Reviewer Recommendation and Comments for Manuscript Number egusphere-2023-671

SPECTRAL INDUCED POLARIZATION IMAGING TO MONITOR SEASONAL AND ANNUAL DYNAMICS OF FROZEN GROUND AT A MOUNTAIN PERMAFROST SITE IN THE ITALIAN ALPS

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Summary

This manuscript presents a large spectral induced polarization (SIP) monitoring data set collected at an alpine permafrost site over the period of 3 years. Refraction seismic measurements in the field (late summer), broadband SIP measurements on sediment and rock samples at different temperatures (freezing and thawing cycles), and borehole temperature data complement the SIP monitoring data set and aid in the interpretation of the results.

SIP field and lab data show a characteristic increase of the resistivity phase shift at the high-frequency limit of the field data set around 100 Hz (higher frequencies are affected by electromagnetic coupling), which can be associated with the presence of water ice in the pore space. This interpretation is supported by the results of a joint petrophysical inversion approach of electrical and refraction seismic data, which resolves for the subsurface ice content (amongst other parameters), and by theoretical considerations (not discussed in detail in the manuscript). The authors propose the introduction of a quantity called the "phase frequency effect", which consists in the difference of the logarithms of the phase shifts observed at the highest and the lowest frequency, respectively, divided by the logarithms of these frequencies. This new quantity shows a good correlation with ice content estimates from the joint petrophysical inversion approach.

The manuscript is well written and structured. It provides all information needed to understand the collection, processing and interpretation of the presented data set. However, the text contains several instances of repetition and redundancies, which could be merged and streamlined to shorten the manuscript and improve its clarity. Furthermore, providing a more complete picture and discussion of the expected broadband response of ice-containing sediment and rock would improve the understanding of the effect underlying the newly introduced "phase frequency effect". Finally, the authors could provide some additional discussion on the risk of misinterpreting the effect of electromagnetic coupling in terms of ice content (both lead to increased phase values at high frequencies). These issues together with a number of specific comments and technical corrections listed below, should be straightforward to address. Therefore, I recommend accepting this interesting and relevant manuscript after a moderate revision.

General comments

<u>Connection of phase frequency effect with ice relaxation at higher frequencies</u>

As mentioned correctly by the authors, the main characteristic relaxation of ice-bearing sediment and rock is expected is most obvious in the kHz frequency range, where it often causes a clear peak in the phase spectrum. To allow all readers to understand how the increase at significantly lower frequencies (i.e., around 100 Hz) is related to this high-frequency peak, I suggest showing some broadband phase spectra from the lab measurements carried out within this project or a suitable theoretical model. Although this more complete picture would probably show that the suggested approach is not yet the best of the (theoretically) possible methods for ice-content estimation, the contribution is very valuable as it shows a suitable and well-justified workaround as long as no multi-channel high-frequency SIP devices are available.

Influence of inductive/capacitive coupling at high frequencies

An increase of phase values at high frequencies does not necessarily indicate a high ice content. More commonly, high phase values at high frequencies are related to inductive and/or capacitive effects. I suggest including a more rigorous discussion of the risk of misinterpreting such coupling effects in terms of high ice content. It should also be mentioned that such a misinterpretation can best be reduced by improving measurement capabilities at high frequencies (100 kHz and higher).

Potential of shortening the manuscript

The manuscript could be shortened significantly by systematically removing redundancies (data presented repeatedly in various figures and repetitions of explanation and concepts in the text). For (only some) examples that might help to start streamlining the manuscript, please see specific comments below.

Specific comments and technical corrections

Line 3: frequency-dependence -> frequency dependence (without hyphen) (orthography)

Line 30: physical properties -> more specifically: thermal state (or similar)

Line 45: parts -> part (grammar)

Line 46: is transformed into ice -> freezes

Line 56: Add "which has not yet been implemented in the PJI."

Line 59ff: "… while the imaginary component relates to surface conductivity arising from the accumulation and polarization of charges at the EDL formed in the ice-water

or rock-water interface as well as protonic defects in ice surface." Without further discussion, this list of effects is not complete. I recommend adding an "e.g." and defining the frequency range of interest (too late for this statement, if introduced in the next sentence).

