Review of Schmidtke et al.: Elastic anisotropies of rocks in a subduction and exhumation setting

In this paper, the authors measured the crystallographic preferred orientation of the minerals from a variety of metamorphic rocks in the Lago di Cignana area, NW Alps. These data were subsequently employed to calculate the seismic velocities and velocity ratios of different rocks, including eclogite, blueschist, amphibolite, greenschist, micaschist and gneiss. The knowledge of elastic data as provided in this study is very important when we try to distinguish different rock types at depths using seismic methods. The content of the paper is appropriate for the journal, but the current version has still large room to improve. Below are my detailed comments for this paper.

- Thank you for the constructive comments on this manuscript. These have greatly improved the quality of this submission.
  The responses to the comments are listed below in red. All references to specific lines are made to the "changes tracked"-version of the manuscript.
  References mentioned in this response are listed at the end of this document.

Major comments

- Probably the most critical issue in the paper is the representativeness of the studied samples. As I can see, especially for the metabasic rock types, i.e., eclogite, blueschist, amphibolite and greenschist, each rock type has only one sample. Considering large variations of the deformation structure and mineral modal composition even in the same rock type, which has been frequently observed by other researchers, it is therefore difficult to reach a meaningful comparison of the elastic properties between different rock types by solely taking the few samples in this study. I suggest the authors to incorporate more elastic property data from other studies and compare them in a larger data set.

  - This is indeed a very valid argument. In order to better ensure comparability further studies and their respective elastic properties have been added to the discussion. To give the reader an overview of the data presented in this study in comparison to other studies table 3 has been added to the manuscript. In it all referred to studies are mentioned, as well as the methods used to acquire the elastic properties listed therein.

- Some single crystal elasticities that the authors used in this study may not be very suitable. I suggest the authors to choose the more appropriate ones with respect to the mineral compositions. In this sense, some substitutes that the authors used are not necessary. Below are some references to the latest single-crystal elasticities.
  - *Chlorite*: Mookherjee, M., & Mainprice, D. (2014), Unusually large shear wave anisotropy for chlorite in subduction zone settings, *Geophysical Research Letters*, *41*(5), 1506-1513, doi:10.1002/2014gl059334.
  - *Amphibole***:** Brown, J. M., & Abramson, E. H. (2016), Elasticity of calcium and calciumsodium amphiboles, *Physics of the Earth and Planetary Interiors*, *261*, 161-171, doi:10.1016/j.pepi.2016.10.010.
  - *Omphacite*: Hao, M., Zhang, J. S., Pierotti, C. E., Ren, Z., & Zhang, D. (2019), High-pressure single-crystal elasticity and thermal equation of state of omphacite and their implications for the seismic properties of eclogite in the Earth's interior, *Journal of Geophysical Research: Solid Earth*, *124*(3), 2368-2377, doi:10.1029/2018jb016964.

  - The new single crystal data has proven to be a valuable addition to the manuscript and as many samples contained chlorite, all of these samples have been recalculated with the single crystal constants provided in the study by Mookherjee and Mainprice (2014). In the case of actinolite sample MJS36 was also recalculated with the single crystal data from Brown and Abramson (2016). In the case of omphacite we have selected to remain with those by Bhagat et al. (1992) for our calculations as we see it as an appropriate fit. Further in the case of calcite we have recalculated the samples MJS20 and MJS22 using the single crystal data by Chen et al. (2001).

- I think the elastic properties of rocks in this study were calculated using the singlecrystal elasticities measured at the ambient condition, therefore, the effects of pressure and temperature need to be evaluated or at least discussed. This is important for different rocks that are stable at different metamorphic P-T conditions.
    - This is a good point, which is now addressed in section 6.5 (lines 668-670)

- The elastic properties of rocks in this study were calculated using Voigt average. However, to my knowledge, such data were mostly computed using Hill average in the literature. It is okay to use either one for the calculation, but for the purpose of comparisons especially with the data from others', it is recommended to follow the most commonly used method.
    - While Hill average is more commonly used, there are also numerous studies, which apply Voigt averages (e.g. Rasolofosaon et al., 2000; Takanashi et al., 2001; Ivankina et al., 2005; Ullemeyer et al., 2006; Keppler et al., 2015; Ullemeyer et al., 2018). We chose the Voigt approximation to gain the uppermost possible P-wave velocity and avoid an overestimation as well as uncertainties concerning the values of the aijkl parameters. Furthermore, the Voigt averaging approach gives the closest agreement between CPO derived and laboratory-measured seismic velocities (e.g. Seront et al., 1989).
    - We agree that this has to be pointed out; however, we already mention the issue both in the methods part and the discussion of common issues. We added additional statements (lines 129-131 and lines 646-650) to point this out to the reader.

