The Cryosphere Discuss., 4, C301–C325, 2010
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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: 27 May 2010

REPLY TO REFEREE #2 (H. HAUSMANN)

Many thanks for this comprehensive and very constructive review! I tried to follow your suggestions and revised the paper accordingly. However, it was not possible to realize all of your suggestions for this paper (e.g. according to some restrictions of the applied software, or due to the request to shorten the paper by Referee #1, etc.). Please find below short notices to all comments, and explanations in case of disagreement or misunderstandings.

| 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 focusing on the reliability of the final seismic velocity models and the use of additional data (ERT, ground temperature, etc.) to support the interpretation.

- REPLY: see comments below

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.).

TCD

4, C301–C325, 2010

Interactive Comment

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- REPLY: In my view this paper is a first step towards assessing the applicability of time-lapse seismic tomography for alpine permafrost studies, which are illustrated with two case studies. The aim was not to analyze the permafrost distribution at these two sites in detail (this was already shown in former publications) but to confirm the potential of using time-lapse seismic for analyzing the permafrost evolution. I modified the title and introduction to clarify the aim of the study and to reduce the purely technical focus. Also the description of the tomography was slightly extended (see also further comments below).

I recommend reworking the section ’conclusion’ since it actually did not reflect the major findings of this study.

- REPLY: DONE

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.

- REPLY: DONE

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’.

- REPLY: DONE

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.

- REPLY: DONE

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.

- REPLY: I agree, in terms of interpretation of first 1D and then 2D information the order of Figures 4 and 5 and accordingly the discussion could be changed. However, I have the impression that for glaciologists without a specific focus on geophysics it is easier to follow the reasoning in the given order, because travel time plots are more common and intuitive than source-receiver offset plots. I therefore prefer to keep the order as is.

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.

- REPLY: DONE

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.

- REPLY: clarified

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

- REPLY: Clarified

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'.

- REPLY: the expression was changed to time-lapse refraction seismic tomography

(TLST) in the whole text

‘as an independent and complementary method to ERTM’: leave this, almost every geophysical method is complementary to a single applied method.

- REPLY: DONE

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.

- REPLY: In my opinion, the selection of appropriate survey intervals and shot points (a and b) is a necessary condition for a successful realization of repeated seismic surveys and not part of the evaluation strategy of a time-lapse seismic approach. Similarly, also the detection of major processes (d) are part of the interpretation section and not of the evaluation of the approach. Of course a joint start model (e) was used for the inversion of repeated surveys – this was clarified in the text and described in more detail.

1. Motivation p79 15: delete ’Due to its complementary nature,’

- REPLY: DONE

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’.

- REPLY: The sentence was changed to “However, P-wave velocities range from ca. 2500-4200 m/s for permafrost ice and from ca. 2000-6000 m/s for bedrock (both frozen and unfrozen). These large ranges and their wide overlap make a differentiation of

TCD

4, C301–C325, 2010

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stratigraphic details in bedrock and/or below the permafrost table (e.g. in rock glaciers or talus slopes) often difficult."

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.

- REPLY: Compared to the processing of ERT data the processing effort for seismic data (including travel time picking) is much higher, regardless of the expertise. Similarly, to conduct a high resolution seismic survey in the field with shot points between each pair of geophones, also more man power is necessary than for an ERT survey.

p80 4: General conditions of what? - Do you mean (general) subsurface conditions?

- REPLY: Changed to "general subsurface conditions".

Why do you think that changes in ice/water content are similar or higher sensitive for the method (1) or (2)?

- REPLY: Due to the advantages of the seismic method compared to ERT mentioned in the previous section.

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 ').

- REPLY: DONE

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)

- REPLY: DONE

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 Âń 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 Âż as the fissures or the pores. p82 4: Equation 2: see requirements/restrictions for Equation 1. p82 14: see comments on Equation 1/2.

- REPLY: many thanks for this clarification! In an overview over the different models (with their different limitations) Carcione & Seriani (1998) compared the above approaches, and it can be seen that the (purely qualitative) statements of section 2 are still valid, independent of the model formulation. Since these relations are used here only to qualitatively explain the relation between velocity change and change in ice, air or water content, I rewrote the paragraph accordingly by using a more general assumption and included this additional information.

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 work (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?

- REPLY: the specifics of the tomographic algorithm are not new and are described e.g. in Sandmeier (2008) (see also www.sandmeier-geo.de) and in many studies that applied the software ReflexW, also for permafrost studies (see e.g. Hauck & Kneisel 2008 and references herein). It was felt that a renewed repetition is beyond the scope of the paper. The derivation of tomograms and time-lapse tomograms was already described in the methods and results section. However, I added a sentence for further clarification (no time-lapse inversion was applied but velocity differences of individually inverted tomograms were calculated).

