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The Cryosphere
Discussions

Interactive comment on "P-wave velocity changes in freezing hard low-porosity rocks: a laboratory-based time-average model" *by* D. Draebing and M. Krautblatter

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We thank David Amitrano (Ref. 1) and the anonymous referee (Ref. 2) for their valuable comments and helpful suggestions. First we correspond to general comments before we answer the detailed comments.

General comments of Ref. 1:

Ref. 1: Unfortunately the authors do not directly compared their model with the data so the relevance of this new model is not clearly shown.

We added a new figure (Fig. 4) to show the offset between the two-phase equation

(Eq. 2) and our measured results and explain the offset as a result of increasing matrix velocity. Figure 4 plots P-wave velocity (vp) and matrix velocity (vm) increase due to freezing against mean effective porosities for six different rock groups. P-wave velocity increases A) parallel to cleavage or bedding and B) perpendicular to cleavage/bedding, the dots are measured values and the quadrats are values calculated using Eq. (2). Matrix velocity increases C) parallel to cleavage or bedding and D) perpendicular to cleavage/bedding, the dots are values calculated with Eqs. (13) and (14) and the quadrats are the values assuming no matrix velocity increase according Timur (1968). The demonstrated outcome is also included in the results and the discussion section.

Ref. 1: An interesting point is that the authors observed an increase larger than the one explained by the water phase change. This excess of velocity change is explained by an increase of the matrix velocity associated with the freezing. This effect is well observed but not clearly explained in the manuscript. I suggest the following explanation based on laboratory observations in absence of freezing. It is commonly observed that the p-wave velocity increases when the confining pressure or the uniaxial stress increases (e.g. Wassermann e al 2009, Heap et al, 2010, Eslami et al 2010 and ref. herein) when the stress does not surpass the damage threshold. This is generally explained as the effect of the closure of crack parallel to the major stress. This is valid only when the stress is below the stress corresponding to the onset of damage after that the damage increase and the p-wave velocity decreases.

We added the suggestions and references to the manuscript. "P-wave velocity will increase due to decreasing porosity if the confining pressure does not surpass the damage threshold and porosity increases due to microcracking (Eslami et al., 2010; Heap et al., 2010; Wassermann et al., 2009)."… "Stress increase due to loading can preferentially close pre-existing microcracks perpendicular to stress direction and decreases anisotropy (Eslami et al., 2010; Heap et al., 2010; Wassermann et al., 2009). However, stress increase can also lead to preferential opening of axially orientated microcracks (Eslami et al., 2010) or microcrack generation due to threshold surpassing (Heap et al., 2010; Wassermann et al., 2009) which then enhances anisotropy."..."A surpassing damage threshold or opening of microcracks could explain anisotropy increase."

Ref. 1: As the authors indicate, the pores concerned by the water freezing porosity are only the ones hydraulically connected. So the non connected porosity only reacts to the changes of stress. So one may consider the following mechanism: when freezing, the pore pressure increases in the connected porosity inducing the increase of stress applied on the matrix (including the non connected porosity) and the closure of the non connected cracks imbedded in the matrix. In order to confirm the possibility of such mechanism it is suitable that the authors provide the partition between connected and non-connected porosity for the various rock types they examined and to plot it against the matrix velocity increase. In addition a direct confrontation between their model and their data should be added to convince the reader that their model is better explaining the data.

This comment is very valuable. The distinction of connected and non-connected pores was not possible because necessary methods were not available and we think this is beyond the scope of the present paper but a very helpful suggestion to further work. The referee's suggestion would enable a better understanding and a quantification of the process. This should be addressed in further work. We added to the manuscript: "To distinguish quantitatively connected and unconnected pores will help the interpretation but necessary methods were not available."... "To evaluate pore form by porosimetric analyses and to partition connected and non-connected porosity would enhance a more quantitatively interpretation and should be done in future research."..." The way ice-pressure is effective depends on the pore form of connected and non-connected pores. A quantitative analysis needs to distinguish between connected and non-connected pores."..."... calculating matrix velocity with absolute porosity values would change matrix velocity only by 2 $\pm$2 %, which is well below the accuracy within the clusters."

Ref. 1: If available the porosimetry (i.e. the distribution of pore size, obtained by

mercury injection or other technics) could be also very helpful for understanding the differences between rocks. A more detailed description of rocks, in particular the nature of the anisotropy could be also suitable.

This comment is definitely true. A porosimetric differentiation would enable an understanding which pores react and how. Methods for porosimetric differentiation were not available and should be incorporated in further studies. Lithology was used as a proxy for pore shape. A more detailed description of rocks and their nature of anisotropy were added. Added to the manuscript: "To evaluate pore form by porosimetric analyses and to partition connected and non-connected porosity would enhance a more quantitatively interpretation and should be done in future research."

Ref. 1: An important question is the possible induce damage due to freezing. Does the velocity changes when comparing the unfrozen rocks before and after the freezing sequence? In other words, is there any damage or increase of porosity induced by the freezing?

This question is important. The samples which we have tested did not show a significant change in p-wave velocity after rethawing. Matsuoka (1990) used this method to evaluate freezing damage of sandstone and tuff samples. Low-porosity samples like our samples show no significant p-wave velocity decrease after one freezing cycle. We think that the freezing damage in these hard low-porosity samples that have already undergone repeated freezing is negligible. High-porosity samples like the Tuffeau Limestone show p-wave velocity decrease after one freezing cycle and freezing damage cannot be excluded for this single sample.

Ref. 1: Another important remark concerns the building of the time average model (equation 1). It is important to indicate that this model consider an assembly of matrix and pores in series along the wave propagation direction. This is a good approximation for an anisotropic rock with waves propagating perpendicular to the anisotropy direction. But it is no more valid pour a random porous media or when the waves are propagating parallel to the anisotropy. This must be discussed in the introduction as it one of the reasons why such model does not apply to all the rock types.

Thanks for this valuable comment. We added a short description of the time average model in the introduction. We put the focus of the interpretation to calculated matrix velocities in perpendicular direction to fulfill the model requirements. For parallel measurements, results for calculated matrix velocity are given with constraints to their interpretation. Added to the manuscript: "Calculations of p-wave velocities parallel to cleavage or bedding reflect this offset trend but are violating the seismic ray assumptions of the time-average equation and should be used with cautiousness for parallel velocities."