- The structure of Discussion section of the paper feels a bit strange to me. I would recommend to put "CPO development" and "Elastic anisotropies" as the secondary headings, and put "Metabasic rocks", "Metasediments" and "Gneiss" as the third heading. Besides, the content of Vp/Vs ratios, is not related to the elastic anisotropies; and it can be integrated into the Results section.
    - Even so this restructuring is possible the authors of this manuscript wish to remain with the original structure for several reasons. The first of these being, that this makes orientation in the manuscript far easier, as the previous sections "4. Composition and microfabrics of the samples studied" and "5. Results" are already divided by lithology and not by the methods or other categories. Adhering to the order found in "4. Composition and microfabrics of the samples studied" allows the reader to select a specific lithology of interest and filter the manuscript for data on this lithology. Further our manuscript is divided in this way, in order to cater to different audiences. The sections attaining to CPO development are primarily intended for readers from the structural geology and tectonics field, while the elastic properties are of greater interest to geophysicists.

- The elastic properties data in the paper are presented in the formats of texts and tables; and they are often hard to follow, especially when comparisons are made. I think some figures would be needed to help readers catch the points.
    - This is a very valid point. To remedy the dry nature of all the presented numbers, figure 9 has been added, which better illustrates the differences and similarities of the rock properties by the criteria of AVp and VP/VS-ratio, which are often referred to in the discussion and throughout the manuscript (see figure 9).

- When discussing the elastic property data in the context of a subduction and exhumation setting, it would be great to combine them with the P-T data or path of the studied rocks. In this sense, the elastic property data can be presented in a P-T diagram with P-T path for the studied samples. A good example could be the Figure 11 in Park and Jung (2019).
    - Park, M., & Jung, H. (2019), Relationships between eclogite-facies mineral assemblages, deformation microstructures, and seismic properties in the Yuka terrane, North Qaidam ultrahigh-pressure metamorphic belt, NW China, *Journal of Geophysical Research: Solid Earth*, *124*(12), 13168-13191, doi:10.1029/2019jb018198.

- We agree that this is a helpful representation of data in Park and Jung (2019). However, since we do not have PT-data for each of our samples it would not be possible to pinpoint these on an exact place on a PT loop for the Zermatt-Saas zone. This would also be beyond the scope of this manuscript, since we aim toward representative samples for a broad field within different metamorphic facies, instead of samples with a specific PT condition. However, we are thankful for the reference, which is now also included in the discussion.

- The authors need to give more details about the approach they used to estimate the volume proportions of different mineral phases, as well as their uncertainties if possible, because an accurate mineral percentage is critical for obtaining a reliable bulkrock elastic property.
  - The estimates of volume proportions were made on the basis of thin-section microscopy and then compared to the volume proportions calculated during RTA. As the later however is known to be imprecise the estimates from the thin-sections was primarily relied on. This is mentioned in section 3. Methods "The volume percentages of the phases are estimated from thin-sections and calculated in MAUD by RTA.". A paragraph mentioning the possibility of errors resulting from this approach has been added to the manuscripts discussion section.(lines 664-667)

- The descriptions of the structures and textures of different rock types appear a bit simple. I suggest the authors to add more quantitative information such as grain size and grain shape, as well as more optical photographs to show the textures of related samples described in the text.
  - In response to this comment, we have added thin-section photographs of lithologies previously not depicted. A closer analysis of grain shape was not made, as studies have found the influence of grain shape, particularly in sheet silicates, on the bulk elastic properties is limited (Vasin et al. (2013, 2014, 2017); Nishizawa & Yoshino (2001)).