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 ’timelapse’. 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.

- REPLY: Rays are not plotted for t1 and t2 but for 3 different subsurface conditions, which can correspond to different time steps, e.g. Fig. 1a as t1 and Fig. 1b as t2. This is clearly indicated in the Figure caption. The difference to the tomographic approach of REFLEXW was indicated in section 4. The wave fronts were removed from Figure 1.

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

- REPLY: Seismic tomography may be not the best approach to delineate pronounced refractors, but it is the only appropriate method to account for heterogeneous subsurface conditions, which are typical for mountain permafrost. A wavefront inversion for example would be well suited to image a refractor but this method is based on a strict layer concept of the subsurface and is not able to adequately image spatially confined anomalies or lateral velocity changes.

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'.

- REPLY: DONE and clarified

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).

- REPLY: that is correct: a new paragraph was added to explain the pros and cons of the two approaches and the reason why the 2D tomographic approach was chosen.

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 _1.5 months?

- REPLY: DONE (the misleading term ‘interval’ was changed to ‘time span’, since here only 2 data sets with a time span of 40 and 47 days, respectively, are analyzed)

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?

- REPLY: The debris cover is up to 5 m thick, but this depth is only confirmed for the borehole. Previous seismic measurements by Hauck (Hauck 2001) revealed heterogeneous conditions all over the northern slope of Schilthorn with depths to bedrock varying between 0 and 5 m, which is confirmed by a number of bedrock outcrops at the surface. From the debris covered surface appearance it is assumed that there is rather a gradual transition from strongly weathered to firm bedrock than a clear bedrock interface in the ground.

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?

- REPLY: Most alpine permafrost sites at this altitude have active layer thicknesses between 2 and 4 m, thus 5 m is high for an altitude of 3000 m. However, this is not important in this case, and was deleted.

P84 20 – 23: ‘You might use these sentences to describe the strategy of your new application.

- REPLY: see reply to similar comment above

P84 26 – It is already mentioned that there are borehole temperature data. Do you use this data for validation?

- REPLY: Yes, the thaw depths mentioned in the text are derived from borehole temperatures. In addition, for Schilthorn the temperatures are shown for all 3 measurement dates in Figure 14

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?

- REPLY: The reference to table 1 was placed at the end of section 3

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.

- REPLY: In the final data set, there were no problems due to noise from the steel plate or from disintegrating boulder surfaces. In case of these problems in the field, the respective shots were deleted and repeated.

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).

- REPLY: this is a very good idea, and will be used for future measurements. Thanks for this comment!

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.

- REPLY: AL stands for active layer thickness and is mentioned in the caption. In addition, the respective active layer thicknesses are indicated in the tomograms – The missing details on data acquisition were added to the text and the table.

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 _1ms) should be used to find a limit for the iterations. It would not be useful to

TCD

4, C301–C325, 2010

model stochastic errors (< 1ms in this case).

- REPLY: I am not sure what exactly the reviewer means with ‘absolute’ and ‘relative’ uncertainty of the picks? However, I would estimate the uncertainty to lie around 1 ms for Lapires and between 0.1 and 0.5 ms for Schilthorn. As I explained in the text, I think that the accuracy in the identification of the first arrivals significantly improves if time-lapse data sets are analyzed jointly (see Figure 3) since systematic shifts of the first arrival can be identified and distinguished from noise. The pick uncertainty was not used as a convergence criterion in the tomographic inversion up to now, but I want to thank the reviewer for his suggestion, and I will consider this for future studies.

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.

- REPLY: DONE

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?

- REPLY: For both sites the start model was defined based on the qualitative estimation of velocity ranges from the travel time analysis, i.e. at both sites the gradient model starts with 400 m/s at the surface and increases with a gradient of 400 m/s at Lapires and 600 m/s at Schilthorn, resulting in about 4000 m/s and 6000 m/s respectively at 10 m depth. According to Lanz et al. (1998) I used a relatively high gradient, to ensure sufficient ray coverage. However, I also tried start models with much lower gradients and did not observe significant changes in the result. Especially for the Schilthorn data

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set from July 2008, also with a gradient of only 100 m/s with depth the same (shallow) depth for the refractor was obtained. The same (site-specific) start models were used for all time-lapse data sets. This was clarified in the text.

