REPLY to reviewer's comments on "A new model for quantifying subsurface ice content based on geophysical data sets" by Hauck, Böttcher & Maurer (The Cryopshere Discussion)

Fribourg, 8.5.11

Dear Editor and Reviewers,

thank you very much for your considerate and helpful reviews of our submitted paper. We have modified our manuscript according to your suggestions and would like to address these changes in the following.

We hope that we have responded to all comments in a satisfying way and would like to thank you again for your helpful comments and suggestions.

Best wishes,

Christian Hauck and Co-Authors

Replies to comments by Reviewer 1

Reviewer 1, specific comments:
1. P790, L7, "the resistivity of the probe". Does it really refer to the probe? Should it be the resistivity of the material?

REPLY:
changed as suggested

2. P791, L16. I understand fw refers to "water", but it really should refer to liquid water (ice is the solid form of water). Please use liquid water to avoid any confusion.

REPLY:
changed as suggested

3. P792, L7. I suggest changing "insulator similar to the air and the rock matrix" to "insulator similar to the air", to be consistent with the definition of Eqs. (2)-(4), which treat ice as part of pore space, similar to the air.

REPLY:

changed as suggested

4. Figure 3. These figures need scales. I suggest using elevation maps instead of (or in addition to) oblique-angle photographs.

REPLY:

Maps of the study areas are now included

5. P796, L3-11. The description of calculation is too brief and abstract. Please include sufficient details so that the reader can understand how all solutions are calculated. It will be useful to demonstrate an example with graphs.

REPLY:

The paragraph was rewritten to better explain how all solutions were calculated. We also included a figure (Figure 3) for illustration of the dependencies between the 4 phases for the general model.

6. P798, L18. Please include a sentence or two describing the method for temperature measurements and laboratory sample analysis, so that the reader does not have to read the reference.

REPLY:

The corresponding information from the reference was added.

7. P800, L12, “appear larger”. Please indicate values. It looks to be about 15-20% in the figure.

REPLY:

Correct, the values were added as suggested.

8. P800, L16, “smaller than 15%”. Figure 5a shows a blank region below 20-m depth, which is inconsistent with the texts. Please revise the texts.

REPLY:

Changed as suggested

9. P801, L27, “extending over the tongue”. This is inconsistent with Figure 3, which shows the line terminating just after the tongue. Again, I suggest that maps be used in Figure 3, rather than oblique-angle photographs.

REPLY:

Figure (now Figure 5) was changed as suggested

10. P802, L23-24. This sentence is difficult to understand. What does “respectively” refer to?

REPLY:

The wording of the sentence was misleading, and we changed it as suggested.

11. P804. I suspect that the results are somewhat dependent on parameters listed in Table 1. Just to demonstrate such dependence, it will be very useful to include sensitivity analysis. For example, how will the results be affected by uncertainty and spatial variability in Archie’s law parameters?

REPLY:

A new figure (Figure 4) was included showing the sensitivity of the ice content estimation to several of the above mentioned parameters. The largest uncertainty is due to a potential mismatch between the prescribed porosity and the P-wave velocity of the rock material, as was already mentioned in the original manuscript. A corresponding paragraph describing this sensitivity was added. See also reply to a similar comment by reviewer 3

12. After reading the entire manuscript, I felt that the second example from Murtel rock glacier does not add substantially more to the paper. I think that the paper will become stronger if the authors remove the second example and add more information on the first example. However, it is up to the authors to decide if the second example is really important for the paper.

REPLY:

We included both examples to show that the model can easily identify the effect of heterogeneity within a rock glacier (Muragl) as opposed to the often assumed homogeneous subsurface conditions (Murtèl). As we think that the possibility of this distinction is one of the strengths of the possible applications of the 4-phase model, we would favour to keep both examples in the paper.

13. Figure 4. Please explain in the figure caption what the dashed white line in (a) indicates and what the red regions at the top of (b) indicate.

REPLY:

A sentence was added to the caption (now Figure 6).

14. Figures 5 and 6. Please indicate B1 and B2 in the figures for the reader's convenience.

REPLY:

Changed as suggested (now Figures 7 and 8)

15. Figure 6(c). Is the color scale (0-20%) correct in this figure? It will be nice to use consistent color scales in all of (a) – (c).