- The authors used F2 index to quantify the CPO strength, which is, to my knowledge, an index not as frequently used as J- and M-indices. It would be great to also provide J- or M-indices, so that a comparison with the results from other literature would be straightforward.
  - As we preform Rietveld Texture Analysis (RTA) in MAUD (Lutterotti, 2010, Wenk et al., 2010) the F2 index is the standard index for texture strength. This index was primarily selected for comparability of the texture strengths among samples within this study. Furthermore the F2 index is also implemented in other software such as BEARTEX (Wenk et al., 1998) and MTEX (Hielscher & Schaeben, 2008).

Minor comments

- Minor comments within the manuscript have been addressed directly therein or in the table below.

line	Comment	Answer
51-60	This part of content can be integrated into the Method section.	As the method used is a key component of this manuscript, we wish to keep this content in the introduction. It is of course explained in greater detail in section 3.
128-132	Here I think the authors mainly talked about the method of retrieval of CPO, rather than the calculation of elastic properties. It would be better move this part of content to the paragraphs above.	This is indeed the case. The paragraphs in question have been moved to the section above concerning Rietveld Texture Analysis (RTA) in MAUD.

201	Is the RTA dervied CPO an equivalent to one-point-per-grain or one-point-per-pixel CPO or other else in the EBSD derived data?	Since sample volumes are measured in neutron diffraction, the methods are difficult to compare. Depending on the grain size and the volume percentage of each mineral phase several thousands of grains are measured or more. However, one large grain weighs in as much as several smaller grains that add up to the same size, so the CPO is more comparable with point-per-pixel, or maybe "point-per-unit-of-volume".
202	What is F2 index and its physical meaning? It is better to give a short definition in the Method section.	The definition of this index is a very valuable addition to the methods section. A paragraph containing a brief explanation has been added to the method section. In summary the F2 texture index is OFD based. In it a completely random texture (e.g. powder sample) would result in an index = 1, while a single crystal would result in an index = ∞ (Matthies et al., 1997).
211	It would be great to provide such data in the supplementary figures. The same for below.	As the textures are random we have chosen not to depict these.
217	Clinozoisite and epidotite have very similar crystallographic structures. Could you describe a bit about how you separate their CPOs using TOF neutron diffraction and RTA.	These minerals are indeed very similar, so that a separation of their CPOs using TOF neutron diffraction and RTA is not really possible. Thin sections were used for determination of mineral phase and volume percentage, and the according phase was chosen for the RTA.
245	How to separate calcite and dolomite, which have very similar crystallographic structures.	Calcite and dolomite different d-spacings that can be clearly separated in the spectra.
281-282	how about the S1 polarization anisotropy?	Values have now been added to a table. They are nor discussed in detail, since anisotropies are relatively low.
284	How did you calculate the average Vp and Vs? Please provide the formula somewhere in the text.	The average velocities were calculated by the VRH-averaging scheme as described in section 3 (lines 125-131).
353	Why not formed during the retrograde deformation event or during exhumation? The amphibolite appears to be the retrograde product from eclogite.	This would also be a possibility, however as stated in the manuscript, the assumption is made due that these are prograde mineral growth. However the exact timing of mineral growth is not the topic of this manuscript and it is primarily important that this growth took place at blueschist facies conditions.
559	How about the effects of the mineral assemblage changes with P-T condition on the elastic properties?	This is an interesting point, however, data on single crystals at different conditions is missing for most minerals used in this study. However, we now elaborate this in the discussion.
576-578	The key issue is whether one or several samples can turely reprenent each rock type. Especially for the meta-basic rocks, eclogite, blueschist, amphibolite and greenschist.	This is true, however as the samples are compared with literature data (now also listed in table 3), we feel confident in the choice of samples and that these can be seen as representative of the lithologies in question.
591-592	Could you provide references here. To my knowledge, many elastic properties data were calculated using Hill average.	This question is answered in detail above and has been expanded upon in the manuscript.

598-599	But some rock types, such as greenschist, are stable at low pressure condition in which microcracks could be ubiquitious.	You are right, at lower pressures these will become a factor. We briefly discuss this in the current issue, but there is also an elaborate discussion as well as data on microcracks in our companion paper (Keppler et al.).

Fig. 1c. It is better to mark N-S-E-W directions in the stereonets.

The cardinal directions have been added to the stereonets in figure 1.

