Review Report on Solid Earth MS#: se-2021-27
Title: Elastic anisotropies of deformed upper crustal rocks in the Alps
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Special Issue: New insights into the tectonic evolution of the Alps and the adjacent orogens

Dear Solid Earth Topical Editor M. Vrabec,

Following the instructions provided in your Referee comment request, I present below my report organized in the three sections expected. Rather than introducing corrections and comments in the pdf article file provided (which I have not editted), I list below my viewpoints to help discussion and manuscript improvement, if the authors consider it appropriate.

1. General comments

I have read Keppler and co-workers' article various times with personal interest and attention and, in my opinion, it is adequate for publication in the Solid Earth aforementioned special issue, as it deals with a classic major nappe of the Alps Lepontine dome as well as with the use of sophisticated fabric analysis techniques, derived calculations and rock geophysical measurements applied to metamorphic rocks that record a poly- orogenic geological history. The results of this study may also constitute relevant inputs for ongoing geophysical studies of the Alps (the manuscript mentions the AlpArray high-end seismological array experiments) and in other similar initiatives elsewhere. The text is well written and organized in general, and the figures that illustrate the manuscript are correct and appropriate. For these reasons I would recommend acceptance of the article, though there are some items that, in my opinion, should be clarified and improved previously, all this requiring a minor/moderate revision. I explain below my comments and suggestions in a long section 2 dealing with specific comments and a short section 3 including a few technical corrections.

2. Specific comments

a. "The upper crust within collisional orogens". The authors mention several times in the abstract, conclusions and elsewhere in the MS that they are studying typical lithologies from deformed upper crustal rocks in the Alps. I am mystified by the statement since the rocks dealt with are ortho- and paragneisses that underwent intense ductile deformation coeval with amphibolite facies metamorphism under relatively high pressure conditions (or truly high-P in the eclogite facies). It is not a surprise that the authors describe it in their texts and support it with references. However, what do they actually mean with "upper crustal rocks"? Currently they are in the upper crust, but they did not acquire their principal characteristics in this realm, but at great crustal depth. When the authors design their rock ultrasound velocity experiments at different confining pressures up to 400 MPa they are implicitly admitting at least mid-crustal ambient conditions. The authors should revise what they actually mean and, if necessary, rewrite the parts of the MS where they include the label "upper crustal rocks".

b. On the point of elastic anisotropy measurement and calculations. The authors deal with seismic anisotropy directions in the Alps (with supporting citations) and rock average seismic velocities/anisotropies to support the interest of rare studies as the presented here on gneisses. However, though I can foresee, it is not clear for me (after the current text) which are the specific relationships implied or the eventual applications. On one hand, the authors report on seismic anisotropy parallel or transversal to the orogenic surface trend, but its origin (attending to the publications cited) is teleseismic wave birrefringence essentially generated in the mantle. The authors mention mineral and stretching lineations of surface metamorphic rocks to support geometrical relationships (that I do not challenge at all), but their contribution to teleseismic signal anisotropy likely is very small due to the relative short tract of crustal segments compared to mantle ones in wave rays, and the still shorter of the uppermost crust (in spite of being much anisotropic). So, which is the contribution expected of crustal rock anisotropy to teleseismic wave anisotropy? Is it relevant? Would it help/hinder seismic lithological reconnaissance along profiles with different orientations? Please, explain. On the other hand, the authors concentrate of the interest of determining "average rock" geophysical properties, but it may be misleading. I wonder if a highly anisotropic medium such as de Adula nappe (with a gentle-dip layered organization of interleaved ortho- and paragneiss units meter to tens or a few hundreds of m thick, and their internal penetrative foliation/lineation fabrics) would be seismically imaged as a coherent unit, nearly transparent in reflection seismology experiments and distinguishable from over- and underlying units in seismic refraction. In the latter case "average rock" properties might be sound. However, the actual lithological organization of the Adula nappe appears to be prone to produce several reflections. In this regard, it should be noted that, rather than the thickness of layers, the key resides in the existence of acoustic impedance contrasts across contacts, which abound between ortho- and paragneisses, and between them and scarcer marble and metabasite lenses, as described in the manuscript. As a conclusion to this item, I consider the authors should describe to what aspect of seismic studies their data and results intend to make a significant contribution.