General comments of Ref. 2:

Ref. 2: 1. You use 22 samples from 15 different field sites – which is quite a lot compared to other studies, but still very small if you want to draw conclusions for different lithologies in a general way, like you do.

This is definitely true. We use 22 samples but this is still far from a complete coverage of alpine lithological heterogeneity (the same would be true for 220 samples). However we have systematically chosen samples to represent all dominant types of lithology and lithology is a systematical proxy of pore shape (Takeuchi and Simmons, 1973; Toksöz et al., 1976). To better emphasize this in the text we have included statement in the discussion that outlines that these general values have to be tested with further specimens of different lithologies. However, these first approximations already show some systematical insights such as the high drop in anisotropy of schists where lithology is a necessary proxy to explain the behavior. We have changed the statement at various points to outline that further research is needed. Added: "Drawing general conclusions and transfer our model to other as mentioned rock types should be applied with caution due to the restricted number of samples."

Ref. 2: Your values for e.g. carbonate rocks are based on 2 samples with very different

properties (to calculate a mean porosity value (Table 1) does not make any sense to me), for clastic rocks you have only 2 samples from the same site, similarly for volcanic and plutonic rocks. Can you really generalize the results for all volcanic rocks based on two rock samples of Präg?

This is an interesting comment. If we arrange rock samples according to porosity we gather very different lithologies and pore shapes in a cluster – if we arrange rock samples according to lithology we gather different porosities in a cluster. But as has been stated above the lithological diversity even within one type of lithology is big and can of course not be fully represented by two samples – however, this is how we have to start. We included a remark saying that especially volcanic rocks and carbonates are very difficult rock types for a generalization.

Ref. 2: What about the representativeness of the samples for the (permafrost) field sites: I guess they were taken from the surface: are they representative for permafrost conditions at larger depth? I suggest a revision of the general wording towards less absolute statements.

This is an interesting comment. A rock specimen from the top or inside a permafrost rock exposure has been exposed to enhance periglacial weathering no matter if it was inside or at the surface of a rock face. We have taken samples from permafrost sites because periglacial weathering could possibly affect pore shape and connectivity due to ice-segregation pressure and other processes which occur at the surface and presumably up to 10 m depth (Matsuoka and Murton, 2008).

- Blocks sampled at the surface of permafrost rock walls: Over the course of the Holocene (assuming a rockwall retreat rate of 0.1 mm/year), samples that we presently find on top of exposures have presumably been located at permafrost depths; and permafrost has occurred there over the course of the last 2 Myrs. Very likely, all samples have experienced periglacial and permafrost conditions over most of the last 2 Myrs.

- Blocks from talus slope and scree slopes: These have also been exposed to permafrost conditions in the last 2 Myrs: As you recommended, we have done both, added Table 1 that exactly states their origin – and we found a reference for all sample origins, that describes permafrost at these sites.

Ref. 2: 2. Where do you put the limit of low-porosity rocks? To phrase it differently: it would be good to quantitatively calculate for which porosity ranges your relation applies, and from which porosities onward the effect of the pore liquid becomes important. This you could measure, by analyzing rock specimen with higher porosities, but you could also calculate it using Eqs. (2) and (16), respectively. Similarly, you could analyze the effect of saturation by measuring dry rocks (see comment below) or calculate its effect using Eq. (3). An extension of the present study in this direction would certainly be interesting, if such data are available!

We will use the definition by Tiab & Donaldson (2004). They set the upper threshold for low-porosity rocks to 10 %. Porosity in their sense is absolute porosity. Effective porosity is always smaller than absolute porosity. Added:" Alpine rock cliffs in permafrost regions mostly consist of hard low-porosity rocks (<10 %), according to Tiab and Donaldson's (2004) definition, and the applicability of geophysical methods to these is yet unclear." Regarding the comment on the range of application in terms of porosity of our model – we try to show this in the new Figure 4. Fig 4A and B show the overall change of p-wave velocity which combines the pore infill and matrix velocity increase: Here we show the calculated results from the time average equation (Eq. 2) and the offset to the measured values. This offset is explained in Fig 4C and D, which only shows the matrix velocity increase. Matrix velocity changes of several hundred m/s are presumably important for all porosities, even if their proportional impact is greater for small porosities. Added: "All measured rock samples show significant matrix velocity increases vm due to freezing except one gneiss sample (X5). The results of Eq. (14) are plotted against mean effective porosity for the six rock groups (Fig. 4C and D). Timur (1968) expected no matrix velocity increase due to freezing. The zero increase is incorporated in the figure. The difference between calculated matrix velocity increase and non-increase

is in the same order than the offset between measured and calculated p-wave velocity increase due to relative small porosity values." The effect of saturation would in our opinion be better addressed in a separate paper since homogeneous partial pore saturation is itself difficult to generate.

Ref. 2: 3. Why did you only measure saturated rocks? For a comparison with Timur's equation, as well as for the confirmation of your statements regarding ice pressure being responsible for reduced anisotropy and P-wave increase upon freezing, a measurement of P-wave velocity change (upon freezing) for dry rocks would be important.

There are two reasons: First we must assume saturated permafrost rocks in alpine conditions: All rocks below the upper 10 cm from the rock face must be considered as saturated (Sass, 2005). We think that for field measurements in Alpine permafrost rocks, values under laboratory-dry conditions (24h at 105°C) are not relevant. Secondly, Timur (1968) stated that under laboratory-dry conditions no apparent changes in p-wave velocity occur due to freezing.

Ref. 2: 4. Details of rock samples and the method of obtaining them are not given (neither in Table 1 nor in the methodology or otherwise)! How did you get them? Are they representative for permafrost conditions in the subsurface? Are they related to proven permafrost conditions (do papers/references exist)? Do field measurements of seismic velocities exist and how do they compare with your data? Some of the names given in Table 1 are referring to known permafrost sites in the European Alps, and for many of them published seismic field data exist. Are the samples relating to these or is this coincidence?