Figs. 4-8. Please mark foliation and lineation (or x, y, z-axes) in the pole figures.

This is indeed a very sensible addition and said marks have been added to the mentioned figures.

Fig. 8. Please provide labels of subfigures and use them in the text. It is hard to follow the text without a subfigure label.

All subfigures of figure 8 have been labeled and are now referred to by these labels throughout the text.

References:

Brown, J. M., & Abramson, E. H.: Elasticity of calcium and calciumsodium amphiboles, Physics of the Earth and Planetary Interiors, 261, 161-171, 2016. doi:10.1016/j.pepi.2016.10.010.

Chen, C.-C., Lin, C.-C., Liu, L.G., Sinogeikin, S.V., Bass, J.D.: Elasticity of single-crystal calcite and rhodochrosite by Brillouin spectroscopy, American Mineralogist, Volume 86, pages 1525–1529, 2001.

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Lutterotti, L.: Total pattern fitting for the combined size-strain-stress-texture determination in thin film diffraction, Nuclear Inst. and Methods in Physics Research, B, 268, 334–340, 2010.

Matthies, S., Lutteroti, and L.,Wenk, H.R.: Advances in Texture Analysis from Diffraction Spectra, J. Appl. Cryst. 30, 31–42, 1997.

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Vasin, R.N., Lebensohn, R.A., Matthies, S., Tome, C.N. & Wenk, H.-R.: The influence of grain shape and volume fraction of sheet silicates on elastic properties of aggregates: biotite platelets in an isotropic matrix, Geophysics, 79, 433–441, 2014.

Vasin, R.N., Wenk, H.-R., Kanitpanyacharoen, W., Matthies, S. & Wirth, R.: Elastic anisotropy modeling of Kimmeridge shale, J. geophys. Res., 118, 3931–3956, 2013.

Wenk, H.-R., Lutterotti, L., and Vogel, S.C.: Rietveld texture analysis from TOF neutron diffraction data, Powder Diffraction 25, 283–296, 2010.

Wenk, H.-R., Matthies, S., Donovan, J., and Chateignier, D.: BEARTEX, a Windows-based program system for quantitative texture analysis, J. Appl. Cryst. 31, 262–269, 1998.

Comment on se-2021-3

Bjarne Almqvist (Referee)
Referee comment on "Elastic anisotropies of rocks in a subduction and exhumation setting" by Michael J. Schmidtke et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-3-RC2, 2021
Schmidtke et al.
"Elastic anisotropies of rocks in a subduction and exhumation setting"

Submitted to Solid Earth Discussions

In this study Schmidtke and co-authors present a set of results for calculations of seismic properties of different rocks in a subduction environment, including eclogites, blueschists, amphibolites, greenschists, metasedimentary rocks and gneisses. The paper is generally well written but some effort can be spent in carefully going through the text (I've made some suggestions in an attached pdf). Given the scientific content, the study is interesting and contributes some valuable results. The points made on the greenschists are most noteworthy. However, there are things that can and should be improved.

Thank you for the constructive comments on this manuscript. These have greatly improved the quality of this submission.
The responses to the comments are listed below in red. All references to specific lines are made to the "changes tracked"-version of the manuscript.
References mentioned in this response are listed at the end of this document.

General comments I think the main thing that is currently missing are 1) a discussion on the larger implication of the results seismic anisotropy in a subduction/exhumation zone environment and 2) a comparison of results obtained in this study with actual observations in a subduction zone and exhumation environment. These two parts can be added to the discussion and would provide a broader perspective of the results in this study.

These points are of great interest and we added further references and discussed the characteristic lithologies/compositions in further detail. Thereby, we attempted to put our results into the context of subduction and exhumation. The vertical seismic resolution in the depth range of our sample set is currently on the order of 10 km and therefore above the thickness of the investigated lithological units which is in the order of 1 km or below. Therefore, it is still speculative to directly compare our results with large-scale geophysical models. That remains a future perspective when continuous technical and processing improvements further increase the seismic resolution.

