The Cryosphere Discuss., 4, C67–C81, 2010
www.the-cryosphere-discuss.net/4/C67/2010/

The Cryosphere Discussions

Interactive comment on "Applicability of time-lapse refraction seismic tomography for the detection of ground ice degradation" *by* C. Hilbich

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Received and published: 26 March 2010

| Additional Comments |

Altogether the author presents qualitatively good and interesting data. The observation of temporal variation of elastic waves in alpine permafrost is hitherto unique. From theory we expect that seismic methods show high sensitivity to changes in ground ice distribution. This is shown clearly within this study.

The author begins with a comprehensive overview on physical properties which are mainly related to deep exploration geophysics. The results of these studies are connected to material properties under high pressures and low porosities which might be not the best approach for shallow seismic studies under weathered or fractured

conditions usually found in alpine permafrost. The stated equation (1) lacks due to theoretical reasons since the model imply short seismic wavelengths. However, the result of this study is not affected by this. The seismic data, displayed as wave field, point to a high qualitative data set. The observed temporal variation represented by the data exhibit that the two study areas were well selected. For the analyses of the seismic data the author used adequate methods and developed a strategy for the introduced approach. This strategy should be discussed in more detail and is definitely relevant for future studies focusing on the temporal change of ground ice distribution. Of importance are considerations focussing on the reliability of the final seismic velocity models and the use of additional data (ERT, ground temperature, etc.) to support the interpretation.

The reviewer has the impression that this study includes two different titles such as (A) 'Time-lapse seismic tomography at two alpine permafrost sites' and (B) 'The applicability of time-lapse seismic tomography for the detection of ground ice degradation'. For both studies I would expect more details on the tomography (see comments on p82 15 & P85 21): For the study A I suggest to fairly shorten section 2 'Theory and approach'. In contrast, for the study B I would adapt section 2 'Theory and approach' and give a more general methodical approach as well as to discuss how time-lapse seismic tomography could be applied in the future (e.g the time-lapse interval, seismic designs, spatial sample interval, algorithms, methods for analyses, methods for combining results, etc.). I recommend reworking the section 'conclusion' since it actually did not reflect the major findings of this study.

Altogether I recommend to publish this study in 'The Cryosphere' with minor (A) to major (B) revisions.

| General comments |

In general, the manuscript is well structured. The title 'refraction seismic tomography' could also be interpreted as analyses of head waves generated at a refractor. Since in

this study a diving wave tomography was applied (in Reflex its called refraction tomography) there should be a short notice.

The headline for the chapter 5.3 could be shortened from '5.3 Analysis of refraction seismic tomograms and quantification of velocity changes' to '(Analysis of) time-lapse tomograms'. In general I would omit the term 'Analysis of' for the sections 5.1 - 5.4. I recommend to describe only seismic results in the section 5 'Results' as is indicated by the title of this paper, but not other methods. Thus, I would change section 6 'Interpretation' to 'Interpretation in support of ERT' or 'Interpretation by constraints from ERT'. In this section I would expect to involve results of other methods.

The name of section 6 'Reliability' is unclear (the reliability of what?). I suggest to change this headline in 'Travel time residuals and ray coverage' or similar and to renumber it as section 5.3. The section 7 (Conclusion) mainly covers the previously stated interpretations and should be reworked.

For section 5.2 '(Analyses of) travel time curves'. It might be useful to first discuss the common offset plot (1D information), and then the travel time curves (2D). Thus the commented (p87 25) missing velocities (estimates from 1D-plot) could be stated at this point.

Use in the text and in the figures the same order for the two study sites. There are some figures without any information on the study site or they appear in different order. Further, it is not always clear if the cited permafrost studies as well as the two study sites are related to permafrost in rock or to permafrost in unconsolidated sediments.

Figures 8,9,10 are rather small. Maybe it's better to arrange only 3-4 tomograms in one column and to move the rest into a new column.

| Particular comments |

Abstract:

P78, 7: 'In theory..' I don't understand this sentence.

P78, 10: Seismic tomography monitoring indicates that you continuously observe the seismic wave field. Thus, the term 'monitoring' is not appropriate for this study; you might use 'Time-Lapse Seismic Tomography' or a similar term. If you want to use abbreviations, you might use 'TLST'. 'as an independent and complementary method to ERTM': leave this, almost every geophysical method is complementary to a single applied method.

