Authors Response to Report #1 (from 02.05.2022)

General Comments
the subject of the article is well adapted to a publication in cryosphere. The estimation of ice content from geophysical measurements is undoubtedly a very important purpose but complex and difficult subject to solve!
The complexity of the problem comes from the physic of the phase change but also from the methods used. The article is well written and the illustrations are good.
It is a pity that the authors did not validate the model with your synthetic cases or with laboratory measurements beforehand. the study site is interesting for the complexity in the distribution of permafrost.

thanks to the authors for this paper.

Reply: We appreciate the generally positive comments. We are aware that we are dealing with a complex problem and we tried to improve the material along the suggested lines.

Specific comments
1) plan and purpose
the distribution of the parts is to be improved. the model used is too little developed whereas the instrumental part is too present.
The development of new instruments and measurement techniques in the field is a very interactive and an important subject but I advise to explain it another paper.
Indeed, the instrumental problems that are mentioned are numerous and are covered too quickly. Moreover, this is not really the subject of the article which is the estimation of ice content. if I am not mistaken.
The presentation of the results is a bit long, especially in relation with the geological context. This long presentation of the results is done at the expense of the discussion which is not sufficiently developed, especially on the chosen hypotheses and on the model.

Reply

The intention of the paper was to outline the entire procedure to estimate ice content at the field scale. We believe that the succesful application at the field scale - as compared to work in the laboratory - is a major advancement. Therefore, we would prefer to keep the instrumental section in the paper; in particular, since some of the material presented here came in as a result of reviewer's comments on the previous version.

We agree, however, that the model might be better discussed and justified, and therefore, we shortened the instrumental section and expanded the theoretical section and the discussion of the model.

2) Bibliography and freezing problem

the theoretical part is to be developed. It is focused on one type of approach without mentioning or discussing other models. Therefore, there is a lack of bibliographic references, especially on other approaches to model the SIP or HSIP response of an icy environment. the focus of the model only on the polarization of the ice leads to a too simplistic model to model correctly the SIP response of an icy medium. at least, you have to justify much more the assumptions.

Reply

We extended the theoretical section, and we now include more references, in particular on low-frequency polarization. Alternative high-frequency models are already mentioned in the theoretical section, and we are not sure which ones are missing.
In order to better justify our assumptions, we tentatively expanded our model to add low-frequency-polarization in an icy environmend using a model published in literature. Using this extended model, we carry out a small synthetic study to show that the low-frequency polarization may indeed be neglected.
We agree that our model might still be extended in future in order to include more processes, and we extend the discussion in the conclusons section to cover this aspect.

Comment
in the introduction of the article it is mentioned that it is necessary to develop a tool to follow the evolution of permafrost melting (or its renewal). a presentation of the thermal process is necessary to add. I am thinking in particular of the frost curve to be presented (e.g. Watanabe and Mizoguchi 2002). It is important to remember that permafrost consists of a solid fraction, a pore space filled with air, water and ice. Depending on the temperature and the initial water content of the soil, the ice content of a soil can be low. I'm not sure the assumption of just focusing on ice polarization is acceptable or shrewd. moreover, along an ERT profile, the lateral variations of lithology necessarily cause a variation in the importance of the polarizations of the medium and of the ice.

Reply
Actually, we are not sure which section in the introduction the reviewer is referring to, as we believe we are not using the word "evolution" in our introduction and do not specifically expand on permafrost melting and renewal anywhere. However, we agree that the aspect of the frost curve, and the fact that ice content may strongly vary close to 0°C is important, in particular in the context of the temperature dependence of the ice polarization. We thank the reviewer for pointing out the corresponding literature. Therefore, we included a paragraph in the corresponding section, and expand the corresponding discussion in the conclusions. We prefer, however, not to include another figure, because this is not quite along the main lines of our present work, and would further expand the material.

3) Model
from the outset, the model used is intended to be high frequency in order to focus on the polarization of the ice. however, there is no mention of the physical processes explaining why there is high frequency ice polarization (Bjerrum effect, interface polarization...). by the way, what is the frequency range of this model? we are talking about high frequency (greater than 100khz) for the ice effect but also at much lower frequencies of the order of 100 Hz. In this case, for low frequencies, the broadband model must integrate the polarizations of Maxwell

Wagner and of the electric double layer. otherwise, one must clearly justify why the polarization of the electric double layer is negligible.

Reply
We add a section to discuss the frequency range of validity and low-frequency polarization. We also add a figure with a synthetic study where we tentatively include polarization of the electric double layer to justify our decision to neglect it. We also point out that Maxwell Wagner polarization is implicitly included in our model (the Zorin and Ageev model), and add a reference on the ice polarization. Nevertheless, we are aware that our model may still be expanded in future to include more processes, and we add a corresponding section in the conclusions.

