Authors Response to Review 1 (from 22.07.2021)

R: Referee's comment

A: Author's response

General Comments

R: The paper by Mudler et al. is devoted to the problem of ice content estimation in the buried rocks using non-invasive geophysical techniques, namely, the high-frequency induced polarization (HFIP) method. The manuscript is well-structured, rather concise and supported with sufficient illustrative materials. It contains novel interesting results and clearly deserves publishing even without any significant corrections. However, below there are some comments and suggestions, which I believe may help the authors to further improve the scientific quality of the paper.

Specific comments

(1)
R: The main idea of the manuscript is to estimate the ice content in the buried rocks by using the two-component weighted power mean (WPM) model for fitting measured broadband HFIP spectra. For this purpose the authors first invert the observed 2-D HFIP data by means of the conventional Cole-Cole model and then make separate additional inversions of the revealed Cole-Cole response within each model cell by means of the two-component WPM formula. This approach is vulnerable to criticism, since the WPM and Cole-Cole functions could not be generally converted to each other, hence application of the Cole-Cole inversion to the data complying with the WPM model may only introduce additional errors and thus appears to be rather undesirable. If the authors believe that the WPM model is the best choice for quantitative description of the observed HFIP data, then the most natural way of handling them would be direct 2-D inversion for the WPM model parameters, without unnecessary intermediate use of the Cole-Cole function. Consider trying this approach if there are technical capabilities to do so – it may probably yield better results, provided that the non-ice IP effects in rocks are relatively small (otherwise some combination of the Cole-Cole conductivity + WPM permittivity models could be required instead).

A: We agree that the approach of first using the 2-D Cole-Cole fit and afterwards adjust the WPM model appears cumbersome and needs justification.
The reason why this approach is taken is that all available possibilities to extract the two-dimensional distribution of the total spectral information from HFIP data are based on some type of Cole-Cole parameterization. The code AarhusInv used here (Mudler et al., 2019), is not freely modifiable. Alternatively, the code based on the pyGIMLi framework (Günther and Martin, 2016) might be considered, but so far, only the single Cole-Cole model has been implemented. Nevertheless, implementing the WPM directly into the 2-D inversion is exactly our goal for the future.
We added a discussion of this aspect in the results and conclusions section, and we tried to make clearer in the document why it is currently necessary to use the Cole-Cole model first. We understand our approach as a first step of a two-dimensional ice content determination from field data from the single method of HFIP, which does not exist yet.

(2)

R: The choice itself of the employed model and its variable parameters should be discussed in more detail, if possible. For quantitative description of the HFIP response of an ice-bearing rock one may use the 2-component (Zorin, Ageev, 2017), 3-component (Stillman et al., 2010) or 4-component (Bittelli et al., 2004) WPM formula, not to mention the other potentially applicable mixing models, such as that of Hanai and Bruggeman. Why did you choose to employ the 2-component WPM for your data set? Are the temperature and clay content in the frozen layer under study low enough to consider all non-ice sources of IP effect negligible? Is it legit to fix the relaxation time constant of ice as a known value (page 5 lines 27-29), while it could in general vary by several times depending on ice purity and temperature?

A: We use the 2-component model due to its simplicity so that the number of free parameters is initially kept as small as possible. Nevertheless, we agree that one problem is that low-frequency IP effects are not adequately taken into account.
However, since we apply the model to the simple Cole-Cole fit, i.e., with one relaxation, the coexistence of the ice relaxation and IP relaxation of a data set is not foreseen at this stage. Nevertheless, extending this to a 3-component model (extended by water) could lead to better reproduction of effects in unfrozen regions (unfrozen active layer, talik). The intention to increase the model to 3 or 4 components and thus to consider IP effects, as well as the temperature and other aspects of the subsurface, is the goal of an upcoming project. However, at this stage and as a proof of concept, we consider the presented approach as reasonable. We discuss this aspect in a little more detail in the conclusions of the revised version.

R: To answer these and other related questions it should be useful to provide the inversion results for all parameters of the employed WPM model and discuss more thoroughly the quality of data fitting, especially within the ice-bearing cells: the reported average misfit of 20% for amplitude and 0.15 rad = 8.6 degrees for phase (page 16 lines 7-8) appears to be rather high, but there are no illustrations showing how exactly and at which frequencies the actual data diverge from the best-fit model, so it is difficult to understand how the employed model should be modified to achieve better results.

