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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

Anonymous Referee #2

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Review of the manuscript "P-wave velocity changes in freezing hard low-porosity rocks: a laboratory-based time-average model" by D. Draebing and M. Krautblatter submitted to The Cryosphere

This manuscript presents laboratory experiments exploring the effect of freezing on P-wave velocity in low porosity rocks. The authors investigate the behaviour of various rock samples as a function of temperature, analyse the effect of freezing on anisotropy and compare these experimental observations with previous laboratory work. The topic is of particular interest to the mountain permafrost community, as P-wave velocity obtained from seismic field measurements is often used as indicator for the occurrence of permafrost, or, in a monitoring context, because changes in P-wave velocity are attributed to freezing/thawing processes.

The extensive laboratory measurements presented are very valuable for the cryospheric community and should be published, I have nevertheless a number of remarks concerning several details written in the paper, especially regarding some of the generalisations drawn by the authors, which I will list in the following (together with some minor details regarding wording, typos etc).

General comments:

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. 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 generalise the results for all volcanic rocks based on two rock samples of Präg ? 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.

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 analysing rock specimen with higher porosities, but you could also calculate it using Eqs. (2) and (16), respectively. Similarly, you could analyse 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 !

3. Why did you only measure saturated rocks ? For a comparison with Timur's equa-

tion, 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.

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 ?

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

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.

Comments in detail :

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Interactive Discussion

Discussion Paper

Abstract :

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 ? See also general comment 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.

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.

Introduction :

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

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

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

p795/l.20 : to what extent

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 Wyllies equation, which was stated for unfrozen conditions. The order should be more locically 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

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(mean values for lithologies) ?

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

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

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).

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

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.

p798/l.13-16 : This sentence should be moved to directly before Eqs. (7) and (8).

p798/l.20 : For permafrost conditions

p798/l.4-21 : some of the repetitive references to the same three papers can be combined.

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

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.

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Methodology

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.

p799/l.19-20 : repetition of "atmospheric"

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

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

p800/l.9 : as illustrated/shown by Krautblatter (2009)

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.

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

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

p800/l.21 : Give details or reference to the p-wave generator p801/l.2 : Which porosity was used for Eq. (2) ?

p801/Eqs 13. and 14 : the brackets in the equations are unnecessary

p801/l.10: The change of anisotropy

Results

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

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.

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).

p803/l.1-2: This generalisation 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.

p803/l.6-7: The hysteresis effct 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) ?

p803/l.13: sample X5: try to homogenise 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.

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

Discussion

p804/l.7: representativeness

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

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 analysed.

p804/l.17-23: I consider the generalisation 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.

p804/l.21-25: see comments above: The "McGinnis-bashing" is not appropriate here!

p805/l.9-10: Unclear: Which porosity was then used in Table 1 and Figs. 2 and 3 ?

p805/l.11-13: see comments above: what exactly is meant and why do you include the carbonate samples at all in your discussion ?

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.

p805/l.16. only one Arctic site was sampled

p805-p806: points (ii) and (iii) are containing a lot of references which are not really

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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 ?)

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

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

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.

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.

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

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

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.

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

p808/l.20: lesser extent

Figures and Tables:

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 homogenised naming would be beneficial. Maybe, an additional Table is needed for that.

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 homogenise 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 ?)

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.

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.

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).

References:

The following references are missing in the list:

Wohlenberg (2012) Hauck and Kneisel (2008)

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