Line 61ff: Please note that the study of Mudler clearly exceeds the usual IP frequency range by at least two orders of magnitude. As stated in later in the discussion, ice-related relaxation effects most clearly show up at frequencies >1000 Hz. Already at this point, the reference to the study by Mudler et al. (2022) should be accompanied by a more detailed discussion of the differences of "usual" SIP measurements and high-frequency IP (HFIP) measurements (see my general comment).

Line 72: "retrieved ice volumes" -> "were able to estimate ice volumes at a … from electrical resistivity data (or similar)".

Line 73: What is a talus slope? Please add a brief description.

Line 89: "measured surface conductivity" -> How can the surface conductivity be measured directly? Maybe better "the effect of surface conductivity"?

Figure 1: Please add detailed information on sources of the background images: Digital elevation model in panel 1, satellite data in panels 2 and 3. In the caption, mention that the satellite background picture is overlain by an orthophoto of the survey area including contour lines (derived from the orthophotos?). What do the blue shadings in panel 2 mean? What does the single blue circle south of the label "MON" indicate?

Line 134: “monitoring period” -> “SIP monitoring period” (as other monitoring periods of the site started as early as 2004…)

Line 137: according to Fig. 2 b and c, temperatures in DBH during summer 2022 were even higher. Please check!

Figure 2: In sight of the total length of the manuscript, I recommend reducing the data shown in this figure: Are precipitation and air/surface temperature strictly necessary for the discussion of the geophysical data or could they be shown in the supplementary material? Ground volumetric water content is redundant with the data shown in Fig. 11. Panels b and c show the same data in two different ways. Part of the same temperature data is shown again in Fig. 12.

Line 169, equation (1): “-“ -> “+”, otherwise ρ'' needs to be defined as the negative of the imaginary part of the resistivity.

Line 170, equation (3): Remove “-“ (see above)

Line 186ff: Should read “…between the logarithms of the low-frequency and high-frequency… normalized by the difference of the logarithms of…” or similar.

Line 208: Add “remaining” or similar before “liquid pore water phase”

Line 229: Add “(in percent)” following “volumetric water content” to motivate the multiplication by 100.

Line 233 / equation (9): Shouldn’t the equation rather read $S_W = e^{\frac{\ln\left(\frac{\rho_W \phi^{-m}}{\rho_{bcorr}}\right)}{n}}$? I also suggest expressing the equation in a simpler form eviting the exponential, the logarithm and the negative sign of the cementation exponent as follows: $S_W = \sqrt[n]{\frac{\rho_W}{\rho_{corr}\phi^m}}$.

Line 236: Instead of “represents a temperature correction applied to the bulk electrical resistivity data”, consider writing “is the temperature-corrected bulk electrical resistivity”

Line 237, equation (10): Please consider introducing this temperature correction together with the temperature correction in Line 200, equation (6).

Line 262f: “using 4 potential dipoles (with lengths of…)” -> “using 4 potential dipole lengths of …”

Line 268: Do these current injections refer to the RS-check or the actual measurement. Please specify in this sentence.

Line 269: “acquired”-> “observed”

Line 275: “Corona pandemic” -> “COVID-19 pandemic”

Line 299f: Please justify/motivate the use of equal error parameters for all frequencies in more detail. Usually, high-frequency data is more affected by electromagnetic coupling than low-frequency data. Why would a frequency-independent error model still be a good assumption?

Line 307ff: Please mention that the computation of the RMS includes a normalization of the individual misfits by the error taken from the error model. Otherwise, the dimensionless target value of 1 does not make sense.

Line 315, equation (14): Please provide a more information on the determination of R(x,z). E.g. are m_1 and m_2 fixed as suggested by the definition in the next line or do they vary with x and z as suggested by equation (14)? What are the values of m_{1r} and m_{2r}?

Line 316f: Are values of R computed for resistivity and phase images and applied separately to the corresponding images? Or do you use one “mask” for all images?

Line 318: Is the DOI index equal to R? In this case, could you either use DOI OR R?

Line 320, Figure 3: Here, the computation of the DOI analysis appears before the inversion step. From the explanations above, I understood that this analysis was based on the inversion output (“mean… values observed in all SIP images”). Please check and – if necessary – explain in more detail.