REPLY:

We use 0-20% for water and air contents to be able to delineate the spatial variability of these two phases (they would be too small in a scale with 0-50 %). On the other hand, ice contents are much larger, which is why we use 0 – 50% for ice. We propose to keep the current scales.

16. Figure 7. Please show only the data discussed in the texts.

REPLY:

The Figure was changed as suggested (now Figure 9).

17. Figure 10. Is the color scale in these figures correct? At a distance of around 150 m, all three phases do not appear to add up to 100%. How is that possible?

REPLY:

We are thankful for the reviewer to spot this error (in the old figure the values were mistakenly divided by the rock content and not by the porosity). We fixed this error and changed the figure accordingly (now Figure 12).

***Replies* to comments by Reviewer 2**

Reviewer 2, General comments:
This in, in general, an excellent and truly innovative paper that provides a novel quantitative method for the geophysical interpretation of high-porosity permafrost systems. In my view, there are only some minor shortcomings in the present version:

(i) The theoretical background could be explained with some more detail with respect to the premises behind Archie's law and Wyllie's / Timur's slowness averaging concept: I think, the reader needs a little bit more background why these concepts were chosen (Introduction) out of a number of possible representations of multiple phase systems (e.g. Carcione and Seriani, 1998) and how this choice affects the possible output of the four phase model (Discussion).

REPLY:

We have chosen these two mixing rules as basis for our first approach because they are one of the simplest representations possible and well known in applied geophysics. Other, more advanced representations (e.g. effective medium or percolation theory), seem to be theoretically possible, but are out of the scope of the present paper (see also response to specific comments below).

(ii) This also applies for the statement that “Sheng 1990 justifies the empirical relations mentioned above for a wider parameter range” – please spend 3-4 sentences to explain the implications, restrictions and problems of the effective medium theory (both Intro and Discussion).

REPLY:

See response above and response to specific comments below.

(iii) Please check the stated volumetric contents of ice, rock and air with the Arenson and Springman (2005, Fig 4) paper – I have partly found different values.

REPLY:

We would like to thank the reviewer to spot this error! The erroneous values for the rock content were corrected (see also response to specific comments below)

(iv) Perhaps this is a little bit meticulous, but one could facilitate the understanding of Equ. 6 and Equ. 7 by homogenising them (see spec. comments).

REPLY:

Changed as suggested

(v) P801 "This confirms the above hypothesis, that the decreasing porosity at larger depths (including the occurrence of firm bedrock) is responsible for the inability of the porosity dependent model to find solutions are larger depths (Figs. 5 and 6)." – There could be another problem which is related to the fact that the slowness average model (Timur 1968 and Wyllie et al. 1958) systematically underestimates the velocity increase in freezing low-porosity rocks: (see specific comments) – please include a short statement on this topic in the discussion.

REPLY:

See response to specific comment below

(vi) P804 "Because the seismic velocities of ice and rock are comparatively similar (Table 1) and their electrical resistivities do not enter the set of equations used in the 4PM a differentiation between ice and 20 rock remains difficult without a priori information." - Please discuss in more detail – the first statement results from mathematical description of the phase change you apply for resistivity (Archie) and seismic velocities (slowness averaging) – please shortly discuss their shortcomings in the description of these phase changes (suggestions, see specific comments)

REPLY:

See response to specific comment below

Specific comments:

P 789 line 8 following: you could be a bit more specific about the deliverables of the 4-phase models and the scientific communities that will profit from the outcome.

REPLY:

A sentence was added which specifies the need for water and ice content estimates.

P790, L10: "for the elastic properties such as the time-average equation for seismic P-wave velocities by Wyllie et al. (1958)" - I personally think that "slowness averaging" is the more intuitive and instructive term describing how Wyllie et al. (1956; 1958) derived their composite "measured" velocities. It could help the reader, who is not so familiar with these papers, if you shortly state the original formula of Archie and Wyllie et al. 1958 in the Introduction.

REPLY:

"time-averaged" was changed for "slowness-averaging".