This is very good suggestion. We have included a table stating (i) location, (ii) conditions at the study site (rock wall, talus slope...), (iii) sample origin (surface, rock wall), (iv) lithology, (v) short geological description, (vi) porosity and degree of saturation, (vii) published data on permafrost conditions at the sample location.

Ref. 2: 5. How and with which accuracy did you derive density, which apparently is an important factor in determining the effective porosity?

We have included a detailed statement on how density is derived. “Rock density is derived from Wohlenberg (2012).” Wohlenberg (2012) delivers a range of density for different rocks. We calculated the effective porosity using the minimum and maximum of the density values. After that we calculate the mean effective porosity and mean deviation of each rock sample. Mean deviation shows the sensitivity of effective porosity calculation due to density. The highest mean deviation value possess the Tuffeau Limestone sample but this derives from the high water absorption capacity (WA = 17.04). The high WA is a result of the high porosity. All the low-porosity samples possess low WA (mean value 0.80$\pm$0.40). These samples are not sensitive to density and high differences of density values result only in small mean deviations of effective porosities.

Ref. 2: 6. Comparison with least-square fit shown in McGinnis et al. (1973): I do really not understand your focus on the data shown in Figure 5 of McGinnis et al (1973): This is only a least-square fit (a straight line through some points) of published data, which incidentally results in Delta_Vp = 0 for a porosity = 0.0363. This fit was used for a case study in Antarctica and was surely not intended to provide valid data for very low porosities, otherwise they could have easily constrained it to Delta_Vp = 0 for a porosity of 0. It is quite unfair to cite this equation in this context! As stated below, I propose to omit Eq. (4) and focus on the much more relevant Eq. (3) with its special case (2). If you really want to compare your data with the data of McGinnis et al. (1973), then you should constrain their “model” (which it is not really) with the boundary condition of Delta_Vp=0 for a porosity = 0 and see how that compares in your Figure 3 (which is not really discussed in the text anyway). I am not sure that the results would be so different.

Thanks for the comment. We have shifted the focus to the time-average equation and omitted most citations of McGinnis et al. (1973). However, we must at least refer to McGinnis et al. (1973) because his work has frequently been cited in this context.

Technical remarks, typos and comments:

1 Abstract

Ref. 2: p794/l.9: you use different "definitions" of low-porosity (here : <6%, p795/l.20 : <5%): be more specific whether you mean your measurements or low-porosity rocks in general; for the latter: does a limit exist?

We will use the definition by Tiab & Donaldson (2004). They set the upper threshold for low-porosity rocks to 10 %. Porosity in their sense is absolute porosity. Effective porosity is always smaller than absolute porosity.

Ref. 2: p794/l.10: permafrost rock samples: as mentioned on p805/l.15-16 these are not in situ bore cores, but samples taken (I guess?) from the surface. This means they are not really permafrost samples (they were not conserved in frozen conditions), but rock samples from permafrost areas. This should be changed.

Changed to: “. . .metamorphic, magmatic and sedimentary rock samples from permafrost sites with a natural texture (>100 micro-fissures) from 25°C to –15°C in 0.3°C increments close to the freezing point.”

Ref. 2: p794/l.17-18: it is not the physical basis for refraction seismics in low-porosity bedrock but the basis for its application to differentiate between frozen and unfrozen state.

Changed to: “. . .demonstrate the general applicability of refraction seismics to differentiate frozen and unfrozen low-porosity bedrock.”

2 Introduction

Ref. 2: p794/l.23-25: this statement is not only valid for rock permafrost but for all permafrost occurrences

Changed to: “Permafrost is not synonymous with perennially frozen underground due to freezing point depression resulting from solutes, pressure, pore diameter and pore material (Lock, 2005; Krautblatter et al., 2010). Ice develops in pores and cavities (Hallet et al., 1991) and affects the thermal, hydraulic and mechanical properties of the underground."

Ref. 2: p795/l.17: Scott et al. 1990 would be the more appropriate reference of the two

Deleted Scott et al. 1978

Ref.1: Page 2 lines 21-26: The S waves are generally considered to be more sensitive to pore change and liquid saturation than P waves.

Changed into: "In field applications, the most prominent geophysical parameters for the differentiation between frozen and unfrozen underground are electrical resistivity and compressional wave velocity (Hauck, 2001)."

Ref. 2: p795/l.20 and l.26: does high-porosity rocks mean > 5%? Be more specific!

Changed into: "Alpine rock cliffs in permafrost regions mostly consist of hard low-porosity rocks (<10 %), according to Tiab and Donaldson's (2004) definition, and the applicability of geophysical methods to these is yet unclear."... "The p-wave velocity of freezing rocks was investigated in the laboratory mostly using polar high-porosity (>10 %) sedimentary rocks (Dzhurik and Leshchikov, 1973; King, 1977; Pandit and King, 1979; Pearson et al., 1986; Remy et al., 1994; Sondergeld and Rai, 2007; Timur, 1968). Only few studies included low-porosity (<10 %) sedimentary rocks (Pearson et al., 1986; Timur, 1968), igneous rocks (Takeuchi and Simmons, 1973; Toksöz et al., 1976) and metamorphic rocks (Bonner et al., 2009)."

Ref. 2: p795/l.20: to what extent

Changed into: "Alpine rock cliffs in permafrost regions mostly consist of hard low-porosity rocks (<10 %), according to Tiab and Donaldson's (2004) definition, and the applicability of geophysical methods to these is yet unclear."

Ref. 1: Page 3 lines 16-19: The pore size is quite different when comparing laboratory

and field data that could explain the observed discrepancies.

Changed into: "Akimov et al. (1973) note the discrepancy between seismic laboratory and field investigations. Due to different ambient settings, the comparison of small scale laboratory results to large scale field applications is complicated. These include a high rate of cooling, a non-representation of the stressed state of earth, supercooling and the time required for transition into ice in laboratory studies."

