Dear authors,

I have reviewed your manuscript and find that at the present state, it is unsuitable for publication at this journal. Firstly, I find that the overall presentation of the manuscript is weak and there are several spelling and grammatical errors, poor quality figures and the content is not well structured. I would highly suggest that you forward the text to a native speaker or make appropriate corrections before resubmitting.

Regarding the methodology and results, I find that your approach is a rather standard application of GPR that is better suited for an applied journal. You do not propose a novel methodology, only an application of existing approaches. Furthermore, you do not assess the need of migration for your GPR data which makes you over/underestimate the dimensions of the frozen soil body. Although you perform a depth conversion (from traveltimes) you do not mention the need of migration, which would correct for the strong hyperbolic patterns observed in your measurements.

In your discussion you mention a 3D geological model which is unfortunately never presented in the manuscript. Your error analysis focuses on positioning errors of the boreholes themselves, but you never mention positioning errors of the user (i.e., when running the borehole antennas in and out of the borehole). In several occassions you do not adequately explain your approach (e.g., when correcting for first arrival picking).

Overall, I find that this manuscript needs a major revision before being accepted to any journal, and I find that EGU solid Earth is perhaps not the most suitable journal because your work is primarily applied. You will find detailed comments of my revision in the appended pdf file, present as comments (larger tasks) or highlights (smaller edits).

Regards,

Thank you for your thorough review and constructive feedback on our manuscript. We appreciate the time and effort you have invested in evaluating our work.

We acknowledge the need for significant improvements in the presentation of our manuscript. We will address the spelling and grammatical errors and improve the quality of the figures. We will also seek the assistance of a native speaker to ensure the language is polished and clear.

Regarding the methodology, while we understand your point about the application of Ground Penetrating Radar (GPR) being considered standard, we believe our focus on process observation and structural exploration in a highly unusual experimental context brings a unique perspective that fits within the multidisciplinary scope of EGU Solid Earth. Our approach, though based on existing methods, is tailored to the specific challenges and conditions of our study, which we believe adds value to the current body of knowledge.

We also recognize the importance of discussing the need for migration in GPR data. However, due to the large trace distance used in our study, migration is not feasible or appropriate. The large trace distance results from our aim to develop a fast applicable measuring setup, which necessitates a compromise between resolution and efficiency. This lower resolution diminishes the effectiveness of migration in correcting for hyperbolic patterns. Instead, we have focused on depth conversion from traveltimes and have provided a robust analysis within these constraints. We will clarify this point in our revised manuscript to ensure it is well understood.

Regarding the 3D geological model, we will rephrase the manuscript and clarify our intention. We create a 3D velocity model shown in Figure 5. With the information from the logging one can extrapolate the geological layers according to the velocity model.

We will clarify our approach, especially in areas such as first arrival picking corrections, to ensure that our methodology is comprehensively explained and transparent.

While we understand your concern about the journal's suitability, we believe that EGU Solid Earth, with its focus on experimental and multidisciplinary research, is an appropriate venue for our work. Our study emphasizes structural exploration aiming for process observation in unconsolidated shallow aquifers, e.g. glacial aquifers, which align with the journal's scope.

We appreciate your detailed comments and will address each one carefully in our revision. Thank you again for your valuable feedback.

major comments

l 124 are these monopole antennas? If so, how do you perform time zero correction?

Each antenna contains source and receiver. Distance between centre of source and receiver will be added in the description. Zero-time correction is done by picking the maximum of the first arrival and shifting the traces by this time difference.

l 127 Can you please elaborate on this correction? It is not clear from the text what you mean, at least to me.

This will be rephrased to make it more understandable. Zero-time correction is described in the answer to the previous comment. The reason or using the maximum of the first phase instead of the first break is the low signal-to-noise ratio of the ice body reflections. Picking first breaks of the reflections is often ambiguous, while the maximum is more reliable.

Fig 2 why are there no axis labels or units on these figures? Please include.

Axis labels will be added. Units are contained in the scale bar within the figure.