Geophysical models are usually structured by prominent changes in P-wave velocities, for example, the Conrad discontinuity separating the upper from the lower crust. For that the transition is defined by a drop from a VP of 5.3 km/s above to 6.5 km/s below the discontinuity. As seen in this study and many others, fluctuations in such a range of VP can be achieved in a single lithology making the interpretation of lithospheric stacks in collisional orogens solely based on geophysical imaging difficult or even impossible. Only the combination of a variety of geophysical methods, geological surface data, and tectonic interpretation furthermore remains the best approach to unravel the geodynamic structure and evolution of collisional orogens.

Please indicate the sample reference frame in the figures containing pole figures (4-7) and calculated wave speeds (fig. 8)

As requested, reference frames have been added to each of the figures in the manuscript.

Please also report the S wave anisotropy in the results and discussion. These were apparently calculated and reported, but is only reported briefly in Table 2. What about shear wave splitting, what role does this have in seismic anisotropy?

It plays a role, however the differences are relatively small, which is why we do not include an elaborate discussion. The AVS1-% and AVS2-% values have now been included in table 2.

Related to the point above. How was the Vp/Vs ratio calculated? Were the isotropic seismic properties calculated to obtain Vp/Vs, or is this parameter calculated in some other way?

The VP/VS-ratio was calculated from the isotropic seismic properties. The calculation of the velocities is explained in the methods section of the manuscript (lines 115-131).

Elastic anisotropy is continuously referred to in the manuscript. I can understand why, but really the calculated seismic anisotropy is reported (P wave anisotropy and S wave anisotropy). In addition, it should be made explicitly clear what anisotropy is referred to, i.e., AVp (%) and not just using an A (%).

This is again a very valid point. To avoid any confusion this has been rephrased throughout the manuscript and in all figures.

The anisotropy reported in this study are never that high (AVp is max 8.2 % for micaschist), and therefore it is probably better not to not "high" anisotropy, but rather "intermediate". The results in this study are furthermore interesting because they represent low anisotropy in general, which contrasts considerably with other studies cited in the paper and further non-cited papers. I think a more in depth discussion on this would make a valuable addition to the paper.

We agree that the anisotropies cannot be considered "high" and rephrased this throughout the manuscript. Concerning a discussion on the generally low anisotropies of our samples, we only partially agree. We are making comparisons with values from other literature for each lithology (see lines 432-462; lines 494-497; lines 542-550; and in table 3), and our anisotropies are always within the range, or even close to the average. The exception is the blueschist sample, which shows lower values indeed. However, this is already discussed in detail in section 6.1.2 in the lines 448-458.
When comparing elastic anisotropy values calculated from EBSD to those measured with neutron diffraction the latter ones are often lower. As far as we can tell, an important factor causing slightly lower values for elastic anisotropy in our samples is the fact that we measure the CPO of large sample volumes. In our experience, EBSD analysis frequently yields stronger mineral CPO, even in the same sample, since only selected sample surfaces are measured. Bulk CPO determined by TOF neutron diffraction therefore likely produces more reliable overall elastic anisotropy.

There is actually literature on the elastic constants of chlorite, and although these constants are predicted through ab-initio calculations, these constants should be considered or compared with (see Mookherjee and Mainprice, 2014, Geophysical Research Letters; this reference is of particular interest to S wave anisotropy, but do contain the full elastic stiffness tensor). There are also more up to date elastic constants available in the literature for different minerals (for example amphiboles by Brown and Abramson, 2016, Phys. Earth. Planet. Int)

This is also a very valuable addition to the manuscript and as many samples contained chlorite, all of these samples have been recalculated with the single crystal constants provided in the study by Mookherjee and Mainprice (2014). In the case of actinolite sample MJS36 was also recalculated with the single crystal data from Brown and Abramson (2016). Only in the case of the barroisite we have chosen to remain with our previous substitution by the single crystal data of glaucophane. Reasons for this are given in the discussion section (lines 455-457 and lines 635-645), however in short it can be summarized as glaucophane also being a good fit for a more general blueschist rock. Further in the case of calcite we have recalculated the samples MJS20 and MJS22 using the single crystal data by Chen et al. (2001).