P78, 15: Although it is difficult to set up a strategy for this new application my thoughts might be useful. I would suggest a strategy such as: (a) Selection of survey interval (monthly, only in summer, etc.) and survey design. (b) Selection of stable excitations points & tests of signal reproducibility. The test of the signal reproducibility could follow at the end of every field survey. (c) Analyses of time-lapse seismograms and travel times. (d) Detection of major processes (e.g shifts due to freezing/thawing processes in the active layer and/or velocity changes). (e) Generation of a (joint) start model (f) Inversion, comparison of tomograms and their reliability.

1. Motivation

p79 15: delete 'Due to its complementary nature,'

p79 20: 'However ... overlapping ranges of P-wave ...'. This sentence is unclear. Try 'Interpretation of stratigraphic details might be difficult since P-wave velocities ranging from A to B indicate either weathered & fractured bedrock or permafrost in sediments'. I can't follow your note on the high measurement effort. If you consider that the analyses of seismic data is carried out by an expert it is not 'comparatively high' in processing effort (with except of travel time picking). For scientific studies I expect that only experts apply such methods.

p80 4: General conditions of what? - Do you mean (general) subsurface conditions? Why do you think that changes in ice/water content are similar or higher sensitive for the method (1) or (2)?

p80 8: ‘Despite ... qualitative stratigraphic interpretation ...’. Doesn’t the term ’stratigraphic’ relate to outcrop sequences or cores? Since you have an indication of the subsurface material by measuring seismic velocities which are correlated to the elastic properties of the subsurface material and its densities (respectively their porosity) another term might be more appropriate (e.g. ’subsurface interpretation ’).

p80 12: see comment (P78, 10), you did not use a monitoring. To use the term ’seismic tomography’ would be much clearer as ’refraction seismic tomography’ which is used for various methods. For another ’refraction seismic tomography’ travel times are picked for a selected refractor which is than used for the inversion. As result a 2D/3D undulating image of the refractor is yielded. Delete ’independent and complementary monitoring method’. You might use ’According to the theoretical suitability of repeated seismic measurements a time-laps (seismic) tomography approach and its potential to observe temporal changes in ice (and water) content in alpine permafrost will be evaluated in this paper’. Which type of permafrost do you investigate? (permafrost in rock or/and in unconsolidated sediments)

2. Theory and approach

p81 15: The results from Timor (1968) are based on samples under high pressures and high frequency acoustic measurements. Thus a comparison with the applied low frequency seismic measurements is doubtful. Further, Zimmerman and King (1986) cites Timor (1968) and his relation (time average equation) to be inapplicable to unconsolidated permafrost sediments.

p81 24: Note that the theoretical assumption for the time average equation (Wyllie et al. 1956) is that the seismic wavelength is « as the fissures or the pores (see also King 2005). You might either discuss here relevant theoretical approaches and if they are applicable for this study or to remove equation (1). Potential approaches are described in Gassmann (1951), Voight (1928), and Reuss (1929). For these models the seismic wavelength is » as the fissures or the pores.

p82 4: Equation 2: see requirements/restrictions for Equation 1. p82 14: see comments on Equation 1/2. p82 15 How does ’time lapse seismic tomography’ work? It is unclear what you mean by the term ’time-lapse?’ You have to explain the new method in general. Then you can show how it is applied for permafrost studies. How seismic tomography does generally works (line integrals along ray paths). How the applied tomographic algorithm (SIRT) does works (FD approximation of the eikonal equation). How do you obtain the RST tomograms and the RST time-lapse tomograms that are shown later?

Figure1: The illustration and idea of showing the major thawing/freezing processes and its effect on simplified ray paths of head waves is well done. But, why are you plotting head waves when you use diving waves for the tomographic approach? I miss that the rays are plotted for a time t1 and a time t2 which is described by the term ’time-lapse’. Note that the model shows two layers with constant velocities v1, v2. In your tomography you use a gradual velocity depth function. The wave fronts (?) near the source seem to be circles (isotropic medium?), leave it.

p82: Is seismic tomography the best method to image a pronounced refractor?

p82 19: ’Necessary conditions ... to detect changes ... include a constant source-receiver geometry’. Delete ’monitoring’. What do you mean with ’constant source-receiver-geometry’? Do you mean that the location of the sources and receivers must be the same over time or that the spatial sample interval is constant? The last part of the sentence is obvious; delete ’and an amount of P-wave velocity that is well above the noise level’.