c. The regional geology of the Adula nappe. The authors present a succinct description of this nappe and illustrate it with a simplified map and a cross section parallel to the regional lineation. In a brief bibliographic survey on the nappe I have found outstanding maps and cross sections that provide a much more precise image of the organization of the unit. I am not sure if I am allowed to include excerpts of them in this report, but in any case they can be accessed, for example, in M. Carvagna-Sani's thesis (2013, Université de Laussanne, "The Adula nappe: stratigraphy, structure and kinematics of an exhumed high-pressure nappe") and related publications on the Adula nappe. They are not cited in this manuscript and, though do not contradict the descriptions presented in this article, they are outstanding as complementary structural support that likely merits citation of consideration to redraw the Fig. 2. Complementary to this, it should be mentioned that the orthogneisses correspond to two groups of different age: Ordovician and Permian. Also, now in relation with the regional geology, in the line 173 (and then in the 281) the authors refer to the "Zapport" phase, which likely is unknown to most readers unfamiliar with Alpine geology. Please, explain what is understood as the "Zapport" phase (pervasive Alpine deformation phase in the northern part of the Adula nappe, associated with the regional foliation, isoclinal folding with approximately N-S fold axes and N-S stretching lineation associating top-to-the-North sense of shear), state its age, and (maybe as a colateral must) that there also exist younger regional phases with local names (Leis and Carassino).

d. Comments on the cross section of the Alps (Fig. 1A), the (over) simplified Fig. 1B and the subdivision of the crust into weakly deformed isotropic upper crust and strongly deformed anisotropic Alpine upper crust (sections 2.1 and 2.2). Apart of the already discussed above on the clarification of the actual meaning of "upper crust", I suggest the authors should reconsider their writings in this section. I understand what the authors intend, but I feel it is an oversimplification not supported by the regional geology as I explain. First, consideration of the crystalline external massifs and the Adriatic basement as recorders of weak or disregarded elastic anisotropy contradicts the fact that these massifs record the imprint of pervasive orogenic deformation, metamorphism and magmatism in internal zones of the Hercynian orogen (se below some Von Raumer and co-workers' articles on the subject). Also, in several cases the evidence exists that the Hercynian chain reworked an older Cadomian orogen.

Von Raumer, J.F., Stampfli, G.M. and Bussy, F., 2003: Gondwana-derived microcontinents - the constituents of the Variscan and Alpine collisional orogens. Tectonophysics, 365: 7-22.

Von Raumer, J.F., Bussy, F. and Stampfli, G.M., 2009: The Variscan evolution in the external massifs of the Alps and place in their Variscan framework. C.R. Geoscience, 341: 239-252. DOI: 10.1016/j.crte.2008.11.007.

Von Raumer, J.F., Bussy, F., Schaltegger, U., Schulz, B. and Stampfli, G.M., 2013: Pre-Mesozoic Alpine basements – Their place in the European Paleozoic framework. Geological Society of America Bulletin, 125: 89-108. Doi: 10.1130/B30654.

Additionally, during Mesozoic hyperextension in the Alps, several crustal units of distal parts of the involved plate margins (e.g. the Adula nappe basement) underwent widespread ductile deformations that generated foliations, lineations, and elastic mechanical rock anisotropy (there exists also a considerable bibliographic background on this topic that the authors might know, or even articles authored). All this predated the Cenozoic Alpine orogeny, of course, but likely those units are mechanically as anisotropic as those resultant of the Alpine evolution. In my opinion, all this should be acknowledged in the relevant parts of main text, even though actually the authors arrive at their current positions on crustal organization for future modelling.

e. The authors follow a correct linear procedure in explaining how they record mineral rock fabrics and, together with experimental mechanical constant determinations available, follow calculation procedures to quantify ideal rock mechanical properties that then are compared with laboratory measurements in real rocks. This has been done previously in several occasions with peridotites and eclogites and the authors cite the relevant literature grouped into publications dealing with calculations and others dealing with measurements. However, some of the citations contain the two types of information and their appearance in only one group might be misleading. Complementary to this, it is remarkable the contrast between eclogites and gneisses in this regard. Less than five citations relate to gneisses because there exists a real lack of such data. Notwithstanding, there are some references (classical and recent) that might be included in support of the authors' statements (both in the section 2 and in the upcoming). These include the classic works of Christensen, Fountain, Ji and co-workers, such as the listed below (from which a few might be picked) and a recent one on ortho- and paragneiss petrofabrics in a tectonic/metamorphic context similar to that of the Adula nappe (Puelles et al., 2018, J. Metamorph. Geol., 36, 225-254).

Christensen, N.I., 1965. Compressional wave velocities in metamorphic rocks at pressures to 10 kbar. Journal of Geophysical Research, 70, 6147-6164. Doi: 10.1029/JB084iB12p06849.

Christensen, N.I., 1979: Compressional wave velocities in rocks at high temperatures and pressures, critical thermal gradients, and crustal low velocity zones. Jour. Geophys. Res., 84: 6849-6857.

Christensen, N.I. and Fountain, D.M., 1975: Constitution of the lower continental crust based on experimental studies of seismic velocities in granulite. Geol. Soc. Amer. Bull., 86: 227-236.