The referencing in this study needs to become more inclusive. For example, there are several relevant references to Shaocheng Ji's group with focus on mica and amphibole bearing rocks. In addition, there are papers by Sasha Zertani that are relevant (2019 in Journal of Geophysical Research and 2020 in Geochemistry, Geophysics, Geosystems).
Other relevant references include:
Bascou et al. Tectonophysics 2001
Worthington et al. 2013, Geophysical Journal International
Wenning et al. 2016, Tectonophysics and Kästner et al. 2020, Solid Earth (on measurements of elastic wave speed anisotropy in metasedimentary rocks and amphibolites, from drill core in the Scandinavian Caledonides).

> Many of these references have been added to the manuscript and further studies have been consulted. Furthermore, table 3 has been included, which contains the elastic properties from all studies referred to in the discussion and throughout the manuscript. This table includes a listing of the methods used to acquire the elastic properties therein and gives the reader a quick overview of our results in the context of other studies.

The work of David Okaya may also be of relevance, in particular the larger scale papers on seismic anisotropy. In any case, a broader referencing is really needed and these are just some suggestions (there are likely some useful additional papers of Nik Christensen and David Fountain, which are a bit older but still important).

> We agree. We broadened our general discussion of elastic anisotropy and included these and further references (lines 583-585 and lines 658-663)

When results of anisotropy are compared from different studies in the discussion, it needs to be made clear what reults are based on laboratory measurements and what are based on texture derived calculation.

> In response to this comment table 3 containing both the results of this study, as well as all mentioned literature values including a differentiation between laboratory and calculated data has been added. We hope this will better illustrate how our values fit into the data from other studies.

Perhaps a bit of interest for the authors, in 2017 I was involved in a paper that predicted seismic seismic anisotropy fairly weakly anisotropic rocks from a magmatic arc (Cyprych et al., 2017: Earth Planet. Sci. Lett.). We used the ESP toolbox of Vel et al. (2016) to predict seismic anisotropy from texture derived EBSD as well as the microstructural arrangement of minerals in the rocks. When solely including the texture derived anisotropy we obtained a weak predicted seismic anisotropy. This anisotropy increased somewhat when including the microstructural arrangement of minerals in the calculations (in addition to the texture), but more importantly the symmetry of anisotropy changed completely. Given the fairly small anisotropy presented in the current study, as well as presence of minerals with high elastic contrast (notably garnet), it may be of interest to consider or at least discuss potential of microstructural arrangement contributing to anisotropy.

> We are grateful for this suggestion and we now mention this aspect in section 6.5 (lines 651-652). SPO and the overall microstructural arrangement is indeed an interesting topic when dealing with elastic anisotropy of rocks. However, since we are aiming for generally representative samples for each lithology and the microstructures are very variable, even at outcrop scale, we decided to focus on the CPO related anisotropy in our data.

Further comments are provided in the attached pdf.

> The further comments within the manuscript have been addressed directly or in the table below.

line	Comment	Answer
423	there are no s wave data shown in the paper. How was Vp/Vs calculated?	The S-wave data has been added to the manuscript and is now featured in table 3.
425-426	how are these single crystal Vp/Vs obtained?	The single crystal Vp/Vs were obtained by calculating the average velocities as we have done for all minerals in the study (section 3) and simply dividing the average velocities.
542-543	I think this is interesting. can the authors here provide a list of known existing references to seismic properties studies of greenschists (if any exists)?	Unfortunately we know of no such studies on greenschists, otherwise we would have listed them here and in table 3. We hope our data presented here will be of use to others studying this lithology in the future.
591-593	Many calculated wave speeds are based on the Hill average. I think when you discuss this point, it is also necessary to mention how big of difference you obtain when calculating Voigt and Reuss bounds. What are you Reuss bound velocities? How much do they differ from the Voigt bounds?	This would indeed be an interesting factor to analyze in future studies, yet as we have selected to use the Voigt approximation throughout this study, Reuss bound velocities were not calculated. As a result no comment on the difference of these velocities can be made at this point. The reasons for our choice of the Voigt averaging scheme are detailed in the method part, however in summary it was selected to gain the uppermost possible P-wave velocity and avoid an overestimation as well as uncertainties concerning the values of the a_{ijkl} parameters.

References:

Brown, J. M., & Abramson, E. H.: Elasticity of calcium and calciumsodium amphiboles, Physics of the Earth and Planetary Interiors, 261, 161-171, 2016. doi:10.1016/j.pepi.2016.10.010.

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