P 790, L11: "its extension to the frozen phase by Timur (1968). These relationships were originally only validated for a restricted range of materials (e.g. unconsolidated sediments, Zimmerman and King, 1986)." – I am not quite sure about the statement of validation for unconsolidated sediments, since Timur 1968 has used different types of clastic sedimentary, carbonate and metamorphic rocks to develop his theory.

REPLY:

The sentence was rephrased to clarify

P790, L12: "later studies theoretical concepts for simple pore geometries were developed including both electric and elastic properties of the material (e.g., Sheng, 1990), thereby justifying the empirical relations mentioned above for a wider parameter range" - The findings of Sheng 1990 on the application of the differential effective medium (DEM) theory for cemented consolidated materials are a key prerequisite for the development of the 4-phase model. You should consider spending 2-3 sentences on how he modified the existing DEM theory for the application in consolidated "cemented" material as this matches with your problem of transferring unfrozen unconsolidated material to frozen cemented material.

REPLY:

We do not use the Sheng approach (DEM) for our model, except that through this study the empirical approaches of Archie and Timur receive a better physical justification. In order to follow Sheng (1990) we would have to apply DEM theory to all 4 phases, which was way beyond the scope of the present paper.

P792 L12-13: "Since ice is much stiffer than water, the wave velocity is tightly coupled to the ice-to-water ratio." – This is basically the point of Carcione and Seriani 1998 and Timur, 1968 which should be cited here.

REPLY:

Changed as suggested.

P793 L10 "Combining Eqs. (1)–(5) and solving for the ice content." Insert "Volumetric" ice content

REPLY:

Changed as suggested.

P793 L12 "Similarly, equations for the air content fa and the water content fw can be derived" - Insert "Volumetric"

REPLY:

Changed as suggested.

P794 L10 : "The material constants can be taken from literature or can be estimated in the laboratory using field samples (e.g., Schön, 2004)." – Volumetric ice, air and solid content for both of your test sites have been published by Arenson and Springman (2005: Fig 4).

REPLY:

Here, we meant the properties ρ_w, v_r, v_w, v_a, v_i (seismic velocities of the four phases and resistivity of the pore water) and not the volumetric phase contents. The sentence was clarified.

P792 L21: "For a porosity of 0.5 (characteristic for e.g. rock glaciers)" - Arenson and Springman (2005: Fig 4) indicate values raging from 0.4-0.8 for the Muragl – I think that is worth mentioning.

REPLY:

We already discuss the validation of the results with the data from Arenson & Springman in the result section – here, we only show how the model works in principle. We included a range in the revised version indicating that the value 0.5 can only be seen as an average guess.

P793 Equ 7: perhaps this is a little bit meticulous, but you could facilitate the understanding of Equ. 6 and Equ. 7 by homogenising them a little: (SEE SUGGESTIONS IN EQU6AND7.JPG)

REPLY:

Changed as suggested.

P796 L11: "meaning that the model can only determine the sum of ice and rock volumes" – replace volumes by fractions

REPLY:

Changed as suggested.

P798 L18: "Arenson and Springman (2005) found volumetric solid contents between 40–60% within the upper most 15m of borehole BH 4/99 making 50% a reasonable assumption as a mean value." - Arenson and Springman (2005: Fig 4) indicate values ranging from ca. 20-56% for upper 15 m of the Muragl borehole – please clarify

REPLY:

That is correct! We corrected this error and changed the range to 20-56%. The reason we have chosen 50% as mean value is due to the fact that BH 1 does not contain permafrost and we assumed a higher rock content in the left hand part of the profile. As we have no direct information of the porosity from BH 1 and BH 2 (the values from Arenson & Springman were taken from BH 4) and have no way of estimating the spatial variability of porosity at this site we took 50% as a mean value and discussed the results mainly as "relative to the available pore space" (new Figure 7, = Fig. 6 TCD version). The text was changed accordingly.