Line 345f: “same values” -> “same values of porosity and seismic velocities”

Line 349ff: Obviously, the selection of a constant value of the surface conductivity is empirically useful. Could this selection also be physically sensible/meaningful? Please add a short discussion of the implications of this assumption.

Line 358ff: How was the saturating fluid prepared (tap water with NaCl)? Please provide some more detail.

Line 268: Actually, this section is rather a “Results and discussion” section, as most of the results are already discussed in some detail. In particular, in this section the text could be shortened by streamlining (in German “straffen”) the presentation of results. E.g., lab and field data could be discussed together instead of separately, which leads to many redundancies in the discussion.

Line 374: “inflection point in electrical resistivity”: The depth slices in Figure 4 are not suitable to show an inflection point in the depth profile. Please consider discussing this concept based on depth profile (e.g. at the positions of the two boreholes).

Line 376: In the Figure, all resistivity values are shown as log10-scaled values, while the text refers to unscaled values. This makes it unnecessarily hard to follow the discussion. Please consider replacing the corresponding labels of the colour bar in the Figure by unscaled values (e.g. 3.75 -> 5620 Ωm) or similar to make the information more easily accessible.

Line 382 / caption to Figure 4: Please add meaning of white contour lines (surface elevation).

Line 387: “The baseline SIP imaging results…” please consider adding “along the permanent monitoring profile”

Line 410 / Figure 5:

- Please consider numbering photos in (a) from left to right 1-2-3. It is not clear, why the figures are numbered in inverted order.
- Imaginary resistivity and phase values of 0.5 Hz and 75 should be plotted on the same scale (colour bar limits) to facilitate a direct comparison of these pairs of sections (as discussed in the main text).

Line 419: “including warm (…) and phase (…)” consider adding “in the active layer” (this information is given in the Figure´s caption but would help here in the main text).

Line 423: “with no” -> “with only small”

Line 416: “above” -> “starting at”

Line 449 / Figure 6:

- Can the field data in panel a) be presented in the same fashion as the lab data in panels b)? I.e. could the curves be coloured based on the temperatures measured in the boreholes to facilitate the comparison of field and lab data even more?

- For understandable reasons (comparison with field data), lab data is only visualized up to 100 Hz although it has been measured up to 45 kHz. Please consider providing the complete spectral information in the supplementary material.

Line 459: Please provide more information on why and at which frequency such a peak would be expected (e.g. "…which according to… would be expected around … Hz…")

Line 481 / Figure 7: Could the curves for freezing and thawing be combined in one plot? This would make it easier to appreciate the effect of hysteresis between the two directions of temperature change.

Line 482 / caption of Figure 7: Please provide information on the measurement frequency displayed here.

Line 505 / Figure 8: Is it a fortunate coincidence that the ice-content data and the phase frequency effect data at the borehole position correlate that well (see Figure 13)? Why show the good correlation of the two sub-datasets around the borehole here and discuss the limitations separately later in the manuscript? Neither the ice-content nor the phase frequency effect data is limited to the location around the borehole. Regarding the streamlining (shortening) of the text, please consider discussing this correlation only once.

Line 554: Discussing Figure 10c, the authors state "… we also observe at the field-scale lower ρ', ϕ and ϕFE during freezing (from October to January) than during thawing (from May to July)…" Figure 10c does not really support this statement. At any given temperature around 0 °C, I observe more low values in warm colours (summer months = thawing) than low values in cold colours (winter months = freezing). Please check!

Line 556-559: Redundant: Has already been discussed above.

Line 577-595: Please consider discussing possible sources and effects of data error in a separate subsection (i.e. with a separate title).

Line 596: Delete "so-called"

Line 604-606: Redundant.

Line 626: Please define "R".

Line 641 / Figure 11: Please check equation in panel b) (see comment on equation (9).

Line 701 / Figure 12c): Please consider showing the temperature data only down to 15 m depth to facilitate the comparison with the phase frequency effect data in Figure 12d).

Line 715: It would be helpful for the understanding, if some suitable lab data or modelled data containing the "ice peak" in the frequency range between 1 and 45 kHz was shown earlier in the manuscript (best where it is mentioned/discussed first).

Line 739: "Blacheck" -> "Blaschek"

Line 870: Data availability statement: A link to the mentioned repository should be provided during the review process to allow the reviewers to assess the information provided.

03/07/2023, Matthias Bücker
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