Round 1

Reviewer 1:

Comments for author and editor

Fliedner and French present experimental and theoretical analyses that help illuminate the link between seismic wave attenuation and fluid flow in tectonic settings with high pore fluid pressure that produce large earthquakes, i.e., subduction zones. The results show that the measured Young's modulus, shear modulus and attenuation changes with the frequency of the waves when water is present. They are able to fit portions of the experimental data to predictions of viscoelastic models, which indicates that viscoelasticity can produce attenuation. Although it is well known that the measured elastic moduli can vary as passing seismic waves triggers fluid flow, the authors try to justify the importance of this work by pointing out that crustal measurements near the base of the subduction zone are limited. The work claims to be the first to measure the dynamic elastic moduli and viscoelastic behavior of phyllosilicate-rich metapelite under saturated conditions at seismic frequencies. The manuscript is well-written, and the conclusions are justified by the data; however, I'm not sure if the importance of the work rises to the level of this journal. The Letter to Editor convinced me a bit more about the broader relevance of this work to low-frequency earthquakes, but I don't believe this point was mentioned or explored in the manuscript. I leave it to the Editors to decide if the manuscript rises to the appropriate level. More detailed comments follow.

1) Line 30 "by fluids through in the pore spaces of rock". Maybe there is a typo/extra word here?

2) Line 318: "To evaluate the low and high frequency peaks separately, we segment the data into two frequency ranges from 5×10^{-5} to 2×10^{-2} Hz and 3 to 20 Hz corresponding to ranges over which we see peaks in attenuation above some background." Could you explain why you need to or it is useful to separate the data in this way? Maybe it is because you try to explain these two different mechanisms (patchy saturation and squirt flow), and they have different parameters in the model? Is it not possible to create one model or set of parameters to explain both mechanisms?

3) Figure 6: On some of the plots it is clear that the dashed lines corresponding to the PS and SF differ (a), but in other plots they appear continuous (c). If I understand correctly that different sets of models produce the different lines, then it would help if you could make the SF and PS lines different colors.

4) Line 392: "regions of that have been interpreted as having low effective stresses". Regions of what?

5) Line 424: “the predominance of thin elongated pores is the primary microstructural characteristic controlling the magnitude of dispersion”. The manuscript describes very clearly why thinner pores would have a greater influence on attenuation than more equant pores, and that schist tends to be dominated by thinner pores. I wonder if the authors consider that the presence of fractures could also contribute some significant influence on the attenuation, as well as thin pores? Fractures can be highly anisotropic, and may exist in relatively high densities surrounding faults where large earthquakes occur. But maybe the term “thin pore” is just another name for a fracture?

Reviewer 2:

Comments for author and editor

General comments

This ms describes laboratory measurements of dispersion and attenuation in a greenschist facies metapelite sample, as an analogue for rocks in subduction zones. The experimental data for elastic moduli and attenuation are then compared to predictions from a viscoelastic model. The findings are compared to data from other rock types and to seismology/tomography from subduction zones.

Overall, the ms is very well written, well formatted and with clear figures. The sequence is logical and systematic. The inclusion of abundant supplementary data is welcomed. The findings make an important contribution to our interpretation of data from subduction zones, or other tectonic settings with elevated pore fluid pressures in anisotropic rocks.

Some discussion/clarification of the measurements in terms of static/dynamic elastic moduli might be useful in the introduction. More importantly, I think some further discussion/explanation is needed around the data that are not well fit by the models, say between 10-2 and 10-1 Hz on Figure 6a-b, where there are measured changes in moduli not ascribed to any mechanism or model prediction.

I recommend publication after minor revisions.

Specific comments

L30 – spurious ‘through’

L52 – pore pressure may be high in rocks with low porosity…

L58-60 – presumably, this transition involves dehydration reactions, evolution of free water, and possible increase in pore fluid pressure; worth clarifying, IMO

L64 – give a frequency range for ‘seismic’

L83 – first mention of ‘dynamic elastic moduli’…

L91 – what is the max P and T experienced by this unit?

L106-108 – such a low permeability measured by steady-state method unlikely to be accurate; also, has the N2 measurement been Klinkenberg-corrected, to give a better estimate for water permeability?

L114-115 – how confident are you that the core is fully 100% saturated? What checks were made? Given the low permeability, seems unlikely…

Fig 2a – what is the red line?

Fig 2b – label the LVDTs mentioned in the caption

L138 – ‘Since the stress and induced strains are small, deformation is assumed to be anelastic.’ – can you explain? Why not just elastic?

L142 – I thought shear modulus was defined as shear stress/shear strain; how is your expression equivalent to that?

Table 1 – to clarify, the Test numbers for Orocopia all refer to the same physical sample, just different runs?

L191 – equation 3.2?

Fig 3 – caption; perhaps some colour distortion; in the PDF I reviewed, the data were blue and pink!