Fig 3 This figure can be improved. Some comments

Use Capitalization for figures

eg. Traveltime [m] and not traveltime t[m]

The vertical label on plot b is misspelled

Thank you for pointing that out. We will correct the errors and make the capitalization consistent in all figures.

l 147 I get (and mathematica too)

*s_ice = (v + v_th*v_ice - s*v_ice)/(v_th - v_ice)*

which does not reduce to your equation

We are glad that you pointed that out. The last formula (3) is correct, but there is a mistake one step earlier in line 171 (equation (2). The beginning of the line has to be t=s/v=…. If this is the case, the steps are correct.

In t=s/v we substitute t=t_ice+t_thawed, so we get t_ice+t_thawed=s/v.

Now substitute t_ice=s_ice/v_ice and t_thawed=s_thawed/v_thawed.

We get s/v= s_ice/v_ice +s_thawed_v_thawed.

Substituting s_thawed=s-s_ice and solving for s_ice yields the formula given in the original manuscript.

Fig 5 It is difficult to assess this figure given that you overlap the scatter points and often hide information from points below.

Yes, there is overlaps of data points, but, from our point of view, it only impedes the visibility of one ZOP in Figure 5a (at Easting =333308.8m). However, we have formulated it improperly so far in the manuscript. We will extend the description to make clear, that the figure is a 2D view of a 3D distribution. We choose to display the data in this way because it shows the 3D distribution of the velocity better than the 3D plots we created.

Fig 6 This figure is unnecessarily large, and now you capitalize the labels in contrast to previous figures. You also do not mention in the caption anything about the other wells

(shown in (b).

Also (a) and (b) overlap with axis label/ticks

We will remove the overlap. The large size was chosen for better visibility. We will check if reducing the size will still give insight on all relevant details. We could not identify reflections corresponding to the other wells. We will point this out in either the description or the corresponding paragraph.

Fig 7 Th3 hyperbolic patterns observed in this image are very typical in single hole GPR reflection data. Without migration, these hyperbolas will largely overestimate the size of your ice body. I suggest you perform migration with a suitable velocity model, even if v_ice should be inferred, to avoid ambiguous results. Or at least test igration approaches and comment on these.

You are right, the hyperbolic patterns are diffractions from the upper boundary of the ice body. We tried migrating the profiles, but did not get favourable results, because our trace spacing is to coarse with 0.25m between each trace. We will include this is the discussion and mention, that vertical size will be overestimated. Our main goal was to get the lateral extent of the ice body, which does not suffer from the lack of migration.

Fig 8 Why use parentheses here and in all other figures use square brackets? Units are missing (I assue meters)

Thank you for pointing that out. We will correct the errors and make all figures consistent.

l 255 Can you start your discussion with an overall assessment of your work? It is unclear to the reader what you plan to discuss. It seems like you are going directly into the possible sources of error, but an introductory paragraph should help.

We will rephrase the beginning of the discussion, before focusing on the error sources.

l 303 Have you tried migrating your data with these two velocities to see what are the resulting profiles? You are now creating a simple time to depth conversion which is inaccurate at the frozen state. So including (even) an erroneous s:ice velocity does not seem much different. Migration will at least allow you to avoid the stron hyperbolic patterns you get from scattering of the EM wave.

We tried migration with the velocities derived from ZOP (see answer to Comment on Figure 7). The time-depth correction is still valid in the frozen state, because our reflected signal only travels in the unfrozen medium and we know the velocity distribution in the unfrozen medium from ZOP.

l 326 Not true, you can still have positioning errors which are related to the depth positioning of your antennas in the borehole. You do not address these in your manuscript, but they can easily be there and in the order of cm usually

You are right, there might also be vertical positioning errors. We did the vertical positioning with fixed markings along the antenna cable and aligned these with the top of the observation well, so we are confident that our vertical error is <0.01m. We will mention this is in the discussion.

l 345 you do not present a 3D subsurface geological model in your manuscript.