A: We agree that the data fitting should be discussed and presented in more detail. Therefore, we added a section where we present and discuss the fitting of individual spectra in the range of ice-bearing and active layer. Furthermore, during the review process, we revised the procedure of ice content estimation and we are now able to achieve a better fit by imposing limits of the model parameters within the fitting process. The average fitting error for both amplitude and phase shift in the area of the ice-bearing subsurface is now considerably smaller in the revised version. Note that the results of ice content did not change significantly by the modification. We also added a discussion on the sources of the remaining errors and a potential future solution.
We also agree that it would be useful to illustrate the distribution of additional model parameters. Since all parameters would be too much, we focus on presenting and discussing the distribution of model parameter k.

Technical corrections

A: The authors adjusted the technical corrections noted by the reviewer.
Only the remark about linear scaling in fig. 5 of the revised version (fig. 4 in the originally submitted version) has been retained, because the representation of the additional borehole data from the Shestakovka field site by Lebedeva et al. (2019) was chosen identically and thus a comparability should be given.

Authors Response to Review 2 (from 05.08.2021)

R: Referee's comment

A: Author's response

R: Mudler et al. present a case study using high frequency spectral induced polarization (HFIP) data to detect the frozen/unfrozen layer and estimate the ice content in a permafrost environment. The spectral IP data were fitted using an empirical model to extract the complex dielectric permittivity and DC resistivity parameters. These parameters were interpreted to characterize the frozen/unfrozen layer of the subsurface. The parameters were further used to estimate the ice content. While this manuscript matches the scope of the Cryosphere, it contains a few technical issues in terms of the methodologies and data interpretation.

Major comments

(1)
<u>Measurement accuracy of HFIP</u>

R: This study collected SIP data from 2 Hz to 115 kHz. Particularly, high frequency (HF) data in kHz were mainly discussed as it was stated that polarization of ice occurs in this frequency range. However, the measurement accuracy of HFIP was not evaluated quantitatively. It is well known to the IP community that the four-electrode method results in huge errors at high frequencies. It is very challenging and requires extensive procedures to remove HF errors at the laboratory scale measurements. It is even more difficult to collect high-quality HFIP data at field-scale, especially in a high resistivity environment like this study. This manuscript does discuss the HF error topic (only qualitatively) in Section 4, whereas it does not provide any information concerning instrument accuracy.

A: The accuracy of field scale measurements in comparison with lab measurements is a complicated issue, in particular when EM coupling is involved. We tend to disagree with the general statement that it is more difficult at the field scale, as we are not aware of any studies supporting the statement.
However, we agree that errors are an issue for HFIP measurements, in particular at high frequencies. Therefore, we significantly extended the discussion of potential errors in the measurement. We also included a new figure that helps to evaluate the errors.
One panel of the new figure deals with errors caused by capacitive coupling between transmitter cable and the ground (CE coupling) and shows data with and without the CE compensation. The other panel shows an example of reciprocal measurements with the Chameleon-II device, which are generally considered a good measure of data error (e.g. Flores Orozco et al., 2012). In reciprocal measurements, the roles of transmitter and receiver are interchanged, which should theoretically provide identical results if no systematic errors are present. We also provide quantitative error estimates in the revised version.

(2)
<u>The models</u>

R: I found it difficult to follow the SIP models used in this study. Generally, there are four parameters describing the electrical conduction and displacement/polarization: real conductivity, imaginary conductivity, real permittivity, and imaginary permittivity. It is not clear how these parameters were treated, for example, were the conductivity parameters related to the permittivity parameters? Was any parameter neglected?

A: Concerning the choice of permittivity instead of imaginary conductivity, we added the following explanation in the section below eq. (2):
"In general, there is a choice whether the data interpretation is based on imaginary conductivity, or on the real part of permittivity, because the two are mathematically equivalent. Whereas for low-frequency (<100Hz) SIP measurements, imaginary conductivity is often preferred (Loewer et al. 2017), for high-frequency SIP covering the relaxation of ice, permittivity is generally considered (Bittelli et al., 2004)"

R: Specific questions are:
In eq. (1), are ρ and ε_r complex quantities? If so, is ε_r the same as ε_r^*. If not, is ε_r the same as ε_r'?

A: In principle ρ and ε_r in eq. (1) are real values. We reconsidered eq. (1) and decided that it is not necessary for the following explanations. Therefore, we removed eq. (1) and integrated the explanation of the real and imaginary part of ε_r into eq. (2).

R: The description of Eq. (2) is a bit confusing as a Cole-Cole form model does not have the third term. It is reasonable, though, to have the third term to describe the DC conduction, but again the discussion of these parameters is mixed up. It seems that imaginary conductivity was never mentioned, although it is very important for SIP. Besides, as eq. (2) is the key equation for fitting the data, more information is needed to clarify how complex impedance was converted to a complex permittivity.