Ref. 2: p796/l.23-27: This introduction into the paragraph is a bit misleading, as it starts with relationships for permafrost (Carcione, Zimmerman, King, Leclaire), but it then focuses on Wyllie's equation, which was stated for unfrozen conditions. The order should be more logically starting with the (unfrozen) case of Wyllie. In addition, there are strictly speaking several restrictions to the applicability of Wyllie's equation (and by this also Timur's approach) regarding seismic wavelength and size of fissures/pores (should be similar), and also the measurement set-up of Timur was also different than in the present study (high pressures, acoustic measurements). Could you include a short discussion on that as it might be relevant especially regarding the quantitative results (mean values for lithologies)? And Ref. 1: Another important remark concerns the building of the time average model (equation 1). It is important to indicate that this model consider an assembly of matrix and pores in series along the wave propagation direction. This is a good approximation for an anisotropic rock with waves propagating perpendicular to the anisotropy direction. But it is no more valid pour a random porous media or when the waves are propagating parallel to the anisotropy. This must be discussed in the introduction as it one of the reasons why such model does not apply to all the rock types.

We changed the order and started with the time-average equation. In the introduction the new order follows the recommendation of Ref. 2 and requirements of the time-average equation is introduced: "The time-average equation requires a relative uniform mineralogy, fluid saturation and high effective pressure (Mavko et al., 2009). To fulfil the seismic ray assumption of the time-average equation the wavelength should be small compared with typical pore and grain size, respectively, and the pores and grains should be arranged as homogenous layers perpendicular to seismic ray path (Mavko et al., 2009). Due to larger size and more heterogeneous distribution of vugular pores in carbonate rocks, p-wave velocities of carbonate rocks show less dependency on porosity and the time-average equation underestimates the p-wave velocities Wyllie et al. (Wyllie et al., 1958). "

The calculations of p-wave velocity increase and matrix velocity by using the time-average equation for carbonate rocks and interpretation were resigned. In the methodology chapter the wavelength and potential dispersion of p-wave velocities due to wavelength is introduced: "The p-wave generator Geotron USG 40 and the receiver were placed on flattened or cut opposite sides of the cuboid samples. The wavelength of the generator was 20 kHz to fulfill requirements of the time-average equation; dispersion of p-wave velocities due to wavelengths are negligible (Winkler, 1983)."

In the discussion chapter, we mentioned the caution to handle the calculated results of the parallel measurements. All values now mentioned are results or calculations perpendicular to cleavage or bedding. "Calculations of p-wave velocities parallel to cleavage or bedding reflect this offset trend but are violating the seismic ray assumptions of the time-average equation and should be used with cautiousness."

Ref. 2: p.797/l.2: volumetric porosity fraction: either just porosity or volumetric air fraction?

Changed into: "...Φ is the porosity..."

Ref. 2: p797/l.4-5: This sentence is not clear (independent behavior of p-wave velocities of porosities?) to me. Could you explain in more detail?

Changed into: "Due to larger size and more heterogeneous distribution of vugular pores in carbonate rocks, p-wave velocities of carbonate rocks show less dependency on porosity and the time-average equation underestimates the p-wave velocities (Wyllie

et al., 1958)."

Ref. 2: p797/Eqs. (2) and (3): Equation (2) is only a special case of Eq. (3), i.e. assuming Si = 1. You could omit Eq. (2).

This study tries also to address the topic to a broader audience without geophysical background. That's why the derivation of Timur's model is described in more detail.

Ref. 2: p797/l.11: Equations (2) and (3)

Changed.

Ref. 1: Page 4 line 8: this equation should apply only for a very particular set of parameters, so it not a surprise that it does not work for all the rocks. And Ref. 2: p797/l.16-18: McGinnis et al. based his regression (which was just a simple least square fit) on data from Timur (1968) and Twomey (1968) and their Figure 5 was constructed only as interpreting tool for "porous, frozen ground" in the context of their study on permafrost in the Antarctic Dry Valleys. So there is no need to infer that they "postulate" that there is "no p-wave acceleration due to freezing in rocks with porosities less than 3.63%"! That was definitely not the aim of their study about Antarctic permafrost! The wording should be changed and Eq. (4) should be omitted, as it is only a regression of data which were used in the Timur model in a more physically-based approach.

We left Eq. 4 in the paper just for completeness and changed the wording into: "...based on a linear regression of Timur's (1968) measurements; a formula that implies that there are no p-wave velocity changes below 3.6 % porosity. This relation was only used as an interpretation tool for their field measurements and possesses no validity for low-porosity rocks."

Ref. 1: Page 4 lines 18-21: This is related to change of porosity induced by change of applied stress, e.g. crack closure observed in the laboratory. This supposed that the stress is transmitted to the matrix and should not be confused with pore pressure increase that leads to an increase of the pore size. And Ref. 2: p798/l.13-16: This sentence should be moved to directly before Eqs. (7) and (8). And Ref. 2: p798/l.4-21: some of the repetitive references to the same three papers can be combined. And Ref. 2: p798/l.20: For permafrost conditions

Changed into: “The influence of pressure on seismic velocities (Nur and Simmons, 1969) and porosity (Takeuchi and Simmons, 1973; Toksöz et al., 1976) and is observed by many researchers (King, 1966; Wang, 2001). Two pressures can be distinguished, the confining or overburden pressure of the rock mass and the pore pressure of the fluid. These can reinforce or compete with each other, which is expressed by different values of n (Wang, 2001).” …”Pores react to an increasing confining pressure according to their shape: spheroidal pores deform and become thinner while spherical pores decrease in volume (Takeuchi and Simmons, 1973; Toksöz et al., 1976). P-wave velocity will increase due to decreasing porosity if the confining pressure does not surpass the damage threshold and porosity increases due to microcracking (Eslami et al., 2010; Heap et al., 2010; Wassermann et al., 2009). In measurements with high confining pressures, the effect of pores is negligible but the effects of cracks become more important (Takeuchi and Simmons, 1973). In frozen rocks, the ice pressure effect is most pronounced for spheroidal “flat” pores or cracks (Toksöz et al., 1976).”

Ref. 1: Page 5 line 16: more recent references show the increase of anisotropy during the loading of rocks eg Wasserman et al and Eslami et al, Heap et al.

Changed into: “Stress increase due to loading can preferentially close pre-existing microcracks perpendicular to stress direction and decreases anisotropy (Eslami et al., 2010; Heap et al., 2010; Wassermann et al., 2009). However, stress increase can also lead to preferential opening of axially orientated microcracks (Eslami et al., 2010) or microcrack generation due to threshold surpassing (Heap et al., 2010; Wassermann et al., 2009) which then enhances anisotropy.”