Comment
the addition of a figure from synthetic data showing the impact of the variation of a parameter on the overall response will be appreciated. Finally, the development of a broadband model is very complex even for a soil without frost and requires validating this model from laboratory measurements. Validation of the model remains to be done. the site studied should rather be seen as an application of this model.

Reply
We follow the reviewer's suggestion to add a figure showing the impact of a parameter variation. In order not to blow up the material too much, we merge with figure with the one used to illustrate the effect of low frequency polarization.
Concerning validation, Zorin and Ageev (2017) did test their model in the laboratory, but we agree that further validation will be useful and now mention this in the paper.

4) Instrumental part
the instrumental part is very interesting, but it could be reduced or make a paper in itself. Adding some advice, benchmarking on acquiring good measurements will be appreciated.

Reply
We are not sure whether the instrumental part would make a paper in itself, as we describe advancements over a predecessor equipment that has already been published. On the other hand, as this is the first published application of this piece of equipment, we believe that a section on instrumentation is necessary. Nevertheless, we shortened the instrumental section where feasible and added some advice on the usage.

5) Results
the presentation part of the site is well built as well as the result part. it would be appreciated to improve legend and caption of the figures in relation to the result (indicate the limits of different area, the active layer, the permafrost..). The result part could be condensed especially in the description of the different areas more or less frozen in order to add a few lines on the processing of the measurements (number of measurements acquired and filtered, frequencies used…) and on the inversion.
what is missing is a more relevant discussion, particularly on the model and the assumptions used.

Reply
We improved figures and captions and condensed the results part. We also extended the

discussion of the model, but actually not in the results section, but in the model description section.

Comments

Abstract:
L4-5: "The High-Frequency Induced Polarization method (HFIP) enables the measurement of the frequency dependent electrical signal of the subsurface"

Could you specify the frequency range?

Reply
Done

Comment
The terms "electrical signal" it is not the most suitable. Maybe specify the name of the parameters that depend on frequency (conductivity and polarization) or the name of the processes (polarization and conduction processes).

L 6: "In contrast to the well-established Electrical Resistivity Tomography (ERT), the usage of the full spectral information provides additional physical parameters of the ground"

additional physical parameters: could you add some examples CEC, clay content, ice content...

Reply
We reformulated that section accordingly

Comment

L 11-14: "Amongst other improvements, compared to a previous generation, the new system is equipped with longer cables and larger power, such that we can now achieve larger penetration depths up to 10m."

The paper is not about the development of a new measuring instrument but rather about a methodology to determine the ice content. Maybe reduce the paragraph on this new instrument and detail the methodology in the abstract.

Reply:
As mentioned above, we also consider the instrumental part and the case history important. Nevertheless, we slightly compacted the instrumental section in the abstract and inserted a sentence on the model.

Comment Page 2
L 5: "The frequency dependent electrical properties of ice have been studied by several authors over the past decades in the laboratory for pure ice as well as for ice within sediment mixtures (e.g. Auty and Cole, 1952; Hippel, 1988; Bittelli et al., 2004; Grimm et al., 2015; Artemov, 2019).
complete some references for example:
Coperey, A., A. Revil, et al. « Low-Frequency Induced Polarization of Porous Media

Undergoing Freezing: Preliminary Observations and Modeling ». Journal of Geophysical Research: Solid Earth, https://doi.org/10.1029/2018JB017015.
Olhoeft, G. R. (1977). Electrical properties of natural clay permafrost. Canadian Journal of Earth Sciences, 14(1), 16–24. https://doi.org/10.1139/e77-002

Reply
We include the two suggested references. A complete list would probably be subject of a review paper, and we would like to keep the list limited to the paper we considered specifically relevant for us.

Comment
L 19: "Attempts have been made to estimate ice content with one method only"
complete some references.
maybe you think about studies like Duvillard, 2018 « Three-Dimensional Electrical Conductivity and Induced Polarization Tomography of a Rock Glacier ». Journal of Geophysical Research: Solid Earth.

Reply
Our formulation was misleading, as we were referring only to the references already included. We reformulated the corresponding section. Duvillard et al (2018) do not make an attempt at quantitative ice content estimation, and therefore we do not include the paper here.

Comment
L 20: "A promising parameter is the frequency-dependent electrical permittivity."
there is also the work of Petrenko, 1993; Petrenko and Ryzhkin, 1997, which is very interesting on the dielectric properties of ice.
Reply
We included the references, but at an earlier section in the introduction.

Page 3 Comment

L 6: "The previous studies were limited to qualitative interpretation with respect to ice content. "
Please add some references

Reply
We reformulated the section. The statement refers to the studies that have been discussed in the previous paragraph, and not to additional literature.

