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Reply on RC2

Ruth Keppler et al.

Author 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-AC2, 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.

We thank you for the detailed review. The additional references improved the discussion as well as the introduction and several misleading remarks have been rephrased and are now more clear to the reader.

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.

Thank you for pointing this out. We now added more details on used assumptions and approximations at the end of section 6.1.2 (formerly 5.1.2)

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 average is based on the composition, the CPO and the crack pattern. We model this average rock for different depths, so one can choose input parameters according to the depth of the model. The depth in which we consider the microcracks closed (740 MPa), would be below crustal depth, however rocks of crustal compostion and fabric can still be found at this depth e.g. in subduction zones and collisional orogens. But we fully agree to the criticism, because it was not clear to the reader in the previous text. We now explain this more detail both in section 5.4. and the conclusions.

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.

Yes, indeed several important references were missing and have now been added. In addition the results of these previous studies are now elaborated in the introduction (lines 104-111 in manuscript with changes tracked) as well as the discussion (lines 816-818) and are brought into the context of the manuscript.

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".

Sentence is now rephrased.

L47: reference for AlpArray initiative missing

Reference has been added.

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

This part was deleted.

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

Done.

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.

Additional factors are now mentioned and references have been added.

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

Done.

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.

We agree that there are other factors influencing elastic anisotropy at depths in which microcracks are closed. This is why we now as suggested, mention these at an earlier point. Therefore we do not repeat this here again.

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

Some references have been added and the sentence was rephrased.

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).

These points are now included in lines 104-111 and 816-818.

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

We agree. We rephrased some of section 2.1. to explain that summarizing the isotropic parts in the model is a simplification and more complicated in nature.

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

Done.

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).

The cross section gives the reader a better estimate in the thickness and dip of the respective orthogneiss and paragneiss layers so we prefer to leave in in this figure, but we added the sample locations. The Swiss coordinate system is commonly used in publications about the Adula nappe (e.g. Cavargna-Sani et al., 2014 or Nagel et al., 2008), which we prefer this coordinate system for better comparability. However, UTM was added to the figure.

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

References were added.

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

The samples were deformed during the Zapport phase (e.g. typical NS stretching lineation, and do not seem to be deformed by any of the younger deformation phases.

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

Yes. Name and reference have been added.

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.

Ultrasonic measurements were done during loading. It is true that there is some irreversible closure of microcracks. For the model, we tried matching ultrasonic wave velocities measured at certain pressure levels by adding certain vol.% of microcracks, without relation to the state of cracks in rock massif. A short discussion is added at the end of section 5.5 (formerly 4.6)

L244: please be more specific. What signs?

A more specific description has been added.

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.

The labels for X, Y, and Z directions have been improved and sample names have been added. Since the microstructures of the other samples will be part of another publication, they should not be included in the supplements.

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?

Columns would require an additional figure. In our opinion, such a figure would not contribute much to the manuscript. As mentioned before, most previous publications about the Adula Nappe use the Swiss coordinate system, so using it makes a comparison to these previous publications easier. We therefore prefer to leave this table as it is.

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

Done.

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

This would be a long list for the main text, which is why a table was added in the appendix listing which sample shows which CPO pattern.

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.

The CPO data and microstructures will be part of a separate publication in which the deformation during the Zapport phase will be discussed, which is why we will not include this data in the current manuscript. However, as mentioned before, a list of samples was added in the appendix referring to the specific CPO patterns.

L302: specify if these are lower or upper hemisphere projections

This is already specified.

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

The ratio is based on the frequency of occurrence of each CPO pattern in the sample set.

We now added this information in the text.

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).

We noted that Voigt is an upper boundary of bulk elastic properties. Voigt and Reuss boundaries are in fact the same for the highest possible anisotropy – the single crystal (or single crystal like preferred orientation). And for the random crystallographic texture – meaning isotropic polycrystal – the difference between Voigt and Reuss is maximal [Matthies et al. J. Appl. Cryst. (2001). 34, 585-601]. Though this difference increases with the increase of single crystal anisotropy. Voigt and Reuss averaging schemes provide the same symmetry of elastic properties as self-consistent method.

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

Done.

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

We rewrote this sentence. “Rough estimate” was not the right phrasing, here.

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.

Yes, marble is in fact special in its seismic properties and there is not a large amount of publications on this topic. Discussing this in more detail is a good idea. The discussion of the marble sample is now more elaborate (now chapter 6.1.3.).

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.

Yes, this is actually what we say. Mica is likely the main contributor to the anisotropy. But *what we measure as stretching lineations in the field is more likely the quartz or feldspar lineation. So when trying to correlate our data to field maps it could be a problem because the tilting of mica around the lineation might not be the same as the quartz or feldspar lineation measured in the field. This is already mentioned in section 6.1.2., which is why we do not provide more detail at this point.*

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.

We amended the text accordingly.

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

All technical corrections have been applied.

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

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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