**The Cryosphere
Discussions**

Interactive comment on "A new model for quantifying subsurface ice content based on geophysical data sets" *by* C. Hauck et al.

A. Kemna (Referee)

kemna@geo.uni-bonn.de

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).

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.

1. 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.

2. 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., NearSurfaceGeophysics, 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.

C1697

3. 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".

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.

With best regards

Andreas Kemna