Solid Earth Discussions

Solid Earth Discuss., referee comment RC2
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Comment on se-2021-27

Sascha Zertani (Referee)

Referee comment on "Elastic anisotropies of deformed upper crustal rocks in the Alps" by Ruth Keppler et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-27-RC2, 2021

General comments:

The manuscript “Elastic anisotropies of deformed upper crustal rocks in the Alps” by Keppler et al., presents a large dataset of TOF neutron diffraction measurements on ortho- and paragneisses from the Adula nappe (Alps). The CPO data is used to calculate petrophysical properties of the rocks, which are compared to ultrasound measurements on two of the samples and modelling of an average composition expected to be representative of the upper crust.

As such the manuscript presents a large dataset on the petrophysical properties of gneisses for which a lack of data exists. The paper is in general well written and figures and tables are appropriate. In my opinion the manuscript will be suitable for SE (and the special issue) though some revisions are necessary. My overall recommendation is moderate revisions.

My main concerns are:

- The manuscript somewhat misrepresents the advantages and disadvantages of petrophysical properties measured by laboratory measurements compared to those calculated from CPO data, stating insufficient crack closure as the main short coming of laboratory measurements. This is mainly based on two references (Christensen, 1974 and Vasin et al., 2017) and is surely an important aspect to be considered in studies of petrophysical properties. However, it does not capture the bulk of the available literature and it also neglects to mention the simplification made for the calculations based on CPO data (i.e., no cracks, no minor phases, no grain boundaries). This should be treated a bit more openly to capture what the current state of knowledge is.
- Relating to the above point, I am somewhat confused which of the modelled rocks the authors now consider to be the one representative of the crust. Assuming a density of

2.7 g/m³ and lithostatic pressure, 740 MPa corresponds to approximately 28 km, meaning all of the upper crust will be above. At least to me, it is not clear which "average" rock is considered to be representative.

- The authors claim to upscale and "close the scale gap" (e.g., L97) between the kilometer-scale geophysical studies and the centimeter-scale at which samples are measured. This is of course an important task and not much data exists on the scales in between. However, the manuscript essentially averages some of the phases present in the rocks to construct one "average" rock, which is then considered to be representative. This can be done and is an interesting calculation, but it should be represented as such. The crust does not contain only one rock, as is mentioned numerous times throughout the manuscript. Yet, the authors do not discuss their results in the context of the available scale bridging literature (e.g., Okaya et al., 2019; Facennda et al., 2019; Zertani et al., 2020)
- In general, referencing throughout the manuscript is fine, though here and there paragraphs are completely without reference where some are necessary (specifically in the introduction and discussion sections). Some are pointed out in the specific comments below, but I suggest the authors check this again.

Specific comments:

L45-46: It's not really a matter of depth range but of resolution at depth. I suggest rephrasing this sentence including changing "higher depth" to "greater depth".

L47: reference for AlpArray initiative missing

L49: Could you precise what you mean by input parameters? If its petrophysical properties then its redundant and I suggest deleting that part of the sentence

L52: I suggest changing "lower depth" to "shallower depth"

L50-53: I find this misleading. By no means is the CPO only the main contributor to seismic anisotropy at mantle depth. Neither is everything above the mantle dominated by microfractures. I suggest to be a bit more precise here. Also some references are needed here.

L57: suggest changing "single crystal elastic anisotropies" to "single crystal elastic properties"

L59-64: What exactly do you mean by normal depths? Ultrasonic measurements and CPO measurements have been used for decades to deduce petrophysical properties of rocks. For ultrasonic measurements fitting rules exist to obtain crack free velocities (e.g., Ji et al., 2007). Those results obtained from CPO data have other shortcomings: no grain boundaries, no minor phases, no SPO. This sentence should be rephrased.

L65-70: References needed. What is the information that it is not an issue in the mantle based on?

L74: Here would be a good spot to mention what is known about how structural relationships on the km-scale influence bulk petrophysical properties (see references above).

L76: I would go as far as to claim that there is no such thing as a natural isotropic rock.

L127: Figure 1B would benefit from some labels: massifs/units, height, ...

L155: I suggest deleting the cross section and rather include a map that shows the sample locations. I would find that much more helpful. Also, please either change or add a universal coordinate system (preferably UTM).

L160-166: I am not an expert on Alpine geology but I am sure that this information needs some references.