Ref. 2: p799/l.4 and Eq. (9): max and min refer to the maximal/minimal velocity obtained while measuring parallel and perpendicular?

Changed into: "…where vmax is the faster velocity of both compressional waves parallel and perpendicular to cleavage or bedding and vmin is the slower velocity (Johnston and Christensen, 1995).

Ref. 2: p799/l.9: What are the discrepancies mentioned by Akimov et al. (1973)? As you are aiming at overcoming these discrepancies, you should clearly state what you mean by that! In addition, you should then also address in your Discussion/Conclusion whether you have in fact reached this aim.

We deleted that part. The discrepancies we cannot overcome.

2 Methodology

Ref. 1: Please add a scheme showing the imposed temperature conditions and the geometry of the thermal gradient.

We use 2-3 thermistors in the rock sample in different depths and different positions to guarantee that the temperature field is homogeneous inside the sample. The speed of cooling has been adjusted to keep temperature differences mostly below -0.3°C in the sample. As we only use totally frozen samples (-15°C) and thawed samples as reference values and given the fact that p-wave velocities of once frozen or thawed samples do not change significantly, the geometry of the thermal gradient is presumably not important in this case.

Ref. 2: p799/l.15-16: You have to be more exact and specific about the geographic source of your samples: as far as I can see from Table 1 only one site (Longyeardalen) with two samples is from the Arctic so the statement "several Alpine and Arctic permafrost site" is misleading. In addition you have to explain why you have apparently taken sometimes several samples from one site (Steintälli, Corvatsch, Präg) and sometimes not. As you are afterwards using mean values for different lithologies, the number of samples from one site can be important.

We added a new Table 1 where we include the geographic source of our samples

Changed into: "We tested 20 Alpine and 2 Arctic rock specimens between 1.8 and 25 kg sampled from several permafrost sites (see Table 1 for details)." See answers to general comments 1 and 4 of Ref. 2.

Ref. 2: p799/l.19-20: repetition of "atmospheric" And Ref.1: Page 6 line 6-8: this method of saturation is similar to natural condition but probably includes air bubbles within the pore water; this is more complicated to interpret.

Due to the size of the samples, vacuum saturation will take days to weeks. Changed into: "All samples were immersed in water under atmospheric conditions until full saturation indicated by a constant weight is achieved (Ws). The free saturation method resembles the field situation more closely than saturation under vacuum conditions (Krus, 1995; Sass, 2005) but probably includes air bubbles and can complicate the interpretation."

Ref. 2: p800/l.3: How was density derived? The reference Wohlenberg (2012) is missing in the reference list.

Density was derived from literature: Wohlenberg, J. (2012). 1.2 Densities of rocks. Springer Materials - The Landolt-Börnstein Database (http://www.springermaterials.com). See general comment 5 of Ref. 2.

Ref. 2: p800/Eq. (11): the units in this equation are not matching! Is it correct?

The method of Sass (2005) is only an approximation of effective porosity and described more detailed in the PhD thesis of Sass (1998), the water absorption capacity [%] is multiplied with density [g/cm^3].

Ref. 1: Page 6 line 16: In addition to the concept of hydraulically linked pores, the ratio between linked and non-linked pores is probably useful for understanding the variety of rocks behavior.

Changed into: "…is calculated by multiplying the water absorption capacity with the rock density and includes only hydraulically-linked pores (Sass, 2005). To distinguish

quantitatively connected and unconnected pores will help the interpretation but necessary methods were not available."

Ref. 2: p800/l.9: as illustrated/shown by Krautblatter (2009) And Ref. 2: p800/l.10-12: All of these six specifically prepared samples were immersed. (otherwise it is read as repetition to p799/l.18-20). You have to specify more clearly which measurements were done already by Krautblatter (2009) and which were done in this present study. Besides, it is not clear which part of the methodology was applied to ALL samples and which part only to 6 samples as indicated in line 7.

Changed into: "In an earlier study by Krautblatter (2009), six plan-parallel cylindrical plugs were prepared with diameter and length of 30 mm from six of the 22 samples used in this study and porosity values were measured using a gas compression/expansion method in a Micromeritics Multivolume Pycnomter 1305. These absolute porosity values are used to estimate the quality of the effective porosity values. All 22 samples were immersed again for 48 h under atmospheric conditions and the saturated weight W48h was determined."

Ref. 2: p800/Eq. 12: I do not understand the difference between W48h in Eq. (12) and Ws in Eqs. (10) and (12).

Changed into: "All samples were immersed in water under atmospheric conditions until full saturation indicated by a constant weight is achieved (Ws)." … "All 22 samples were immersed again for 48 h under atmospheric conditions and the saturated weight W48h was determined."

Ref. 1: Page 6 lines 26-29: why does the authors change the cooling rate before and after 0°C. Is there any flank of the samples insulated or are they all directly in contact with the climatic chamber air.

We added a new Figure 1 with the measurement set up. The temperature of the climate chamber was controlled manually. We tried to cool down the samples slowly to observe supercooling processes. After the velocity jump due to freezing, the chamber temperature was decreased. Text changed into: "Subsequently, samples were loosely coated with plastic film to protect them against drying and were cooled in a range of 25°C to -15°C in a WEISS WK 180/40 high-accuracy climate chamber (Fig. 1). The cooling rate was first 7°C/h until sudden p-wave velocity increase due to freezing and was then decreased to 6°C/h (Matsuoka, 1990)."

Ref. 2: p800/l.20: which depths are meant? Depths in the samples? How were the sensors installed there? Were there any differences in temperatures within the sample? You have to give more details how you were addressing potential temperature differences within the sample (or were they negligible ?).

Changed into: "Two to three calibrated 0.03°C-accuracy thermometers were drilled into the rock samples up to depth between 3-10 cm and an spacing of approximately >10 cm depending on sample size and measured rock temperature at different depths and spacings to account for temperature homogeneity in the sample (Krautblatter et al., 2010)."

