Solid Earth Discussions

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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.

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.

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

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?

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?

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 (%).

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.

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)

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).

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).

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.

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.

Further comments are provided in the attached pdf.

I hope these comments are of use to the authors.

Bjarne Almqvist, Uppsala, 2021-03-22

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2021-3/se-2021-3-RC2-supplement.pdf