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Interactive comment on "Mantle flow under the Central Alps: Constraints from non-vertical SKS shear-wave splitting" *by* Eric Löberich and Götz Bokelmann

Anonymous Referee #3

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The authors aim to characterize geodynamic processes in the Western and Central Alps from the azimuthal variation of shear-wave splitting measurements of core-mantle converted S-phases (S(K)KS) considering their non-vertical incidence. The applied method allows to differentiate between a mainly lithospheric or asthenospheric origin for the measured anisotropy, as the polarity of the fast axis variation shows opposite sign. The mechanisms relate to b-up and c-up olivine alignment resulting in horizontal foliation for the asthenospheric flow model and vertical foliation for vertical coherent deformation. They apply the analysis to previously published shear-wave splitting measurements at permanent stations in the Western and Central Alps first for the full study area and subsequently to a northern and a southern subarea based on a travel time in-

tegrated dvp-model of Koulakov et al. (2009). Their findings favour the asthenospheric flow model as origin for the anisotropy for the data set of the full data set and the northern subarea. The southern subarea allows no precise statement, most likely due to the limited event coverage. They conclude, that the anisotropy in the region is originated by a Poiseuille flow driven by the Apenninic slab rollback, likely to coincide with a Couette flow linked to the absolute plate motion. The authors argue also, that a mainly lithospheric origin for the southern subregion is to be expected, due to a dominant effect of the slab.

GENERAL COMMENTS

The method applied in this paper has the potential to improve our understanding of the link between geodynamic processes and measured anisotropy. It is promising with respect to the huge data set of the previous publication, that similar results could be found here for the Central Alps indicating an asthenospheric origin.

Nevertheless, there are some major and minor issues, which give rise to questions. The study neglects important aspects and possibly equivalent models, that could explain the analysed data. Due to this incompleteness the conclusions drawn in this paper remain ambiguous, while a short discussion or comparison could likely clarify these open questions.

Generally, the submitted manuscript contains some weaknesses in language and structure. While it should be revised for grammar and wording, I would also recommend reducing the section about the tectonic setting. A more focused introduction leading to the main open questions discussed in this paper would be sufficient. The paper might benefit generally from a clearer structure.

SPECIFIC COMMENTS

As the method is based on an azimuthal variation of the splitting parameters, a more detailed discussion should be included, clarifying, why a multiple layered anisotropy can be neglected as cause for the observed azimuthal distribution. It is not sufficient to base the single layer assumption on the spatially coherent anisotropy (page 1/line 10-11, page 3/line 8). Multiple layers with individually spatial coherent anisotropy will also produce spatially coherent distribution for the measured splitting (of course with azimuthal variation at each individual station). This question remains with respect to the limited azimuthal coverage, which appears sparser in figure 4 then described in the paper (page 8/line 5). A 90° periodicity, as expected for layered anisotropy, cannot be excluded. Therefore, I would suggest showing a fit of a two-layer anisotropy at exemplary stations to allow a meaningful discussion.

The favoured Couette-Poiseuille flow in abstract and conclusion seems contradicting to the main assumption of a single layer anisotropy resulting in an azimuthal variation of splitting parameters solely caused by the non-vertical incidence of S(K)KS-phases. A Couette-Poiseuille flow would produce a depth dependent fast axis direction resulting in azimuthal variation of phi and dt (with 90° periodicity). I agree with the first conclusion made in this paper, that the data is best explained with a Poiseuille flow model.

In the following I will list some minor suggestions for improvement

- Further topics to be discussed or mentioned -

Page 2/Line 2: LPO of olivine is not the only origin for anisotropy (Savage 1999).

Page 2/Line 6: What is about other depth regions and their contribution to anisotropy?

Page 2/Line 18-19: The elliptical motion is also depending on the frequency content of the core-mantle converted phase (see Rümpker & Silver 1998)

Page 2/Line 24: This is not generally true, as there are also other effects producing anisotropy e.g. alignment of cracks in the crust or alternating layers of different seismic velocities. (Savage 1999)

Page 3/Line 11: It would be helpful at this point to state the contradicting arguments, and also pick up on them again in the discussions section to show how the understand-

ing of the geodynamic processes is improved by this paper (and what arguments are ruled out by this paper).

Page 6/Line 1: This section summarizes the theory found in Löberich & Bokelmann 2020. Nevertheless, it would be important to also point out the limitations and the assumptions the theory is based on (e.g. the symmetry system).

Page 8/Line 8: Does the number of measurements at one station have a large impact on the results or the observables dphi and ddt?

Page 10/Line 26-29: The southern subarea is also characterized by less data coverage. Might that also be a reason for the different observation compared to the northern subarea?

Page 13/ Line 12-13: That the data supports a high-temperature mechanism is an important conclusion here, as it is stated. While it is mentioned, that geological observations favour a different mechanism, are there geological observations in the area, that support this mechanism?

- Suggestions for reduction -

Page 2/Line 12-13: the comparison with optical anisotropy seems unnecessary at this point, as it is not used further to explain the properties of seismic anisotropy.

Page 4/Line 1: This section might be too long and detailed, as this paper aims solely to differentiate between an asthenospheric and lithospheric origin for the measured anisotropy.

Page 12/Line 12: This statement seems not to fit to the context of the publication.

Page 12/Line 23-24: It is not clear how an extended period of the AlpArray experiment is connected to the current paper.

Page 13/Line 1-6: The data set has been strongly simplified by taking the mean (and median) for the intervals around the extrema in dphi, which also coincide with ddt expected to be zero. With this simplification it is only natural not to see any further complexities, necessary to be fitted by a more complex model. With this I don't think it is necessary to point out what complexities are not considered for the modeling, as it gives no additional information for the conclusion of this paper.

TECHNICAL CORRECTIONS

Page 2/Line 1: More generally the waves are affected by the medium they propagate through (not necessarily layers.

Page2/Line 8: effect=affect

Page 2/Line 18: "show up" = appears

Page 2/Line 19: isotropic Earth = isotropic medium

Page 2/Line 20: "shows a signal" – The measured time-series on the transverse component is not an independent signal. I would rather refer to the energy of the signal that occurs on the transverse component due to the splitting.

Page 2/Line 26: "So" seems not the right word here, as resolving the foliation seems not to be related to the weak depth resolution.

Page 4/Line 14: "e.g." seems to be unnecessary here

Page 4/Line 18: "till" = until

Page 6/Line 4: "SKS" and SKKS or S(K)KS

Page 7/Line 5: "individuals"=individual

Page 8/Line 9-10: This does not seem to be a "correction". It would be better to simply describe what is done: First the mean values are subtracted from the individual values to obtain ddt and dphi. Subsequently the backazimuth is reduced by the mean of the fast axis to shift all measurements to the same reference.

Page 8/Line 25: A "verification" might not be possible, but it might proof it as a most

probable model, which is already very promising.

Page 10/Line 21-24: This description is slightly confusing. The statements regarding the fast axis directions don't fit in the general structure of the sentence.

Page 10/ Line 26: "transition" might be the wrong word to describe a drop of magnitude in a histogram.

Page 13/Line 31: "shear strain does too"

Page 14/Line 31: "Now we return"

Page 14/Line 35+ Page 17/Line 24: "render" – seems to be the wrong word?

Page 17/Line 4: derived=measured; angular=azimuthal

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