5. Results P86 3-4: Move this to the descriptions of the data sets.

- REPLY: DONE

5.1 Analysis of seismograms 5.1.1 Lapires P86 7: Does unfiltered means unprocessed (without gain, etc.)?

- REPLY: yes

P86 9-14: The reproducibility of the signal cannot be evaluated after a period of 1.5 months. Use a linear regression for the travel time differences and state the yielded parameters.

- REPLY: I did not mean to use the term "reproducibility" in a statistical sense. I changed this paragraph accordingly.

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.

- REPLY: the order was corrected, and the picks were plotted on the seismogram

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?

- REPLY: see comment further below. No statistical correlation was intended because of the strong changes in the subsurface (change in thaw depth by 3m, Fig. 14) causing also a considerable change in ray paths. The observed phase changes are most probably due to a different evolution of the properties of the heterogeneous subsurface, as the polarity of the geophones was not changed.

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).

- REPLY: DONE

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

- REPLY: yes. see also comments above

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.

- REPLY: shot-receiver distances were already indicated in Table 1. The phrase 'where the signal-to-noise ratio usually decreases' was deleted. The term "constrained picking" was written in quotation marks to avoid overvaluation, and is kept since I think it is still a good way to describe the advantage of having similar data sets for the identification of the first arrivals in critical cases.

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.

- REPLY: DONE

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.

- REPLY: the sentence was changed to “The consistent pattern of these curves for both dates confirms the applicability of the time-lapse seismic approach in case of small to medium changes in the subsurface.”

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.

- REPLY: DONE

p87 25: State values for the mean velocities or velocity ranges for the zones A,B,C.

- REPLY: DONE

p88 24: Specify the parameters for a linear regression through the travel time differences (Fig. 5b) (slope, average value, R2).

- REPLY: see comments above – no statistical evaluation was intended due to the heterogeneous ground conditions. As different travel time differences are caused by different materials (with different properties) they cannot necessarily be attributed to the same layer and/or material. A quantitative regression would indicate an accuracy that cannot be drawn from these results. A sentence was added in the Figure caption for clarification.

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?

- REPLY: DONE (also “...from Lapires. . .” was added to the figure caption)

Fig. 5b: leave ’calculated’, this might be confusing since you later calculate theoretical travel times.

- REPLY: DONE

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

- REPLY: DONE

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.

- REPLY: clarified in the text

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?

- REPLY: Zone A is indicated in the figure caption. Irregular pattern means a much more heterogeneous travel time pattern than for Lapires (Fig. 4), which is attributed to a pronounced topography of the refractor, as described in the text.

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).

- REPLY: 'rough' was changed to 'first'.

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

- REPLY: DONE

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.

- REPLY: see comments above (Figure 5). For the travel time differences see next

TCD

4, C301–C325, 2010

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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.

- REPLY: The plot with the travel time differences for Schilthorn and the corresponding discussion was originally not shown to reduce the length of the paper, but was now added according to this request. The unclear sentence about the systematic analysis was thus removed.

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!

- REPLY: I agree, and the ERT results are actually only discussed in section '6 Interpretation', but introduced already in section 5 since they are plotted together with the RST tomograms. I included a note, that the ERT results will be discussed later, but I still prefer to plot RST and ERT results together for better comparability.

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.

- REPLY: in my opinion it does not make sense to discuss this earlier in the data acquisitions section, as the problem becomes only evident in the tomograms. Also, the gradient of the starting model was not too strong to cause this strong refractor, rather do I observe similar problems at other sites, which is why I find it important to discuss

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this in combination with the tomograms.

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).

- REPLY: DONE

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

- REPLY: DONE

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.

- REPLY: that is true, one could omit the relative changes in percent. They were included for better comparison with the relative changes in the ERT data (also in percent) and to better highlight lateral differences in the subsurface material (higher percentage changes can indicate higher porosity and/or initial ice content and vice versa). But to shorten the paper, I decided to remove this sub-figure and the corresponding paragraphs.

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?

- REPLY: changed

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.

- REPLY: see response to similar comment before: I included a note, that the ERT

TCD

4, C301–C325, 2010

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results will be discussed later.

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

- REPLY: yes, I agree. I assume that this is an effect due to the strongly (and deeply) weathered state of the limestone schists (mixture of very weathered and jointed bedrock), i.e. that in frozen state the rather high velocities of unweathered limestone schists dominate, while in unfrozen state the air (or air and water) filled joints strongly reduce the bulk velocity of the material. A corresponding sentence was added.

p92 6: You should describe the depth to the refractor and its undulations with statistic parameters (average depth _ standard deviation).

- REPLY: Sorry, I do not understand why this should be useful or what new information can be gained from a statistic description of the depth of the refractor.

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.