P799 L26 : "corresponding rock content of around 30–40%," - in (Arenson and Springman, 2005) I find values of 20-40% while the other fractions correspond to what you indicate in your paper please clarify

REPLY:

That is correct, thanks for spotting this error. We corrected the values to 20-40%

P801 L11: "This confirms the above hypothesis, that the decreasing porosity at larger depths (including the occurrence of firm bedrock) is responsible for the inability of the porosity dependent model to find solutions are larger depths (Figs. 5 and 6)." – There could be another problem which is related to the fact that the slowness average model (Timur 1968 and Wyllie et al. 1958) systematically underestimates the velocity increase in low-porosity rocks: see Timur (1968) black shale: 8.2% velocity increase, 3.5% porosity; also reproduced (and ignored) in McGinnis et al. (1973: Fig. 5).

REPLY:

Even though we agree with the reviewer that this underestimation for low-porosity rocks is a severe shortcoming of the slowness-average approach, it cannot be the reason for the discussed model's inability to find solutions, as this was calculated with a porosity of 50%. It may, however, be a problem for a 4PM application on pure bedrock sites.

P803 L 1: "distance 90 m, where high minimal rock contents indicate the presence of a large boulder within the rock glacier." – what about 130 m (x) and 50 m (y)?

REPLY:

See the following half sentence and brackets in the manuscript – we changed the sentence for clarity ("Furthermore, high minimal rock contents below the tongue of the rock glacier indicate the presence of bedrock at 15-20m depth (between horizontal distance 130-150 m in Figures 13d and 13e).")

P803 L5: "Finally, maximum ice contents (_100% saturation, Fig. 10a) are slightly higher for rock glacier Murtèl than for rock glacier Muragl (_90%, Fig. 6a)" – in accordance with (Arenson and Springman, 2005)

REPLY:

Changed as suggested

P804 L18: "Because the seismic velocities of ice and rock are comparatively similar (Table 1) and their electrical resistivities do not enter the set of equations used in the 4PM a differentiation between ice and rock remains difficult without a priori information." - Please discuss in more detail – the first statement results from mathematical description of the phase change you apply for resistivity (Archie) and seismic velocities (slowness averaging) – please shortly discuss their shortcomings in the description of these phase changes e.g. (Carcione and Seriani, 1998; Krautblatter et al., 2010: P12-13). AND Page 805 "An improved formulation of the 4PM, e.g. by using an electrical relationship that includes the resistivity of the bedrock, may overcome this problem." (see above)

REPLY:

We rephrased the two paragraphs mentioned by the reviewer to clarify our reasoning of the performance of the general 4PM approach.

***Replies* to comments by Reviewer 3**

Reviewer 3, general comments:
Quantifying ice, water and air contents in alpine permafrost rocks represents an important aspect for the parameterization and calibration of hydro-thermo-mechanical models aiming at predicting the process dynamics in such systems, for instance with a view to the occurrence of rock falls. While geophysical imaging methods have been proven to provide valuable information in this context, this information (in terms of for instance bulk electrical resistivities and seismic velocities) is ambiguous with respect to the different phases of partly frozen, partly saturated rocks.

The present work demonstrates how the above-mentioned ambiguity can be reduced and estimates of the different rock phases (ice, water, air, rock) can be obtained by using combined electrical and seismic imaging results in conjunction with a new four-phase model approach. By this, the paper represents an important contribution in the field of using geophysical imaging results in a quantitative manner for permafrost rock characterization.
The paper presents a novel approach in the field. It is very well structured, written and appropriately illustrated with figures.

I have the following general comments:

1) A four-phase mixing model approach is not entirely new, but has for instance been used for the description of dielectric permittivity based on the four fractions water, air, rock and clay (extended CRIM model). A reference might be appropriate (e.g. somewhere on top of page 790).

REPLY:

The wording was changed and a reference for the dielectric mixing rules was added

Although the authors do point at several limitations of the proposed approach, I think the following aspects should be more emphasized/addressed in the paper:

