $p = 0.04$), that this is consistent with increasing saturation with depth and towards the sea, and that the trend was not applied because it is not robust to removal of individual hyperbolae.

We accept that no in-year validation exists for 2018 and that this is a genuine limitation; it is stated as such in the limitations paragraph, where the 2018 velocity is identified as "the largest single systematic control" on the reported rates.

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2. Processing parameters

"A critical point at this stage is the detailed description of the processing procedures for both the GPR and the HVSR techniques. The text mentions the procedures generally but lacks specific parameter details. This information is crucial, as processing steps, especially signal filtering, have a significant impact on data comparability."

We accept this entirely. The manuscript named the workflow but gave no parameters. We will replace it with the following full description at revision.

Data were acquired with a sampling interval of 0.4 ns over an 84.6 ns window (212 samples per trace), giving a Nyquist frequency of 1,253 MHz and approximately ten samples per cycle of the dominant 250 MHz wavelet. **An identical workflow with identical parameters was applied to every profile from all three campaigns**, in the following order:

1. Trace-by-trace normalisation
2. Resampling to an equidistant 0.05 m trace spacing
3. Time-zero correction, 2% amplitude threshold with manual sample adjustment
4. DC shift removal, full trace as window
5. Dewow, 40-sample (16 ns) window — four periods of the dominant wavelet
6. Background subtraction, running median over 30 traces (1.5 m)
7. Manual gain, rising from 0 to approximately 60 dB by 60 ns
8. Band-pass filter, corner frequencies 150 and 500 MHz, order 50
9. Frequency–wavenumber migration
10. Gaussian smoothing, horizontal standard deviation one trace, vertical standard deviation zero

The only parameter that differed between campaigns was the migration velocity, which was set to each year's project velocity because the medium velocity differs between years. We recognise that this is the one place where the 2018 velocity enters the processing as well as the depth conversion, so we quantify it here. Migration repositions a dipping reflector predominantly laterally rather than vertically: at the dips observed (1:4 to 1:7, so 8–14°), $\cos\theta$ is 0.97–0.99, and a 15% difference in migration velocity changes the depth picked at a given position by less than 3 cm at 1 m depth. That is well inside the ±7 cm picking uncertainty already reported, and an order of magnitude smaller than the same velocity's effect on depth conversion, which the manuscript already identifies as the dominant systematic term.

On the referee's specific concern that filtering affects comparability, three points may be helpful. The band-pass corners of 150 and 500 MHz are 0.6 and 2.0 times the nominal antenna centre frequency, and the upper corner sits at 40% of Nyquist, so no aliasing arises. The background subtraction window of 1.5 m is long enough that the ice surface moves 1.5–2.6 wavelengths across it at the observed dips, so the dipping reflector is not removed as background, whereas flat-lying ringing is. And the final Gaussian smoothing has a vertical standard deviation of zero, so it operates only across traces at constant time and cannot shift a reflector in two-way travel time; its width is approximately one third of the wavelength in the overlying sand, below the lateral resolution the data support.

We note in passing that the applied gain is consistent with the attenuation implied by our own conductivity measurements: approximately 60 dB by 60 ns, against a predicted two-way loss of roughly 53 dB at the 1.9 m depth where signal is lost in the 2023 profile.

The HVSR processing will be described in equivalent detail. Three-component velocity records of 600 s were divided into 30 s windows with 50% overlap, giving 39 windows per station, and a 10% cosine taper was applied to each end of every window. Amplitude spectra were smoothed with a 1000-point Gaussian window, corresponding to a frequency smoothing bandwidth of 3 Hz. Both median and mean spectra were computed, their simi-

larity providing a test of the stationarity of the spectral estimates, and individual window spectra deviating by more than ±2 standard deviations were rejected. Spectral peaks were picked at the maximum of the peak of interest; with a Nyquist frequency of 400 Hz and 131,073 discrete frequencies per spectrum, the spectral sampling of approximately 0.003 Hz makes interpolation to refine the peak position unnecessary. As with the GPR, identical parameters were applied to both the 2023 and 2024 campaigns.

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3. HVSR acquisition duration

"Regarding the HVSR technique, I noted that the data acquisition time was relatively short (only 10 minutes). In many similar studies, I have observed acquisition times exceeding an hour. Could the authors clarify if they compared these 10-minute datasets with longer observations at the same points? If so, did they find any significant differences, or was this duration a fixed constraint for other reasons? A brief justification for the 10-minute window would strengthen the reliability of the HVSR results."

We did not acquire longer records at any station, so no direct comparison is available. The duration should have been justified in the manuscript and will be at revision.

It may help to note that the acquisition system was purpose-designed for this class of target rather than configured to a general-purpose recipe. It uses broadband 4.5 Hz three-component geophones sampled at 800 Hz, giving a Nyquist frequency of 400 Hz, specifically so that thin layers and the correspondingly high resonant frequencies they produce can be resolved; the design and its rationale are described by Khan et al. (2024). Much commercially available HVSR instrumentation is substantially undersampled for this purpose. The design point is therefore quite different from the sub-10 Hz basin resonances for which hour-long acquisitions are conventionally recommended.

The 600 s record length was chosen on the basis of prior experience with HVSR in the Tuktoyaktuk area, where records of this length have consistently yielded high-quality spectra. The ambient noise environment is unusually favourable: there is no major industry or busy highway in the vicinity, so anthropogenic noise is both sparse and of low amplitude, and on Qikiqtarjuak itself — an uninhabited barrier island — there was less evidence of anthropogenic sources still.