Ref. 2: p800/l.21: Give details or reference to the p-wave generator

Changed to: "The p-wave generator Geotron USG 40 and the receiver were placed on flattened or cut opposite sides of the cuboid samples. The wavelength of the generator was 20 kHz to fulfill requirements of the time-average equation; dispersion of p-wave velocities due to wavelengths are negligible (Winkler, 1983)."

Ref. 2: p801/l.2: Which porosity was used for Eq. (2)?

Changed into: "The velocity of the material in the pore space vi is 1570 m/s for water in the unfrozen status and 3310 m/s for ice (Timur, 1968), we replaced porosity with effective porosity in the calculation."

Ref. 2: p801/Eqs 13. and 14: the brackets in the equations are unnecessary Ref. 1: Page 7 lines 11-13: there is a possible confusion between 'V' and 'nu' for the velocity,

please clarify.

Deleted the brackets and changed Vm to vm.

Ref. 2: p801/l.10: The change of anisotropy

Done.

3 Results

Ref. 1: Page 8 lines 1-2: the difference between absolute porosity and effective porosity corresponds to the part of non-connected porosity and should be expressed explicitly.

Changed into: "The absolute (vacuum) porosity values comprehending connected and non-connected porosity measured for 6 samples (A5, X2, S1, S3, X9, A8) by Krautblatter (2009) are compared with the effective (atmospheric pressure) porosity values comprehending only connected porosity."

Ref. 2: p802/l.4-5: Could this be due to the density used for the calculation in Eq. (11)?

The assumed density to calculate the effective porosity is at most between 2.4 and 3.12 depending on lithology (Wohlenberg, 2012). Maximum and minimum values are used to calculate the effective porosity. The influence of the density values on effective porosity values is expressed as mean deviation of the effective porosity values. See also answer to general comment 5 of Ref. 2.

Ref. 2: p802/l.7: All clusters differ less than 1%... this is misleading, as e.g. for schists1.48 +/- 0.5 the relative accuracy is quite low (around 33%), and an absolute accuracy of 1% is rather high for porosities around 1-2%. Please rephrase to avoid misunderstanding.

Changed into: "Absolute deviations of porosity within the clusters are less than 1% except for carbonate rock samples."

Ref. 2: p802/l.14: (see also comment p797/l.16-18, comment to caption Fig. 3 and general comment 6): This makes no sense: Figure 5 in McGinnis et al is only a least-square fit (a straight line through some points) of published data, which incidentally results in Delta_Vp = 0 for a porosity = 0.0363. This fit was surely not intended to provide valid data for very low porosities, otherwise they could have easily constrained it to Delta_Vp = 0 for a porosity of 0. It is quite unfair to cite this equation in this context! As stated above, I propose to omit Eq. (4) and focus on the relevant Eq. (3) with its special case (2). And Ref. 2: p803/l.1-2: This generalization out of only two samples with very different porosities does not really make sense to me. In Fig. 3 you omitted the carbonate rocks accordingly – this should also be done in the text.

We put the focus on the time-average equation and compared our measured results with calculated results of expected p-wave velocity increase using Eq. (2). The results are visualized in Fig. 4A and B. Changed into: "Existing time-average models assume a dependence of p-wave velocity increase from porosity. We plotted the increase of p-wave velocity due to freezing measured and calculated with Eq. (2) against the mean effective porosity (Fig. 4A and B). We excluded the carbonate rocks due to their vugular pores and the constrained applicability of the time-average equation (Wyllie et al., 1958). All measured p-wave velocity increases are much higher than the calculated p-wave velocity increases according Eq. (2) expected as a result of phase transition from water (1570 m/s) to ice (3310 m/s). Parallel to cleavage or bedding, the offset between measured and calculated results is increasing from gneiss (296 $\pm$205 m/s), schists (642 $\pm$314 m/s), other metamorphic rocks (685 $\pm$200 m/s), plutonic rocks (686 $\pm$0 m/s), clastic rocks (815 $\pm$683 m/s), to volcanic rocks (1158 $\pm$278 m/s). Perpendicular to cleavage or bedding, the offset increases from other metamorphic rocks (414 $\pm$210 m/s), gneiss (467 $\pm$108 m/s), volcanic rocks (529 $\pm$183 m/s), plutonic rocks (561 $\pm$41 m/s), clastic rocks (626 $\pm$474 m/s) to schists (1368 $\pm$695 m/s)."

Ref. 1: Page 8 lines 27: Is the hysteresis related to supercooling or to the possible increase of porosity induced by pore pressure (i.e damage).

We moved the part to chapter 3.1: "Supercooling causes hysteresis effects resulting in sudden latent heat release and rock temperature increase observed in 16 of 22 samples and indicated as p-wave velocity hysteresis of three rock samples (A5, X8, L2) in Fig. 2."

Ref. 2: p803/l.6-7: The hysteresis effect is not shown except that super-cooling could be inferred from Fig. 1. How did you deal with it in your quantitative statistics (which branch of the hysteresis did you use)?

We excluded the supercooling in the quantitative statistics and used the matrix velocity at -15°C to neglect influence of unfrozen water content. "Matrix velocity is calculated for frozen (-15° C) and unfrozen status (mean value of v >0°C) both for parallel and perpendicular to cleavage/bedding measurements according to…"

Ref. 2: p803/l.13: sample X5: try to homogenize the denomination of your samples: either always symbols/abbreviations or a combination from lithology and source etc. At the moment it is used very differently in the text and figures.

Thanks for the comment. We homogenized the denomination and now use lithology class or in the case of single samples lithology and then the symbol in brackets.

Ref. 2: p803/l.25-26: Is frost weathering important in this context (Results section)? Could be omitted or moved to the Discussion/Introduction if necessary

Deleted!

4 Discussion

Ref. 2: p804/l.7: representativeness

Done!

Ref. 2: p804/l.8-9: these discrepancies have still not yet been named, and Akimov et al. (cited before in this context) is not Alpine permafrost

Deleted the discrepancies!

Ref. 2: p804/l.17-18: What is exactly meant by the reference to Wyllie in the context of carbonates ? Be more specific. In addition, only two very different carbonate samples were analyzed.

Changed into: “Due to vugular pores, the time-average equation and Eq. (2) are not applicable to carbonate rocks (Wyllie et al., 1958) and we excluded them from further calculations.”

