Solid Earth Discussions

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Reply on RC2

Michael J. Schmidtke et al.

Author 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-AC2, 2021

Response to:

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 bold type. 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 .....

- **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:

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