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. This aspect will be integrated in the outlook and we will 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.

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. For this purpose, the fitting of individual spectra in the range of ice will be presented to better understand model and fitting process. We also agree that it would be useful to illustrate the distribution of additional model parameters. Since all parameters would be too much, we will focus on presenting and discuss the distribution of model parameter k.

Technical corrections

A: The authors will adjust all technical corrections noted by the reviewer.