Comment
L 6: Partly due to the lack of penetration depth of the acquisition system, […]
please, explain why?
I do not see why the lack of depth of investigation causes a quantitative interpretation of the ice content. Moreover, the depth of investigation in a resistant medium is not so bad compared to a conductive medium which requires more injection power and larger profile. Can you detail and give examples of study. thank you

Reply
Apparently, our formulation was misleading. Indeed, the penetration depth is not related to the ice content estimation itself. We reformulated the corresponding section to avoid misunderstandings.

Comment
L 19: "It contains the full information about the two material dependent properties of the ground: the electrical resistivity _ and the relative dielectric permittivity "

general remark not requiring change: it is better to speak or express electrical conductivity rather than electrical resistivity in the purely physical or petrophysical sense, i.e., electrical conductivity is really a material property (in the same way as thermal conductivity). Talking in electrical resistivity is right but less relevant from a physical point of view.

Reply
We appreciate the remark, but since the terminology is an ever-ongoing issue which we cannot solve here, we indeed would like to leave the formulation as it is.

Comment
L 23: "Several reasons for polarizability are known, which can be distinguished by their strength and their occurrence in frequency range (Loewer et al., 2017)."

the terms "process" is more correct instead of reasons.

Reply
we changed it.

Page 4 Comment

L 8: "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 10 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)."

Yes, in fact for broad band SIP, this is better to consider the couple (conductivity and the permittivity). Each parameter is related to a specific process. You can find some extra information with Australian group papers like N. Wagner, T. Bore, A. Scheuermann…

Reply
We studied some of the papers by that group, but we are not sure which ones you are referring to. We find that our justification of using both conductivity and permittivity is compact and sufficient, and would prefer not to add more references without specific need.

Comment
L 26: "The inversion leads to the distribution of all five model parameters, […]"

What are these five parameters?

Reply
These are the same five Cole-Cole parameters defined previously around eq. (1), We added a sentence to clarify this

Comment
L 32: "and on physical models, such as the Maxwell-Wagner polarization, (e.g. Kozhevnikov

and Antonov, 2012; Zorin and Ageev, 2017).”
What do you think of approaches that are based on the polarization of the electric double layer i.e. that do not take into account the polarization of the ice (in the strict sense) but its impact on the polarization of the double layer. (see for example coperey 2018, coperey 2021). A broad brand model based only on the polarization of the ice seems insufficient to me, especially if it wants to be broad band model.

Reply
For the purpose of a synthetic modelling study, we now included the Coperey et al., 2018 model and included a new figure to discuss the effect of low-frequency polarization. We discuss why (at the moment) this is not included into the inversion procedure.

Comment Page 5
L 5: “In that theory it is assumed, that the polarization is fully caused by the ice fraction.”
It depends on the frequencies you consider i.e. at 100 Hz you have the beginning of the polarization of the Maxwell Wagner and possibly the end of the polarization of the electric double layer for fine minerals like clay which are also very polarizable.

Reply
See above; we discuss this aspect in more detail in the new section. Note that Maxwell Wagner polarization is implicitly included in our model, which we now mention in an earlier section.

Comment Page 6
L10: “[…] the temperature dependence of the electrical parameters has been neglected.”
the variation of the resistivity is 2%/°C. it is not because you are close to the melting point that you can neglect this dependence i.e. which is also valid below the melting point. Indeed, the dependence is linked to the mobility of the ions and below the melting/freezing point, there is still some liquid water in the medium.
You can neglect this dependence if the system you are studying does not vary in temperature (or very little) or if you are studying at a given time.

Reply
We modified and expanded the section on temperature dependence, including a discussion of a possible temperature change with time.

Comment L10: “Furthermore, other factors such as the clay content and, in general, low-frequency polarization effects were neglected, since the resulting effects in materials containing ice are much smaller than the polarization of ice.”
Please explain. This hypothesis must be really justified and clearly indicated in which case it is valid (e.g. low CEC or clay content, low salinity, frequency range used).
Precisely, neglecting the polarization of the EDL in some cases can lead to a wrong estimation.

Reply
In order to better justify the model, we added a figure with a simulation study where EDL polarization is included. We conclude from the figure that our assumption is justified and discuss under which conditions the assumption may fail. At the same time, we also try to keep the discussion compact in order not to excessively blow up the material. We are aware that our model is the first stage of a possibly long development, and it may not be feasible at this stage to comprehensively discuss all aspects.

Comment Page 6
L23: “Impedance measurements at the field scale with 4-point configurations up to 230kHz pose special challenges on the hardware.”

why this specific frequency?
Is the 4-point measurement still judicious to use at high frequency?

Reply:
The specific frequency is actually irrelevant in this context; it is given by the specific hardware. We rewrote the section to make clear that we need frequencies > approx. 100 kHz. We are not sure about the second comment, as no possible alternative is known to us. We added a line and a reference to explain that 2-point measurements would not be feasible.