This will be rephrased. We create a 3D velocity model shown in Figure 5. With the information from the logging one can extrapolate the geological layers according to the velocity model.

minor comments

l 46 isn't this true for all geophysical inversion?

Can you not apply sharp boundary inversion for ERT?

The correct arguent would be potential field methods vs. wave propagaton methods, but the current argument on interpreting inversion results is ambiguous. Please be more precise on the actual gap.

Thank you for this suggestion. We will be more precise on why a wave based method yields higher accuracy for the position of a vertical layer boundary. Geophysical inversion is always depending on interpretation, which is why we try to avoid it by deducting information from reflections.

l 65 Can you elaborate on these experiments if they are relevant to the study, instead of listing 8 references? The reader is unlikely to go through all these. If they are not all relevant, it is sufficient to list a few.

Good point, we will either reduce the references or mention the other experiments (heat injection, methane injection, H_2-injection).

l 142 How can correct data acquisition be ensured if velocity stays the same? Data acquisition is dependent on the operator, not on the changing subsurface.

Please reprase.

The underlying assumption is that the subsurface remains unchanged in the reference measurements. Consequently, if there is no change in the data, it can be assumed that there is no significant error in the acquisition process. This will be further explained in the manuscript.

l 152 what do you mean here exactly? Please explain better.

Yes, this might be a bit vague and will be formulated more precisely.

l 199-201 How exactly do you make this inference from your results? Can you guide the reader?

The result is inferred from the results in Figure 4. The paragraph will be rephrased to explain better how this is concluded.

l 201-202 Figure 5 does not give a 3D impression of your experiment. A 3D figure would give such an impression.

In our opinion, a plot with fixed Easting and fixed Northing coordinates can give an impression of the 3D velocity distribution. We tried 3D plots, but the 2D plots were clearer.

l 205 / Fig 4 you call these profiles and radargrams interchangeably. Please be consistent.

We will make the naming consistent.

l 264 Where is this 3D geological model? Why is it mentioned here but never presented in the manuscript?

See answer to comment on Line 345.

l 279 Since you know the geometry so well could you try to remove these with forward modeling, e.g. gprmax 3D ? It also seems like the phase is different between these reflectors and the frozen section, so this could lead to some more effective processing.

It would be nice to model this, but we lack information on permittivity of observation wells and heat exchangers. Moreover unexpectedly, the reflection data from before and after the installation of the heat exchanger show no observable change in signal at the travel times corresponding to the distance to the heat exchangers. So we assume, that the observed reflections are reflections of the observation wells. As the heat exchangers are located in similar distance to the boreholes as the other observation wells, it is possible that any signal that does occur is superimposed. Another observation is that not all of the observation boreholes actually appear as reflections - combined with geological heterogeneities, forward modelling was not used in this first experiment. Therefore, in a recent second experiment, we have avoided installing observation wells and heat exchangers at similar distances, which may allow us to test such a modelling approach. Results are expected in mid-2025.

l 293 be consistent with writing 3D. The correct and used approach is to use 3D. you sometimes use 3 dimensional, sometimes 3D and here you spell out three-dimensional.

We will make the naming consistent.

l 319/320 Did you have consistent time zero at the beginning and end? Please include this information in the metohdology. You are only mentioning this here.

We saw a change in zero time, when there was a high difference between outside temperature and subsurface temperature. Comparing with temperature data showed a temperature dependency, which is why we decided on using the zero time from measurements directly after the last borehole

measurement, because then, antennas and cables were still at the same temperature as in the borehole.

l 321 you are mentioning this as time-zero up until now, when you mention t0. You can not expect the reader to understand a new and not-defined nomeclature at the discussion of your manusccript.

We will make the naming consistent.

l 325 computed or estimated? If computed, how?

This will be changed to computed. For how, see Figure 3b.

l 351 You seem to only do 3 campaigns, and one is only ZOP. So this would not be multiple (better write out 2 or 3)

We did more test measurements to assess repeatability. This information will be added.

l 354 this is only true if anisotropy is not present

True. We consider no anisotropy in unconsolidated saturated sediments.