Christensen, N.I. and Mooney, W.D., 1995: Seismic velocity structure and composition of the continental crust: a global view. Jour. Geophys. Res., 100 B7: 9761-9788.

Fountain, D.M., Arculus, R. and Kay, R.W. (Eds.), 1992: Continental Lower Crust. Elsevier, Amsterdam: 485p.

Ji, S. and Salisbury, M.H., 1993: Shear-wave velocities, anisotropy and splitting in high-grade mylonites. Tectonophysics, 221: 453-473.

Ji, S., Salisbury, M.H. and Hanmer, S., 1993: Petrofabric, P-wave anisotropy and seismic reflectivity of high-grade mylonites. Tectonophysics, 222: 195-226.

Ji, S., Wang, Q. and Xia, B., 2003a. Handbook of Seismic Properties of Minerals, Rocks and Ores. Polytechnic International Press, Montreal, 630 p.

Ji, S., Wang, Q. and Xia, B., 2003b. P-wave velocities of polymineralic rocks: comparison of theory and experiment and test of elastic mixture rules. Tectonophysics, 366, 165-185. Doi: 10.1016/S0040-1951(03)00094-5.

f. Methods. Sample preparation for ultrasonic wave measurement. I am intrigued about how the authors prepared "roughly spherical" rock samples for neutron time-of flight and ultrasonic measurements, as it is not explained in the main text (lines 183-84 and 227). Where they prepared as polyhedra with several facets cut with a discoidal saw, or were they prepared with a spherical grinding machine?

g. Sections 4.5 and 4.6. From my viewpoint, some parts of the texts included in these sections explain methods rather than results and might possibly be reorganized.

h. Theoretical and measured densities used to estimate crack porosity (lines 433-440). In principle the approach to quantify the porosity associated to microcracks may be reasonable, but the 1.7% value appears to me excessive for these rocks. The authors should be aware that considering the actual Ca-Na relationship of gneiss feldspars instead of albite may change the result, as well as would do consideration of the experimental P and S wave data presented to calculate gneiss sample Poisson's ratios, and after it their density under different confining pressures. Since these rocks depart from perfect incompressible materials (with theoretical Poisson's ratios of 0.5), the actual volume changes due to pressurization/decompression should not be ascribed exclusively to microcrack porosity and lowers the 1.7 estimation.

i. Microcracks and the origin of anisotropy at low confining pressure. This is an interesting matter of debate in these rocks and I suggest to include some additional descriptions and points of view. It is out of discussion the relationship between microcracks and seismic velocity magnitude and anisotropy at low confining pressure, as well as the geometrical relationships between microcrack and mesoscopic penetrative structures. In my opinion the microcrack descriptions provided in the manuscript can be improved with the help of the microphotographs presented and, likely, with additional observations. The micrographs presented correspond to standard rock sections normal and parallel to the foliation. In them, in principle would be visible microcracks (intra- and trans-granular) normal to the three principal fabric planes. If the microcracks are narrow rather than wide, it is due to the fact that they form an angle close to 90º with the plane of the image. Alternatively, if they are wide it might be due to either they are open cracks or they are oblique to the image section (actually derived of a 30 µm thick slice) and apparently are wider. These considerations should be borne in mind prior to discussing on crack opening estate. In several microphotographs (and the main text) the authors highlight as microcracks mica exfoliation planes and I do not find convincing evidence in support of that assignation. The reason why those planes are optically individualized by contrast with other cleavage planes can be variable (opaque mineral nanoinclusions, sheared surfaces) and the low mechanical coherence of mica basal planes contributes to it. The authors should provide clear evidence to support those planes are true microcracks. The fact that some mica grains also contain irregular microcracks normal to cleavage planes might support stress states with the plane containing the maximum and minimum stress directions normal to mica cleavage planes. In the case of quartz (notably) the presence of microcracks usually normal to the mineral shape elongation is doubtless and likely record stress relaxation along the lineation direction. No argument against it. However, the Appendix Figure 2 provides clues on the presence of mechanical discontinuities with the same orientation normal to the lineation that parallel to, or laterally grade into fluid inclusion trails. These and other similar features can be thoroughly identified as healed microcracks and are common (though usually overlooked) in quartz-bearing rocks. They denote brittle strain accommodation and immediate crack healing in the presence of fluids under geologically low T conditions, but not as low as those prevailing at the terrain surface. This may be related with the sentence included in lines 556-558: "It is likely that another system of thinner microcracks is required to match the GMS model and experimental ultrasonic wave velocities in RK15-22 558 at low pressure values" and may be also relevant for the discussions raised in the lines 566-572 and 617-618. There exist a background of scientific articles dealing with these intra-granular penetrative microstructures, published during the last two decades, from which the authors may be aware and that might be taken into account in the discussion section in order to explain the progressive seismic anisotropy decrease and velocity increase until stabilization at significant confining pressures (600-700 MPa, equivalent to crustal depths well above 10-15 km). All this would also apply to discussion on the effect in velocity and anisotropy of other ubiquitous mechanical coherence discontinuities with close geometrical relationships to the macroscpic rock fabric: the grain and subgrain boundaries between identical and different mineral phases. I include below some bibliographic citations that might help.