Comment Page 12
L9: “The phase shift is shown up to 115kHz, however, for the magnitude, values are only displayed until 60kHz, […]”
if from 60 kHz, the magnitude values have too much error then how can we be sure that the phase shift values are good?
moreover, for the models to be used it is necessary to have the magnitude/phase shift couple. What is then the interest to show the phase shift beyond 60 kHz ?

Reply
The determination of the phase shift and the magnitude rely on different parameters (the phase shift mainly on timing, the magnitude on an absolute impedance) and thus it is possible to obtain different errors and data quality for both.
The interest in showing both lies in the illustration of the spectral behaviour that can be well matched by the theory.
We slightly reformulated the section to consider the comment.

Comment
L18-19: “If we assume electrical parameters resistivity and permittivity as independent of frequency, […]”
why do you want to make these parameters independent of frequency, if this is the case why do measurements in the spectral domain?

Reply
If there is no ice, the parameters become independent of frequency. The section is meant to show that these specific spectra can be explained without ice. One would still make the measurements in spectral domain because otherwise one would not know that there is no ice. We reformulated the section accordingly.

Comment Page 14
L30:” The resistivity (panel a) is the same parameter as determined by other electric or electromagnetic methods in geophysics, for example by ERT, and can be compared with those results.”

not really, the different methods excite the medium in different ways (assumptions, frequencies, processes, volume of soil investigated are different). The profiles will therefore be different. What do you think about it?

Reply

If all methods do what they claim, i.e. determine a resistivity value at a certain location in space, these values should be the same. Of course, the different ways to obtain the value, including those named by the reviewer, but also the choice of inversion, lead to differences. We reformulated the sentence, and added a sentence further below when figure 9 is being discussed, but we do not want to go too deeply into this discussion, as it is slightly off-topic and refers to any comparison between DC resistivities obtained with different methods.

Comment Page 15

L10: “The relaxation time for this layer is in the range of literature value for ice relaxation close to the melting point.”

Please add a value range and references.

Reply:

We added a range and references.

Comment Page 16

L19-20: “The borehole information on frozen and unfrozen state, in combination with the soil moisture measurements (fig. 5) even allows a quantitative assessment of the estimated ice content. The soil moisture in the frozen sections of the borehole can be directly transferred to ice content.”

even when the soil is frozen, there is still a liquid fraction present in the soil. thus, the water content cannot be converted directly into ice content. it is necessary to know the freezing curve (see Watanabe 2005). especially when you are in the active zone

one could possibly admit a direct relation between water content and ice content under the eutectic of water (approximately -21°C) and still, there is the segregation of the salts present in water.

Reply

We are not sure which Watanabe (2005) publication the reviewer is referring to, as we could not find a publication with that particular reference. We assume that actually Watanabe and Mizoguchi (2002) was meant. We agree that the amount of unfrozen water content below freezing is relevant, and added a discussion of this aspect. We now better distinguish between ice content and water content, and also slightly modified our conclusions.

However, as this aspect concerns rather the estimation of ice content from frozen cores rather than the HFIP method itself, we prefer not to go too deeply into this subject, and did not add a figure, for example.

Comment Figure 2:

How do you explain the few percentage differences, especially on the amplitude for the reciprocal measurements? Why did you choose measurements from an alpine site and not from the Yakutia site?

Reply
The dipole-dipole configuration generally does not have the largest signal strength, and also the conditions were relatively harsh at that site.
As mentioned in the paper, we did not measure reciprocals at during the Yakutia survey, and therefore the figure is considered a general performance indicator rather than a specific evaluation of the error. We modified the corresponding sections accordingly.

Comment Figure 5:
what do the colours represent?
Reply
We are not sure what colours the question refers to. the colours in the lithology bare are described in the figure caption, so we do not see what we could change.

Comment figure 6:
the high frequency adjustment is only controlled by the phase?
maybe you can change the colour of the high frequency fit curve for the |z| magnitude and explain its behaviour in the figure legend.
Reply
Yes, the phase shift controls the high-frequency adjustment. We added a sentence in the main text, and added some information in the caption, but we would prefer to leave the figure itself unchanged.

Comment Figure 7:
the text next to the peak is not very relevant (increasing/decreasing peak with depth), it is better to indicate the relationship between the ice content and the peak size

Reply
We changed the figure

Comment
figure 8: Label the different sections in terms of zone (active zone, frozen zone).

Reply
The figure is initially intended to show only the parameters of the inversion. A differentiated interpretation with respect to the layers is given in detail in the text. Since the active zone (max. 50 cm) covers a very small area of the individual sections in the figure and the layers show significant horizontal variations (Permafrost / Talik), we consider an appropriate labeling to be difficult. Therefore, we prefer to leave the figure unchanged.