

Solid Earth Discuss., community comment CC1  
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## Comment on se-2021-95

Jean-Baptiste Koehl

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Community comment on "Variscan structures and their control on latest to post-Variscan basin architecture: insights from the westernmost Bohemian Massif and southeastern Germany" by Hamed Fazlikhani et al., Solid Earth Discuss., <https://doi.org/10.5194/se-2021-95-CC1>, 2021

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Thank you very much for this very interesting manuscript presenting engaging seismic interpretations and high quality figures that will, no doubt, pick the interest of specialists of the Variscan Orogeny and of specialists of the seismic expression of shear zones. The correlation of Variscan units across Europe has been an outstanding issue that has captivated many over the years and the new interpretation and new seismic data presented in the manuscript are a great addition to the existing literature on the topic. In addition, recent and ongoing work by the lead author on the seismic expression of shear zones has already received praises by peers and the present manuscript will also benefit future seismic interpreters. The manuscript is well organized and written in a readable manner making it easy for the reader to follow the authors on their scientific journey.

Here are a few general comments that may help further improve this great piece of work in addition to the minor comments attached as supplements.

Comment 1: Sub-chapter 3.4 about seismic facies belongs to the interpretation. Its relative place in the manuscript is fine, but the title of chapter 4 suggests that interpretations/results belong to chapter 4 (e.g., interpretation of seismic facies BSF2 as a shear zone should be part of the results). A possible change would be to move most of section 3.4 at the beginning of chapter 4 and to leave in chapter 3 (methods) only a brief account of the method itself: the identification and description of three seismic facies that will be described in chapter 4. Possible re-organization: 4.1 sub-chapter: Description and interpretation of seismic facies (including parts 4.1.1, 4.1.2 and 4.1.3); 4.2 sub-chapter: Interpretation of seismic profiles (include 4.2.1, 4.2.2, etc. for each interpreted profile).

Comment 2: Mention is made of existing literature that refer to deformation phases D1, D2 and D3 in the introduction/geological setting, which is fine. However, recent work has shown that so-called "deformation phases" should be used with caution (e.g., Fossen, 2020 – [doi.org/10.1590/2317-4889201920190109](https://doi.org/10.1590/2317-4889201920190109)). Could some of the antiformal structures discussed lines 459–464 have formed during D1 (see also comment in the attached supplement)? If not, are there any age constraints in the existing literature that can firmly reject such interpretation?

Comment 3: Sub-chapter 5.1 of the discussion missed on the opportunity to discuss

several items of interest highlighted by the authors (in the results chapter) and by the new seismic dataset.

- Lines 370–372: The lower branches of the shear zones (those incising into Saxothuringian basement rocks – e.g., Figure 6–8 and perhaps 9) could have been discussed further together with their implications on shear zone geometry, kinematics, and tectonic/reactivation history. A couple of sentences on their interpretation and implications (with references) could suffice.
- Lines 391–392: The segmentation of the reflections interpreted as Variscan shear zones is not discussed sufficiently. What do you think such segmentation reflects? Could it be related to the seismic survey (angle of acquisition with respect to the shear zone trend/geometry? Other?) or is it a primary feature? And if a primary feature, what types of small scale structures could the shear zones consist of in these places (e.g., duplex structures, antiformal stacks)? And what are the implications of these small-scale structures for the geometry, kinematics and/or reactivation history of the Variscan shear zones? A couple of/a few sentences could suffice.
- Line 407–408: The decreasing reflectivity of the shear zone in profile 1804 is not discussed further. Could it be related to the nature of the overburden (reflectivity more similar to that of the shear zone?) or is it because the shear zone is less intensely deformed/less developed in this area? What are potential implications for the shear zone geometry, kinematics and/or reactivation history. One more sentence could be enough.

Comment 4: Figure 1 could show the extent of the shear zone structures mapped in the present study.

Comment 5: The resolution of figures 6–8 could be improved to the same level as that of figures 5 and 9.

Please also note the supplement to this comment:

<https://se.copernicus.org/preprints/se-2021-95/se-2021-95-CC1-supplement.pdf>