L206-207 – being a pit picky, but you can only say that the Young’s modulus is constant across frequencies when dry; you have no range of frequencies for the dry shear modulus data, so we don’t know if it’s dispersive in this modulus when dry (Fig 4b)…

Fig 4 – very nice plot

Fig 5 caption – no M1 labelled…

L303 – maybe reference (Mavko et al., 2020; his Table 3.8.1):

L317 – equation 5.2?

L323 and onwards – you use R2 as a measure of goodness of fit, so presumably based on a least squares method? Have you considered Maximum Likelihood Estimates (MLE) instead?

L383-383 – doesn't make sense from the values listed…

L385 and onwards – could the equations be re-derived for a distribution in aspect ratios? Also, have you considered quantifying the pore throat size distribution? e.g., with mercury injection porosimetry. Might be useful. See Farrell et al., 2021 JGR Figure 10.

L392 – spurious 'of'

L426 – is it simply the abundance of phyllosilicates? Or is it more the presence of a strong shape preferred orientation of grains (of whatever mineral group) that controls the formation of foliation-parallel microcracks?

I hope these comments help.

Reviewer 3:

Comments for author and editor

For author and editor

The authors of "Dispersive elastic moduli and frequency-dependent attenuation due to wave-induced fluid flow in metapelite" present an interesting experimental study of dispersion and attenuation in phyllosilicate using the forced oscillation method. The authors model the peaks in the attenuation data by a standard linear solid for the considered attenuation and dispersion mechanisms: patchy saturation and squirt flow. They show that the model provides a good fit to the observed data, given the model assumptions of a single pore shape and permeability. Lastly, the authors compare their results to other lithologies and consider the applicability of the mentioned mechanisms in subduction zones. Their findings are credible and shed light on important physical mechanisms in subduction zones. I therefore recommend publication after addressing the comments below.

Comments

Non-technical summary: consider giving a brief description of the ‘forced oscillation’ method.

L30: the word “through” is not necessary.

L101: should be figure 1b instead of 2b.

L114-115: Does the saturation method help remove any air that may be trapped within the sample? How do the authors decide that the sample is saturated (that 7 days are enough)?

L191, L197 and L199: typo – should be equation 5 instead of 3.2.

L195-202: How the correction parameters (A, B, C and D) were picked is not clear. In the first group of experiments are the parameters just averages for the fits to aluminum and PMMA? Why would the correction parameters change between experiment groups?

Figure 4: the captioning does not match the letters (a, b, c and d) on the figure panels – they are mixed up. Furthermore, the depicted frequency ranges for squirt flow and patchy saturation in figure 4 is first mentioned in the discussion and there is no reference to these in the result section where the figure is analyzed. For further clarification, please consider adding some information on them in the results section.

L210: “…at ultrasonic frequencies (10-6 Hz),…” typo, minus sign needs to be removed.

L214: Seems that the Young’s modulus increases linearly for frequencies > 1 Hz and not in between steps. Perhaps include an inset of these steps so that will be more obvious for the readers.

L223: “The shear modulus… at 9 MPa.” this sentence is a bit confusing, consider rephrasing.

L254: Please explain why you consider the saturation length scale to be 53 mm (is that the sample length?).

L255-256: It is to be expected that the gas permeability would be higher than the water permeability (e.g., Tanikawa and Shimamoto, 2006), also the viscosity of the fluid at isobaric conditions is sensitive to temperature variations. Do you have an estimate of the temperature during force cycling?

L272-274: This sentence is very confusing. The aspect ratio of the pores was estimated at dry conditions and the water viscosity is also mentioned too?

L299: should be the “characteristic relaxation time” in order to conform with the notation.

L317-18: typo - “equation 5.2”.

L383: the fits look good and are convincing for both mechanisms. Perhaps adding lower and upper bounds to the aspect ratio can be more informative than the single aspect ratio model (which obviously does not describe the rock very well).

L387: typo – “equation 5.1”.

L406-414: How would the mobility of water in subduction zone change the frequency ranges of the studied attenuation mechanism (would patchy saturation be relevant to subduction zones?). Fluid viscosity should be considerably lower (and in fact be in a supercritical state), and while permeability is expected to decrease with depth, it should still be pretty high for low effective stress. Would ‘warm’ subduction zone have a different dominant attenuation mechanism than ‘cold’ ones?

Nir Badt

References

Tanikawa, W. A. T. A. R. U., & Shimamoto, T. (2006). Klinkenberg effect for gas permeability and its comparison to water permeability for porous sedimentary rocks. Hydrology and Earth System Sciences Discussions, 3(4), 1315-1338.

Round 2

Reviewer 1:

Comments for author and editor

The authors have responded to all of my concerns, including the two main points helpfully highlighted by the editor. I recommend that this work may be published in the present form.

Reviewer 2:

Comments for author and editor

I am happy with the changes made, in response to all 3 reviewers.