p82 23: After discussing the theory I would expect to find a description how you want to use time-lapse seismic tomography and how your strategy is. On the basis of the discussed theory you choose a seismic method for the observation of temporal changes in the ice/water content in alpine permafrost. In the case of a strong refractor (shown in Fig. 1) a seismic refraction method (analysing head waves) should provide the best

accuracy to detect vertical changes. Further the 2D refraction method can also be used to determine lateral changes of the refractor velocity. Thus you might explain that a tomographic inversion could smooth the velocity field and thus did not accurately determine discontinuities. At the point where you discuss the strategy it is also important to evaluate the design of the applied method (see my thoughts in P78, 15/Abstract).

3. Site description and data sets

P83 10 Omit the 'M' from 'RSTM' or change it to another abbreviation. Do you really have measurements every $\sim$1.5 months?

P84 6 How thick is several meters? How thick is the debris cover at your seismic profile? Do you have information on the strength of the bedrock? (weathered, jointed or compact). Are there previous seismic investigations at the Schilthorn, and what are their conclusions regarding the seismic velocity field?

P84 14: Why is 5m active layer thickness relatively high? At Lapires you stated a value of 4m, or do you mean for such a high altitude it is a high value?

P84 20 – 23: 'You might use these sentences to describe the strategy of your new application. P84 26 – It is already mentioned that there are borehole temperature data. Do you use this data for validation?

4. Data acquisition an processing

P85 10 Since the profile lengths are not stated in the text a reference to table 1 is necessary at this point. What is the average shot-receiver distance of far shots?

P85 8 How does the boulder surface change after 20 excitations? Does the steel plate cause unintended noise due to air wave events? If yes, you have to mention this.

P85 11 If shot points were located at 1/3 and 2/3 of the distance between two geophones the data are easier to interpret. Such a design would result in more distinct offsets which are plotted side by side and not superposed (see Fig. 7).

Table 1: I don't understand the values stated within the brackets – e.g. 10 Jul 2008 (AL 4/2.5 m). Are this values AL-depths from two borehole locations? If yes, the boreholes need specific numbers (A,B,..) to understand this scheme and to relate them to their location in Fig.2.

Important details on the data acquisition are not mentioned: the temporal sample interval (0.1 or 1ms?), the record length, the instrument, and the type and natural frequency of the geophones.

P85 21: How accurate could you determine the onset of the phases? What are the absolute and the relative accuracy/uncertainty of your picks? The absolute accuracy (maybe $\pm$1ms) should be used to find a limit for the iterations. It would not be useful to model stochastic errors (< 1ms in this case). At this point I would expect more information on the applied tomography: which cell size is used? The applied cell size as well as the number of iterations controls how the algorithm fits your data (minimizing the residuals). Are the data smoothed between consecutive iterations? If yes, you have to state this too. How did you derive the start model? How is the impact of different start models to the result? The set up of the start model is of importance and can highly affect the result of a tomographic inversion. Start models with a high gradient could result in velocity fields where zones with concentrated rays were developed (e.g. refractors). Low gradient start models normally result in rays which penetrate deeper. Do you use the same start model for all time-lapse inversions?

5. Results

P86 3-4: Move this to the descriptions of the data sets. 5.1 Analysis of seismograms 5.1.1 Lapires P86 7: Does unfiltered means unprocessed (without gain, etc.)? P86 9-14: The reproducibility of the signal can not be evaluated after a period of 1.5 months. Use a linear regression for the travel time differences and state the yielded parameters.

Figure 3: The wave fields seem to be in the wrong order (Schilthorn –a; Lapires - b). You might plot the picks and/or their uncertainties over the wave field.

5.1.2 Schilthorn

P86 9: Specify the parameters for a linear regression for the travel time differences. This shift is really pronounced! In Figure 3a there is a clear change in the frequency content of the signal response. How is the change in the frequency content (or wavelengths) of the signal? Further, there are some shots where I can observe phase changes (e.g. 1038, 1026). Are they caused by subsurface changes or due to changed polarity of your geophones?