Derez, T., Pennock, G., Drury, M. and Sintubin, M., 2015. Low-temperature intracrystalline deformation microstructures in quartz. Journal of Structural Geology, 71, 3-23.

Kjøll, H.J., Viola, G., Menegon, L. and Sørensen, B.E., 2015. Brittle-viscous deformation of vein quartz under fluid-rich lower greenschist facies conditions. Solid Earth 6, 681-699.

Palazzin, G., Raimbourg, H., Stünitz, H., Heilbronner, R., Neufeld, K. and Précigout, J., 2018. Evolution in H2O contents during deformation of polycrystalline quartz: an experimental study. Journal of Structural Geology, 114, 95-110.

Raghami, E., Schrank, C. and Kruhl, J.H., 2020. 3D modelling of the effect of thermal-elastic stress on grain-boundary opening in quartz grain aggregates. Tectonophysics, 774, 228242. Doi: 10.1016/j.tecto.2019.228242.

Richter, B., Stünitz, H. and Heilbronner, R., 2018. The brittle-to-viscous transition in polycrystalline quartz: an experimental study. Journal of Structural Geology, 114, 1-21. Doi: 10.1016/j.jsg.2018.06.005.

Schmatz, J. and Urai, J.L., 2011. The interaction of migrating grain boundaries and fluid inclusions in naturally deformed quartz: a case study of a folded and partly recrystallized quartz vein from the Hunsrück Slate, Germany. Journal of Structural Geology, 33, 468-480.

Stünitz, H., Thust, A., Heilbronner, R., Behrens, H., Kilian, R., Tarantola, A. and Fitz Gerald, J.D., 2017. Water redistribution in experimentally deformed natural milky quartz single crystals - Implications for H2O weakening processes. Journal of Geophysical Research, Solid Eath, 122, 866-894. Doi: 10.1002/2016.JB013533.

Tarantola, A., Diamond, L.W. and Stünitz, H., 2010. Modification of fluid inclusions in quartz by deviatoric stress. I: experimentally induced changes in inclusion shapes and microstructure. Contributions to Mineralogy and Petrology, 160, 825-843.

Trepmann, C., Hsu, C., Hentschel, F., Döhler, K., Schneider, C. and Wickmann, V., 2017. Recrystallization of quartz after low-temperature plasticity – the record of stress relaxation below the seismogenic zone. Journal of Structural Geology, 95, 77-92. Doi: 10.1016/j.jsg.2016.12.004.

j. The authors estate in the lines 530-531 that: "The CPO of quartz and mica was not necessarily formed at the same time and could represent different deformation stages", in order to explain apparent discrepancies between CPOs and experimental velocity patterns. Appart of this being difficult to support with the microstructural features described and shown in the Figure 3 (suggestive of coeval mineral fabric development), likely it is unnecessary, bearing in mind the contrasting velocity distribution patterns in the minerals considered (notably quartz and mica), controlled by their crystallography. I suggest removing the sentence.

3. Technical corrections

Line 50. Check the correctness of "is" (are) for anisotropy data.
Lines 53-54. Revise "...can be either be..."
Lines 59, 590, 671 and 726-728. The correct citation year of Ábalos et al. is 2011, not 2010. In the reference list it is wrong, too.

Lines 191-92, 504, 595 and 672. Check the correctness of citing articles submitted to the same journal issue or in preparation (lines 578-579).
Lines 211-212. Avoid one-sentence paragraphs.
Line 220. Check the form of presentation of citations. Should it be "e.g., Vasin et al. (2013),..." instead as (Vasin et al., 2013; ...)?
Lines 237-270. This section describes petrographic data and likely should be presented as an independent section prior to the "Results" section.
Line 253. "kalifeldspar" is used to explain the kfs abbreviation. Is it correct instead of the "Kfs" or "K-feldspar" terms of the usually recommended after Whitney and Evans (2010)?
Lines 239, 256, 380. Here "potassium feldspar" is used, see previous comment.
Line 311. "P-wave anisotropies ... are (instead of is) defined..."
Lines 313, 328, 329, 340, 350, 493, 494, 502, 509, 524, 535 and 653. Add "the" to "... in lineation direction".
Line 646. Add a comma (,) after "collected".

I expect these comments be of help.

Best wishes

B. Ábalos
Bilbao (Spain), April 29, 2021