Ref. 2: p804/l.17-23: I consider the generalization of the results to lithological classes doubtful, when only 1-2 samples per class are available (e.g. plutonic (2), volcanic (2 from the same site), clastic (2 from the same site), carbonate (2, and very different in porosity). See also general comment 4.

See answer on general comment 4 (Ref. 2).

Ref. 1: Page 10 lines 11-12: Ok the model of McGinnis is not relevant here but this does not exclude that the porosity could be a relevant parameter. And Ref. 2: p804/l.21-25: see comments above: The “McGinnis-bashing” is not appropriate here!

We reworked the whole break and put the emphasis away from McGinnis and on the Timur model: “Timur’s (1968) model would, respectively, anticipate p-wave velocity changes from 104 $\pm$17 m/s (other metamorphic rocks), 75 $\pm$2 m/s (gneiss), 96 $\pm$21 m/s (plutonic rocks), 238 $\pm$19 m/s (volcanic rocks), 301 $\pm$60 m/s (clastic rocks) to 58 $\pm$34 m/s (schists) and underestimates strongly p-wave velocity increases in low-porosity bedrock (Fig. 4B). Due to vugular pores, the time-average equation and Eq. (2) are not applicable to carbonate rocks (Wyllie et al., 1958) and we excluded them from further calculations. The offset between measured velocities and calculated velocities shows that porosity is not the dominant determinant of p-wave velocity changes in low-porosity bedrock.”

Ref. 2: p805/l.9-10: Unclear: Which porosity was then used in Table 1 and Figs. 2

and 3? And Ref. 1: Pages 10 lines 21-24: This sentence is not clear to me, please rephrase. And Ref. 2: p805/l.11-13: see comments above: what exactly is meant and why do you include the carbonate samples at all in your discussion?

All porosity values in the tables and figures are effective porosity values. We excluded carbonate samples in our discussion and changed the sentences to make it clearer: “Hydraulically linked porosity is best described by effective porosity (Sass, 2005) and we replace porosity in Eq. (2) with effective porosity. In future studies, the pore form could be assessed by porosimetric analyses and, thus, the differentiation of connected and non-connected porosity would facilitate a quantitative interpretation. However, calculating matrix velocity with absolute porosity values would change matrix velocity only by 2 $\pm$2 %, which is well below the accuracy within the clusters.”

Ref. 2: p805/l.15-20: Isn’t this just a question of availability of samples? In addition, are you sure the samples represent rock conditions at greater depth (where frozen conditions are present) if the samples were taken from the surface (if this was the case)? I would assume that rock samples from the surface are much more weathered than at larger depth. And Ref. 2: p805/l.16. only one Arctic site was sampled

Changed into: “We choose decimeter-large rock samples from several Alpine and one Arctic permafrost sites instead of standard bore cores. These are derived from the surface or quarried out of rock walls but are affected by permafrost in their history and include hundreds of micro-fissures and statistically represent the natural texture of permafrost-affected bedrock.”

Ref. 1: Page 11 line 5: The term ‘alteration’ is not appropriate here, ‘variation’, ‘change’, ‘modification’ are probably preferable.

Changed to “variation”.

Ref. 2: p805-p806: points (ii) and (iii) are containing a lot of references which are not really used to confirm the hypotheses/results of the present study. I have the impression that these two sections could be shortened, especially regarding the cited references (are they all necessary ?)

From our point of view there are some restrictions of our study and a lack of process understanding which we wanted to address point (ii) and (iii) of the discussion.

Ref. 1: Page 11 lines 18-19: please explain where this threshold value is coming from.

The threshold value is the degree of saturation which is necessary to crack rocks as a result of volumetric expansion. Changed into: “Due to 48h saturation, the degree of saturation reaches at least 0.91 in all samples and the threshold for frost cracking as a result of volumetric expansion is fulfilled (Walder and Hallet, 1986).”

Ref. 1: Page 11 lines 21-30: what about thermal dilation and its effect on porosity? Is the cubic geometry of the samples providing similar thermal gradient that natural conditions?

Modified: “The variation of confining pressure related to rock overburden is a long-lasting process on a millennium scale, whereas pore pressure changes steadily (Matsuoka and Murton, 2008). Frequent daily freeze-thaw-cycles reach a depth of approximately 30 cm (Matsuoka and Murton, 2008) while annual cycles often reach up to 5 m and more (Matsuoka et al., 1998). In our experiment the change in matrix velocity in combination with reduced anisotropy points towards “induced anisotropy” (Wang, 2001) in pores that reflects intrinsic stress generation. The pore pressure in the connected pores presumably increases due to ice-stress applied on the matrix and probably closes non-connected porosity embedded in the matrix which results in decreasing anisotropy. A surpassing damage threshold or opening of microcracks could explain anisotropy increase. The pore pressure can be generated by the ice pressure building (Matsuoka, 1990; Vlahou and Worster, 2010) due to volumetric expansion of in situ water (Hall et al., 2002; Matsuoka and Murton, 2008) and ice segregation (Hallet, 2006; Murton et al., 2006; Walder and Hallet, 1985). In the laboratory, any open system allows water migration and enables ice segregation while closed systems with water-

saturated samples favor volumetric expansion (Matsuoka, 1990). Our experimental setup is a quasi-closed system; water is only in situ available due to saturation and ice can leave through pores and joints. Due to 48h saturation, the degree of saturation reaches at least 0.91 in all samples and the threshold for frost cracking as a result of volumetric expansion is fulfilled (Walder and Hallet, 1986). According to Sass (2005) and Matsuoka (1990) our quasi-closed system and fully saturated samples could be a good analogue to natural conditions.”

Ref. 2: p806/l.23: samples were observed to contract (?)

Changed into: “This is due to the fact that ice pressure is relaxed through ice deformation and ice expansion into free spaces (Tharp, 1987), ice extrusion (Davidson and Nye, 1985) and the contraction of samples was observed in the long-term due to ice creep (Matsuoka, 1990).”

Ref. 2: p806/l.27: will cause suction to several MPa () and ice growth, and presumably

Changed!

Ref. 1: Page 12 lines 2-13: I suggest to plot DeltaV as a function of the nonconnected porosity ratio and to plot data vs model.