5.2 Analysis of travel time curves

5.2.1 Lapires

p87 3: '...24 seismograms corresponding to 24 shot points.' This is redundant information, its better to state how many traces you were able to pick (500?) and which type of onsets you have picked (first arrivals).

p87 6: Ok, you are using visual correlation (of the wave field) to minimize the relative pick uncertainty.

p87 8: You have to specify the (shot-receiver) distance for the far shots. '. . . where the signal-to-noise ratio usually decreases' - leave this, its obvious. The term 'constrained picking' is inappropriate, (see p87 6). Note, that the absolute onset of the signal is defined much worse.

Figure 4: The Figure is too small. Is this the study site Lapires? What do the letters A,B,C denote? You have to describe them in the Figure label too. The label should be 'Travel time curves from ...' as used in Fig. 6.

p87 19: Does Fig. 4 confirm the reproducibility of the overall pattern of first arrivals? I don't understand this sentence. I think that the travel time curves imply that the seismic experiment is working properly.

p87 20: 'Note that ...'. Try to use 'unpicked traces' instead of 'missing picks' and remove

the content in the bracket ('due to ... noise ratio'). It is not important why you did not pick all traces.

p87 25: State values for the mean velocities or velocity ranges for the zones A,B,C. p88 24: Specify the parameters for a linear regression through the travel time differences (Fig. 5b) (slope, average value, R^2).

Fig. 5a: delete 'but'. You might use 'sorted by the source-receiver offset' as is denoted on the x-axis. Which study site is here displayed?

Fig. 5b: leave 'calculated', this might be confusing since you later calculate theoretical travel times. p88 26: Yes, I agree on that interpretation.

5.2.2 Schilthorn Fig. 6: The figure size here is ok. Specify the letters A,B,C,D.

p89 6: What has increased? The seismic velocity or the layer thickness - or both? I don't understand why a direct comparison of travel time curves may fail.

p89 10: Introduce Zone A in p89 10. What does 'irregular pattern .. in zone B' mean? Do you want to describe that you can not clearly interpret a 2- or 3-layer subsurface model?

p89 13: Note that your zone D correspond to a near surface area (t < 14 ms, small offsets).

p89 15: Why 'rough interpretation', delete it. The observed travel time (differences) indicate a significant thickening of the (low velocity) near surface layer. Note, that the term ' low velocity layer' also describes a velocity zone with lower values (velocity inversion, negative gradient).

p89 16: delete 'allowing insights into a more differentiated structural pattern in August which was hidden during the frozen conditions in July'.

Figure 7: The figure is too big. I miss the illustration of travel time differences as it is presented in Fig. 5b. Use a linear regression and plot the parameters. How do you

estimate the velocity of 3500 m/s? I would use a linear regression for selected data (e.g. offset from 10 to 50 m). Thus I would expect the dashed line to be located at the centre of the selected data and not at the boundary.

p89 22: What do you mean with systematic analyses of travel time differences? It is of importance to see the travel time differences for Schilthorn as this study site seems to be quite different. Further as interpreted in p88 24 the analyses for a relation between offset and the differences might provide information on shallow or deep changes of the subsurface caused by thawing/freezing processes.

5.3 Analysis of refraction seismic tomograms and quantification of velocity changes

5.3.1 Lapires

p90 5: see comments under 'general comments'. In the section 'Results' I would expect the results of the seismic data which are stated in the title of this study and not results of other methods!

p90 7-14: You might discuss the exploration depth as function of various parameters (also the kind of the selected source) in another section, but not here. Since your July data exhibit a strong refractor you might explain this with the high gradient in your start?- and/or final velocity model.

p90 17: Delete all cells where no rays were passed through. The term 'penetration depth' does not indicate if the signal will be detected by the instruments. Thus other terms, e.g. 'exploration depth' or 'investigation depth' is more appropriate (as used in p90 22).

p90 23: '.. the low velocity overburden (red colours)' add 'zone A', if this is correct.

p90 27: I don't understand why you plot the absolute velocity differences as well as these change as percentage. If both plots are essential I would describe this in the strategy.

p91 1: 'For the interpretation of ...'. This is not the interpretation section. In this section the results of the seismic experiment should be described. For example, at which locations are small/big changes of the velocity field. Which values do they have, and how do they change with time?

5.3.2 Schilthorn

p91 22: see comments under 'general comments'. In the section 'Results' I would expect the seismic data which are stated in the title of this study.

p92 5: Velocities of > 4000 m/s for seasonally frozen ground are really high.

p92 6: You should describe the depth to the refractor and it's the undulations with statistic parameters (average depth $\pm$ standard deviation).