L175: How are the samples related to the Zapport phase? They do not seem to be eclogite-facies.

L206: Which code/software was used for the calculation? Beartex?

L224-233: Were these measurements performed during loading or unloading of the sample? It is well known that crack closure during loading is to some extent irreversible, which is why such data is often measured during unloading. Specifically with the discussion of this manuscript this is an important information.

L244: please be more specific. What signs?

L246: The lines and labels of X and Y direction are hard to see. Also please clarify from which samples these images are. It might also be necessary to provide images of the other samples in the supplementary information.

L251: Table 1: It would be much easier to compare the different mineral assemblages if the minerals were presented in column. I also don't find it particularly helpful to use the Swiss coordinate system. Is there a reason for not using UTM coordinates?

L258: "high mica content", please provide a number, e.g., "up to XX vol.%"

L274 and following: I would suggest to provide the names of the samples that the authors are referring to

L288-289: This statement should be somehow supported, I suggest to provide all CPO data at least in the supporting information/appendix in order to support the findings.

L302: specify if these are lower or upper hemisphere projections

L387: what is the 5:6 ratio based on?

L548: The authors say that the results from the GMS model and the Voigt model are "quite close". Reuss and Hill averages would likely also be quite close as it is known that V and R become increasingly separated at high anisotropy. I think it is fine to use Voigt averages but considering that the resulting velocities are consistently higher it should be noted here that this is an upper bound (Mainprice and Humbert, 1994).

L553-554: atmospheric pressure and 2 MPa results should still be shown in Tab. 4.

L565: If this is a "rough estimate" what would the error be on this?

L579-581: I am wondering why the marble is included in the manuscript at all since it is not considered to be present in a "significant amount". I would also like to get the authors thoughts on the following: The marble has a fairly high contrast to the more abundant gneisses. Might this not be a reason that even at low abundance it could impact the bulk properties on the km-scale (e.g., Facennda et al., 2019). I do not know the answer but am curious. It might be worthwhile discussing.

L653: It is not really clear to me what the authors are getting at. Mica is quasi transversely isotropic, which is well known. If mica is the main contributor to anisotropy the bulk rock will have a similar symmetry.

L673-675: This could be discussed a bit more openly. There is really not that much information on how structural associations influence the bulk signal on such scales available and the topic is surely a matter of debate.

Technical comments:

L54: change “gained” to “obtained”

L123: suggest to add “, and”

L137: change to “were only weakly over...”

L237: “Sample(s)”; delete “s”

L256: “,” after “however”

L258: suggest to change “represent” to “are”

L311: change “anisotropies” to “anisotropy”

L311: I think the authors mean “x100” instead of “100%”. Please also specify how Vp-mean is calculated. Is it the mean of all directions or (Vp-max – Vp-min)/2, which is more commonly used.

L333-L342: I don’t think that 1.5 sentences require their own subsection. I suggest to combine these. If not than “Metabasites” should be 4.3.4

L423: change “sections” to “section”

L432: change “a following” to “the following” and “.” to “:”

L521: change “micaschists” to “mica schists”

L586: suggest to change “determined” to “dominated”

L591: There seems to be a typo in the citation.

L630: add Backus (1962)

L650: aforementioned

L651: has not been well studied

L665: Furthermore

References of the literature mentioned in this review not cited in the article are listed below.

Best of luck,

Sascha Zertani

Oslo, July 1st 2021

References:

Faccenda, M., Ferreira, A. M. G., Tisato, N., Lithgow-Bertelloni, C., Stixrude, L., & Pennacchioni, G. (2019). Extrinsic Elastic Anisotropy in a Compositionally Heterogeneous Earth's Mantle. Journal of Geophysical Research: Solid Earth, 124, 1671-1687. https://doi.org//10.1029/2018JB016482

Ji, S., Wang, Q., Marcotte, D., Salisbury, M. H., & Xu, Z. (2007). P wave velocities, anisotropy and hysteresis in ultrahigh-pressure metamorphic rocks as a function of confining pressure. Journal of Geophysical Research: Solid Earth, 112(B9). https://doi.org/10.1029/2006JB004867

Okaya, D., Vel, S. S., Song, W. J., & Johnson, S. E. (2019). Modification of crustal seismic anisotropy by geological structures ("structural geometric anisotropy"). Geosphere, 15(1), 146-170. https://doi.org/10.1130/GES01655.1