This is a good idea and would improve the model but unfortunately non-connected porosity values are not available. We incorporated in the text: “The way ice-pressure is effective depends on the pore form of connected and non-connected pores. A quantitative analysis needs to distinguish between connected and non-connected pores.”

Ref. 2: p807/l.2-4: logical order of this introduction to Eq. (16) is not clear to me (lithology is a proxy. . .). Repetition with line 9. And Ref. 2: p807/Eq. (16): This is rather shortly discussed. It would be good to have a Figure showing the improvement of your model with respect to your (and maybe also previously published) data (see general comment 1). As far as it is discussed now, you would not need Eqs (3), (5) and (6) and you could constrain your aims to finding an empirical relationship to explain your data and then relating the fitting parameter m to a rock physical process. It would be beneficial, if you could apply the model also for higher porosities and see whether the effect is really restricted to small porosities and whether the results are consistent for dry rocks.

We overworked the whole part and paid more caution to general conclusions: "Fig. 4A and B shows the offset which is not explainable by Eq. (2). The ice-pressure induced increase (Fig. 4C and D) almost equals the offset. The way ice-pressure is effective depends on the pore form of connected and non-connected pores. A quantitative analysis need to distinguish between connected and non-connected pores. We use lithology is a proxy for pore form in our model and we assume an elevated level of stress in cryostatic systems. The pressure-induced variable m depends on lithology and is introduced as an extension of Eq. (2):"..."Δvm is the increase of matrix velocity empirically derived from our measurements. Drawing general conclusions and transfer our model to other as mentioned rock types should be applied with extreme cautiousness due to the more qualitative property of our results. For our rock samples, we propose values of m of 1.09 $\pm$0.02 for gneiss, 1.09 $\pm$0.05 for other metamorphic rocks, 1.62 $\pm$0.45 for schists, 1.15 $\pm$0.00 for plutonic rocks, 1.12 $\pm$0.05 for volcanic rocks and 1.17 $\pm$0.13 for clastic rocks or, alternatively a general m of 1.34 $\pm$0.31 (Table 2). The use of Eq. (16) enhances to differentiate between frozen and unfrozen status of low-porosity rocks and can facilitate interpretation of field data."

5 Conclusion

Ref. 2: p807/l.17: geophysical modeling of p-wave velocities is something different: that would imply the physical modeling of the propagation of the waves. Better: empirical mixing rules or petrophysical relationships

Changed into: "empirical mixing rules"

Ref. 2: p807/l.22: see above: this generalization with detailed numbers seems doubtful for only 1-2 samples.

Changed into: "All tested rock samples show a p-wave velocity increase dependent on lithology due to freezing. P-wave velocity increases from 418 $\pm$194 m/s for gneiss to 2290 $\pm$370 m/s for carbonate rocks parallel to cleavage/bedding; perpendicular measurements show an acceleration ranging from 414 $\pm$210 m/s for other metamorphic rocks to 2745 $\pm$1444 m/s for carbonate rocks."

Ref. 2: p808/l.2-4: see above: not explained in the text: is the schistosity the reason? That should be mentioned and discussed much earlier in the paper.

Deleted!

Ref. 2: p808/l.15-16. repetition to p807/l.20. In addition: matrix velocity (without capital M)

Changed into: "We developed a novel time-average equation based on Timur's (1968) 2-phase equation with a lithology dependent variable to increase the matrix velocity responding to developing ice pressure while freezing."

Ref. 2: p808/l.20: lesser extent

Changed!

Figures and Tables:

Ref. 2: Table 1: see general comments 1 and 4: this table must be improved regarding the geographic location of the source of the samples as well as further information about how they were collected and whether they relate to published data on geology, geomorphology, p-wave velocities, permafrost conditions etc. In addition, a homogenized naming would be beneficial. Maybe, an additional Table is needed for that.

We added an additional table which includes (i) location, (ii) conditions at the study site (rock wall, talus slope...), (iii) sample origin (surface, rock wall), (iv) lithology, (v) short geological description, (vi) porosity and degree of saturation, (vii) published data on permafrost conditions at the sample location.

Ref. 2: Figure 1: caption: what do you mean by mean deviation? Different P-wave measurements or different temperature measurements (outside/inside) or both? I would also think it better to include the lithology of the samples in the legend. Please homogenize the names of the sample: sometimes a region is given (Matter Valley, Svalbard), sometimes not. Zugspitze/Matterhorn/Aiguille du Midi denote peaks, for Murtel is unclear what is meant (Piz Murtel?)

The mean deviation of the p-wave velocities was meant. We changed the legend and included the lithology.

Ref. 2: Figure 2: legend: if you use the general expression "carbonate rocks" you imply that the result is valid for carbonate rocks in general. Can you really say that? In my opinion, "carbonate rock samples" would be better, at least in the caption.

We overworked the caption and incorporated the suggestions.

Ref. 2: Figure 3: This figure is not discussed in detail in the text! To me, a slight porosity effect seems at least to be present even though the number of samples may not be sufficient to say so?! The reference to McGinnis et al. in the caption is misleading and out of context here (see comments above), and should be rephrased.

And Ref. 2: Figure 4: For porosities close to zero the effect of the pore liquid in Equation (2) or (16) is negligible per definitionem. Because of this, it is not surprising that the calculated change in matrix velocity and p-wave velocities are similar, this follows from Eqs (13) and (14).

We replaced this figure with a new figure plotting p-wave velocity increase in ms-1 against mean effective porosity and matrix velocity in ms-1 against mean effective porosity. We compared measured data with calculated data using Eq. 2 and observed an offset between these two data sets.

References:

Ref. 2: The following references are missing in the list: Wohlenberg (2012) Hauck and

Kneisel (2008)

We added the references to the list.

Additional references not mentioned in the paper:

Sass, O.: Die Steuerung von Steinschlagmenge und –verteilung durch Mikroklima, Gesteinsfeuchte und Gesteinseigenschaften im westlichen Karwendelgebirge (Bayerische Alpen). Münchner Geographische Abhandlungen Band B29, 1998.

Interactive comment on The Cryosphere Discuss., 6, 793, 2012.