A: The used model (eq. (2)) is the Cole-Cole model with an additional third term that integrates the low-frequency conductivity mechanisms. This model has been previously used for cryospheric investigations by several publications (e.g., Bittelli et al. 2004, Stillman et al. 2010, Grimm & Stillman 2015). Nevertheless, we agree that the naming of this model as "Cole-Cole model" is misleading and we clarified this in the revised text. Concerning the imaginary conductivity, see the above Authors Comment. The conversion from complex impedance to complex permittivity has been modified in this section, we hope it is clear now.

R: Eq. (3-5). Ice estimation was made based on these equations. On page 5, line 27 states that three parameters are well known and fixed. These parameters should be provided.
I am also curious how the τ_i was selected as it is a temperature-dependent parameter. Also, it would be helpful to describe the meaning of parameter k and present and discuss the variations of fitted k.

A: In the revised version, we now provide the used literature values of the fixed parameters on page 5.
The polarization of ice does indeed exhibit a temperature dependence, but is approximated here in the first approach as independent and with the values for temperatures immediately below freezing point, which is valid for the discussed field survey. We discuss the temperature dependence of the parameters in more detail. Furthermore, we added a section and a figure in which we illustrate the distribution of the fitted parameter k and discuss the meaning of k with regard to Zorin & Ageev (2017).

(3)
Data interpretation

R: The whole Section 6 describes the raw data from a 1D sounding. However, those data are apparent IP data and do not represent the true electrical responses of the subsurface. Nowadays, these data mostly only serve as a way to assess the raw data quality. Therefore, it is not proper to relate them to a physical process and interpret them so extensively (accounting for half of the results), especially for a non-layered structure as evident from the zones around 'C' in Figure 7.

A: We agree that the description of raw data from a 1-D sounding is not common for conventional measurements. Here, they are only discussed because there is little research on the method and its application to ice-bearing subsoils, and we find it useful to illustrate the relationship between the models and the raw data. Therefore, we would like to refrain from shortening this section too much and hope that this is acceptable to the reviewer. However, we will make it clearer that these are apparent HFIP data that do not allow for physical or geologic interpretation.

R: Besides, there are a few specific questions that need to be addressed:
As the polarization of ice is non-metallic polarization, wouldn't imaginary conductivity be a better parameter to interpret to exclude the effects of variations in water conductivity?

A: We follow the typical parameterization describing the ice polarization in the terms of the complex permittivity (see Authors Comment (2)). Possible effects due to water are not explicitly considered at this stage. It would be an approach for further work to use a combined model from the parameterization of several relaxation processes by the conductivity (low-frequent) and permittivity (high-frequency effects), as proposed for example by Loewer et al. (2017). However, since the number of free parameters would increase significantly, and the application and evaluation of HFIP in the field is quite unexplored, the initial approach was to keep the model as simple as possible and focus only on the known ice relaxation. We presented this aspect more clearly in the paper and likewise revisit it in the outlook.

R: According to Bittelli et al., 2004, the $\varepsilon_{r,DC}$ of ice is around 100, and $\varepsilon_{r,HF}$ is around 3. Figure 7 shows that $\varepsilon_{r,DC}$ is as high as 600 in the frozen layer even though the ice content is less than 100%. Figure 7 also shows high $\varepsilon_{r,DC}$ values (~200) even in the thawed layer without ice. It may indicate that the applied complex permittivity model is a good choice as the fitted $\varepsilon_{r,DC}$ is too high compared to the theoretical value (e.g., 100 for ice). Figure 7 indicates that thawed layer exhibit large relaxation giving the large difference between $\varepsilon_{r,DC}$ and $\varepsilon_{r,HF}$. This is contradictory to eq.(4), which states that the permittivity of the ice-free matrix is constant.

A: The large values of $\varepsilon_{r,DC}$ are an artefact of the 2-D inversion code, due to the fact that the procedure assumes a Cole-Cole relaxation model for all areas of the subsurface. If the data show no relaxation process, some the 5 parameters become poorly constrained, which may lead to unrealistic values. Nevertheless, the data can also be fitted in the areas assumed to be ice-free, but the interpretation there should be focused on the reliable parameters, excluding $\varepsilon_{r,DC}$ and relaxation time. We added a brief discussion of this issue in the revised version.

(4)
R: In addition to the above main points, the significance and broad applicability of this manuscript may not be adequate for the Cryosphere journal as the study only has one survey line at one site.

A: Since the HFIP method is not widely used and the approach of determining the ice content at the field scale is new, we initially validated it on only one measurement area with some prior knowledge on ice content. It is logistically quite expansive to gain access to sites where external information on ice content is available and the method can be validated. We hope to generate some interest in our method which will initiate future fieldwork and more case histories.

Remarks under the heading "Some suggestions"

A: We implemented the suggestions noted by the reviewer.

Literature

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