- REPLY: I plotted only 25% of the rays, because the ray density is so high(!), that the velocity field is not visible any more, when plotting all rays. The number of rays to be plotted is a factor in REFLEXW, thus I did not manually remove specific rays. Instead, every 4th calculated ray is plotted, which guarantees a uniform distribution. I personally prefer the ray paths over a plot of cell coverage, but if explicitly wished, I could change this.

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

- REPLY: The cell size was indicated in section 4. 'Generally high' is meant qualitatively and points to the relatively uniform ray coverage in most parts of the tomograms (except for the regions mentioned in the text) indicating a rather uniform quality/reliability of the model. Accordingly, this qualitative measure is supposed to be a sufficient measure for

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4, C301–C325, 2010

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the ray coverage. See also comment above: if explicitly wished, I can of course change the plots with the ray paths to plots indicating the cell coverage of the rays.

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

- REPLY: I do not see a difference in the two versions.

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?

- REPLY: this is correct: the total absolute time difference is not just the sum of differences between observed and calculated travel times, but the sum (not the square!) divided by the number of travel times. Thanks for this notice. The text was changed accordingly.

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

- REPLY: unfortunately, the software REFLEXW does not provide these measures at the moment (Sandmeier, pers. communication).

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

- REPLY: clarified

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.

TCD

4, C301–C325, 2010

- REPLY: The sentence was split. Concerning the absolute and relative pick uncertainty, see my comments further above. Unfortunately, the software REFLEXW does not provide travel time residuals at the moment (Sandmeier, pers. communication).

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.

- REPLY: figure caption was changed. Yes I compared observed and calculated travel times but did not include the plot, as the paper was already too long. However, a rough comparison is possible by visual inspection of Figures 5 and 13, an extra Figure would not give additional information. For the regression line see comments above (Figures 5 and 7).

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

- REPLY: DONE

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

- REPLY: DONE

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.

- REPLY: Figure was divided into two parts, as in Figure 9

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'?

- REPLY: clarified

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p94 17: State representative values for both, the low velocities and the low resistivities.

- REPLY: DONE

p94 22: Do you mean compact bedrock or jointed bedrock?

- REPLY: firm bedrock (gneiss) would have much higher velocities up to 5000 m/s or even more, but the transition between the talus slope, and the weathered bedrock surface underneath is expected to have values on the order of 3500 to 4000 m/s, then slowly increasing with depth.

p95 12: '...above stated hypothesis...' Do you mean section 2? If yes, see comment equation 1.

- REPLY: in my opinion a different mixing rule approach (e.g. by Reuss) would not change this qualitative interpretation. I am convinced that the heterogeneous observation of thaw processes in the profile is due to the heterogeneous permafrost distribution, which is controlled by the block sizes (coarse blocks in the central and right part, fine material in the left, non-permafrost part of the profile) and not due to varying material characteristics which would change the result of equation 1. However, I deleted the link to the above stated hypothesis.

p95 17: delete '(from a geophysical point of view)'.

- REPLY: DONE

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?

- REPLY: at the borehole, thawed conditions correspond to velocities <1600 m/s, and increase accordingly below under still frozen conditions.

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?

- REPLY: clarified

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

- REPLY: I do not agree. This is the interpretation section of the paper, and I prefer not to separate important aspects of the discussion into different sections.

6.2 Schilthorn Figure 10: Is this the study site Schilthorn?

- REPLY: yes, clarified.

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)'.

- REPLY: As ERT is sensitive to electrical properties (resistivity) and seismic refraction the elastic properties (related to the P-wave velocity), the patterns obtained by the two methods shown in Figure 9 do not have to be the same (e.g. through different effects of unfrozen water or air inclusions). However, relative changes are in both cases related to phase changes from ice to water, and can therefore show similar signals. The sentence was clarified.

p96 23: These velocities are really high, even if considered that in August significant lower velocities (_1800 m/s?) were observed.

- REPLY: see reply to the same comment in section 5.3.2 – the sentence was clarified in a similar way than above.

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

- REPLY: DONE

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

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heat conduction.

- REPLY: The link to equations (1) and (2) was removed here and changed to a more general link to the descriptions in section 2, which were adapted according to the comments further above.

p98 14: Can you display the range along the surface in Fig. 9 & 10 where the bedrock is visible.

- REPLY: was indicated in the text (not in the figures)

p98 16: see comment on the term 'penetration depth'.

- REPLY: changed to investigation depth

7. Conclusions p99 5: delete 'under constant measurement conditions' here and explain it in the strategy.

- REPLY: DONE

p99 6: see comment for the strategy (abstract).

- REPLY: deleted here, according to reviewer 1

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.

- REPLY: DONE

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

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