2) The four-phase model is given by three equations (Eqs. 1, 2 and 5) involving nine unknowns (f_w, f_r, f_i, f_a, ρ_w, a, m, n, v_r) in the general case (assuming that known values are used for v_w, v_i and v_a, and that v and ρ are determined by seismic/electrical imaging); it thus is inherently underdetermined. While the influence of the key parameters of interest (four phase fractions f_w, f_r, f_i, f_a) is well discussed in the paper, there is little discussion on the empirical parameters in the employed petrophysical relationships. Here, in particular the Archie cementation exponent (m), the saturation exponent (n) and the rock matrix seismic velocity (v_r) should have an effect on the fraction estimates. I find it misleading that the authors refer to these petrophysical parameters even as “constants” (here the wording should be certainly changed to “parameters”; occurs on pages 793 and 794) and that the authors give the impression that these values can be easily picked from the literature or from lab analyses on field samples. In heterogeneous environments, such as obviously those considered in this study, the parameters of rock physical relationships generally vary in space (like the parameters of interest). I agree that spatial invariance might be assumed for practical purposes (and might be justified in certain settings); however, it would be interesting to get a feeling of the influence of such an assumption on the phase fraction estimates. Therefore it would be interesting if not only the sensitivity with respect to the phase fractions would be studied (which is nicely done in the paper), but if the sensitivity study would be extended to the “key” rock physical parameters, or at least this issue would be commented on.

REPLY:

A new figure (Figure 4) was included showing the sensitivity of the ice content estimation to several of the above mentioned parameters. The largest uncertainty is due to a potential mismatch between the prescribed porosity and the P-wave velocity of the rock material, as was already mentioned in the original manuscript. A corresponding paragraph describing this sensitivity was added. Finally, the wording regarding “constants” and “parameters” was changed according to the reviewer suggestions. See also reply to a similar comment by reviewer 1

3) The authors apply rock physical models which are valid for “inherent” conditions (e.g. for samples or borehole logs) to geophysical images which result from a more or less complex inversion procedure. It is well known, in particular for electrical imaging, that the imaged property is systematically “distorted” under the imaging process depending on the sensitivity and resolution characteristics of the imaging method, leading to biased, systematically inaccurate estimates of the imaged property. This issue is of highest importance if the ultimate goal is the quantitative interpretation of the imaged property, like it is the case in the present study. In the field of hydrogeophysics, for instance, there are now a number of studies in which the “correlation loss” of petrophysical relationships in dependence of sensitivity/resolution is being discussed and its effect on inferred petrophysical parameters is studied (see, e.g., Day-Lewis et al., J. Geophys. Res., 110, B08206, 2005; Nguyen et al., Near Surface Geophysics, 7, 377-390, 2009). Although I am aware that a full study of this issue in the present case is beyond the scope of the paper, it yet would be good if the authors would point also at this fundamental problem of the overall proposed approach. So far there is no comment/discussion in this direction.

REPLY:

The reviewer is correct in pointing out the importance of an analysis of the uncertainty inherent in the geophysical inversions that form the basis for the 4PM results. As he also pointed out, a full treatment of this topic would be beyond the scope of the present paper (e.g. an analysis of the relative importance of the sensitivity/resolution of the applied ERT and refraction seismic tomography inversion techniques for the various fractions). But we would like to refer the readers to a few papers on the reliability of ERT and refraction seismics inversion in the context of permafrost research which were published by the authors (e.g. Hilbich et al. 2009, Hauck & Vonder Mühll 2003, Hauck et al. 2003, Maurer & Hauck 2007, Kneisel et al. 2008). E.g., in Hilbich et al. (2009) forward-inverse modelling of synthetic data sets as well as investigations using the DOI index were presented for the Murtel rock glacier site showing the strongly decreasing sensitivity of ERT for regions underneath the massive ice core of the rock glacier. A new subsection (2.3) was added to chapter 2 commenting on these issues.

4) I suggest changing the title to “A new model for estimating subsurface ice content based on combined electrical and seismic data sets”. For me, “estimating” is more appropriate here than “quantifying”, and “electrical and seismic” more specific than “geophysical”.

REPLY:

Changed as suggested

A final specific comment:

On page 798, line 25 it reads: “…, violating the necessary conditions of Eq. (1).” This is confusing to me. I understood that Eq. (1) is used to derive Eqs. (6)-(8). In such a case Eq. (1) cannot be violated. Perhaps this is a misunderstanding, but the authors might want to check their statement here.

REPLY:

The second part of the statement is indeed wrong, all 4 fractions add always up to 1. However, the necessary condition of positive fractions can be violated by negative values for f_i, f_a or f_w. The sentence was changed accordingly.