5.4 Reliability

p92 21: Does only 25% of the calculated rays represent the coverage? Which one was not shown? You might plot a cell array where colours denote the cell coverage, as you plot the tomograms.

p92 23: '... is generally high ...' - How do you define high? How many rays pass through high coverage cells? How is your cell size?

p92 24: 'Velocities determined ...'. You may write 'Velocities determined ... have low confidence, but might represent average values ...'.

p93 1: Are you sure that the described total absolute time difference (as explained in the Reflex manual) really does not square the differences and divide them by the number of travel times? If this value isn't divided by the number of travel times, does it make sense?

p93 5: I recommend to state established values to describe the residuals of your models. Thus the results of other studies can be easily compared with this one. Usually the mean value and the standard deviation/RMS are stated for (a) differences of ob-

served and calculated travel times for the start model (b) differences of observed and calculated travel times for the final model.

p93 8: I miss the dates for the stated values 0.46, 0.71, 0.72.

p93 13: It is better to split the sentence at '... of the inversion. The overall uncertainty ...' - At this point I recommend to discuss the absolute and relative pick uncertainty and to compare them with the residuals.

Figure 13: '(a) calculated travel times from the tomographic result' might be much clearer. (b) show a regression line and their equation/parameters.

In the paper calculated travel times (July 2008) are compared to other calculated travel times (August 2008). Do you have compared calculated and observed travel times? You can either do this within the wave field or on receiver-source offset plots.

6. Interpretation

headline section 6: Since you are abundantly use ERT results/data you should change the headline to 'Interpretation supported/constrained by ERT'.

p94 5: This sentence should be either deleted or moved to the conclusion.

6.1 Lapires

Figure 8: The figure is too small. Especially the RST results should be perceptible. The size of Figure 9 is so far ok.

p94 12: If you want to characterize the active layer by seismic velocities you have to state representative values. Or do you want to explain why the observed velocities are low? What do you mean by the term 'intermediate resistivities'?

p94 17: State representative values for both, the low velocities and the low resistivities. p94 22: Do you mean compact bedrock or jointed bedrock? p95 12: '...above stated hypothesis...' Do you mean section 2? If yes, see comment equation 1.

p95 17: delete '(from a geophysical point of view)'. p95 21: Which range of seismic velocities is related to the thawing/freezing front if you use the depth of the active layer from the two boreholes?

p95 25: 'the seismic signal is more sensitive ...' - This sentence is unclear. Is the seismic more sensitive as electrical resistivity or is it more sensitive at temperatures below 0°C as above?

p96 6: delete 'The latter hypothesis ...' p96 7: This seems to be a conclusion.

6.2 Schilthorn

Figure 10: Is this the study site Schilthorn?

p96 11: I don't understand the explanation for the general different image/pattern of the subsurface '(due to their complementary sensitivity to the physical properties of the subsurface)'.

p96 23: These velocities are really high, even if considered that in August significant lower velocities ($\sim$1800 m/s?) were observed.

p97 3: At this point I would confirm the active layer depth by Fig. 14. p97 11: see comment on the term 'penetration depth'.

p97 26: Use an appropriate equation to estimate the effect of ice-filled pores on the seismic velocity. If there are such big changes they might trigger different processes of heat conduction.

p98 14: Can you display the range along the surface in Fig. 9 & 10 where the bedrock is visible. p98 16: see comment on the term 'penetration depth'.

7. Conclusions

p99 5: delete 'under constant measurement conditions' here and explain it in the strategy. p99 6: see comment for the strategy (abstract).

p99 17-27: You have to state the conclusion here, not a summary. I recommend to rather rework this section. Is the introduced method able to describe temporal changes in alpine permafrost? Could this method detect ground ice degradation, and what limitations/advantages does this method have (from p83 7). Which subsurface processes could be studied (thawing/freezing) and how were they detected by the introduced method? Could this method been used to assess the relative ice and water content changes (from p80 14)? Has this method a potential to study interannual changes (from p84 34). You might conclude that the seismic signals showed at both study sites a high sensitivity to small changes in the unfrozen water content below the freezing point (e.g. p95 5:). Finally, the reader want to know if ground ice degradation was detected on the study sites and how the ground ice distribution or its change is characterized at this sites.

Interactive comment on The Cryosphere Discuss., 4, 77, 2010.