As a cross-check, the duration also exceeds the recommendations of the SESAME (2004) guidelines by a wide margin. Those guidelines scale the recommended minimum recording time inversely with expected f_0 — thirty minutes at 0.2 Hz, ten minutes at 1 Hz, two minutes at 10 Hz — and although the table does not extend to the frequencies of interest here, the resonances we pick are an order of magnitude higher than its lowest entry: 10–70 Hz for the base of the active layer and 135–151 Hz for the deeper interface. A 600 s record is thus five times the recommended minimum at the lowest frequency we pick and far more at the rest. With 30 s windows at 50% overlap, the window-length criterion $f_0 > 10/L_w$ is satisfied above 0.33 Hz, a factor of thirty below our lowest pick, and the significant-cycles criterion $n_c = L_w \times n_w \times f_0 > 200$ is met throughout:

Target	f_0 (Hz)	n_c	vs. threshold
Active layer base, low end	10	11,700	58×
Active layer base, high end	70	81,900	410×
Deeper interface, low end	135	157,950	790×
Deeper interface, high end	151	176,670	883×

We accept that a direct comparison against a longer record would have been preferable and will note it for future deployments at this site.

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4. Gradient reported in the abstract

"The current sentence, 'The GPR resolves the upper surface of a massive ice body dipping seaward at approximately 1:5,' appears in the Abstract. I suggest replacing this with a more precise description: 'dipping seaward with gradients ranging from 1:4 to 1:7.'"

Agreed, and we will make this change. The referee is correct that 1:5 is the gradient along the single section shown in Fig. 7a, whereas the B-scans give 1:4 (2024) and 1:7 (2023), and the conclusions already quote the range. The abstract should match.

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5. Orientation indicators

"The figures 1, 2, 5, 7 lack orientation arrows (e.g., North indicators)."

Agreed. North arrows will be added to Figs. 1 and 5. For Figs. 2 and 7, which are oblique photographs and a perspective view rather than plan views, we will instead state the view direction in the captions, which conveys the same information without implying a map projection.

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6. Title

"The title lacks precision regarding the survey area. I recommend revising the title to clearly indicate the specific location."

Agreed. We will revise the title to "Ground ice distribution and degradation rates in the near-shore coastal zone, Qikiqtarjuak, Beaufort Sea", which names both the site and the region.

We note the referee spells the island "Qikiqtarjuaq". We have confirmed with our Inuvialuit partners that "Qikiqtarjuak", as used in the manuscript, is the correct local spelling for Tuktoyaktuk Island, and we will retain it.

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7. Overall concern regarding 2018–2023 comparability

"I have serious concerns regarding the precision of the comparison between 2018 and later years. These concerns stem from the different survey patterns, the lack of precise vertical positioning in 2018, and the differing geophysical acquisition across the years."

We share the referee's view that this is the principal constraint on the work, and the manuscript treats it as such rather than setting it aside. We would draw attention to four elements of that treatment which may not have been evident from a first reading.

Vertical positioning does not enter the comparison. Reflector depths were picked in two-way travel time and converted to depth *below the local beach surface*. The quantity differenced between campaigns is therefore depth below surface, not elevation, and the vertical accuracy of the 2018 handheld GNSS does not propagate into it. This is stated in Section 3 but we will make it more prominent, since it addresses the referee's concern directly.

Beach surface change was measured, not assumed. Contemporaneous photogrammetric DEMs were differenced at every GPR trace position along the transect, giving a mean surface change of -0.7 ± 8.0 cm over the five-year interval — indistinguishable from zero. The comparison therefore does not rest on an assumption of a stable datum.

The comparison extent was set by positional quality, not convenience. The section of the 2018 profile closest to the cliff was excluded because handheld GNSS elevations there departed from the DEM with a scatter of 3.2 m, against 0.6 m over the retained 44.6 m, consistent with multipath from the adjacent ice cliff. The criterion and both figures are stated in Section 3.

The limitations are stated explicitly. The manuscript's limitations paragraph records that the rates derive from a single mid-beach transect rather than a spatially distributed comparison, that the quoted mean is a mean over binned overburden thickness rather than a spatial mean, and that the 2018 velocity remains the largest single systematic control.

We would add that the central result of the paper — the exponential depth-dependence and the forcing period inferred from its decay length — is comparatively robust to the referee's concerns. A systematic error in the 2018 velocity displaces the whole relationship rather than altering its form, and the resulting shift in decay length is already carried in the 7–27 day band. That band excludes the diurnal tidal constituents and the annual cycle by roughly an order of magnitude in either direction, so the qualitative conclusion is unchanged across the full range of plausible velocity error.

On the choice of methods

"From a geophysical perspective, the choice of methods is non-standard. Specifically, the use of GPR with a passive seismic approach (HVSR in this research) is less common than the typical combination of additional Active seismic (e.g. SRT) or geoelectrical methods (e.g. ERT)."

We agree this combination is uncommon and are grateful the referee found the cross-correlation convincing. The choice was driven by site constraints: the survey window is set by the tide, the beach is 1–6 m wide and bounded by an actively collapsing cliff, and equipment must be carried. Passive seismic requires no active source and no electrode installation, which matters where the working area is narrow, transient and periodically inundated. The complementarity is also practical rather than incidental — the radar signal is lost in saturated sand at the seaward end of the profiles, and the seismic data extend the characterisation beyond that limit. ERT has been applied in the subsea setting offshore of this site by Erkens et al. (2025), and the comparison between those results and ours is discussed in Section 3.

References

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Topp, G. C., Davis, J. L., and Annan, A. P.: Electromagnetic determination of soil water content: Measurements in coaxial transmission lines, Water Resour. Res., 16, 574–582, 1980.