

Interactive comment on “Lithosphere tearing along STEP faults and synkinematic formation of Iherzolite and wehrlite in the shallow subcontinental mantle” by Károly Hidas et al.

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This was a well-written and scientifically sound manuscript describing the interaction between deformation and melt along STEP faults. The methods described seem appropriate and the interpretations well-reasoned. I have some comments and suggestions for the authors to consider. That being said, the issues I bring up below are relatively minor and I recommend this paper for acceptance.

Section 5.2: I would have liked to see more of a discussion of the difference in temperatures calculated using the two geothermometers. Is there a geological explanation for this? Should we believe one method over another?

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Section 6.1: (page 11, lines 28-30): I was a bit confused by the terminology of “second phase particles,” but maybe I’m just unfamiliar with this term. Does it always refer to the phase doing the “pinning”? (Also, there is a misplaced comma in line 29 after the hyphen)

(page 12, lines 8-9): Something is odd about this sentence. As is, it doesn’t make sense grammatically, particularly in this part: “relatively cold and then stiffer and high stress...”

Section 6.1.2 (page 13, lines 24-27): This is a more general comment, but is there a reason why the authors chose not to estimate stress magnitude from recrystallized or subgrain size? It seemed odd to me that this was missing, especially because all the authors would have to do is use the already obtained EBSD maps to calculate the average grain size, apply a correction factor, and use a paleopiezometer (e.g., Van der Wal et al. 1993). The authors note how the fine grain sizes of some samples are consistent with high-stress deformation, but don’t put a number on this. People might be interested in this value both from a regional deformation perspective but also to address whether or not higher stresses are associated with their [100]-fiber CPO samples vs. orthorhombic CPOs, as is commonly assumed (e.g., Karato et al. 2008, review).

(page 14, lines 16-17): Was there any microstructural evidence (other than weakened CPO) for a switch to diffusion-assisted grain boundary sliding? As an example, were there any 4-grain junctions?

Figure 3: You should specify whether the maps shown are the entire EBSD area mapped for each sample or just a portion of it. If so, it doesn’t appear that there would be enough opx or cpx grains in 3B for a reliable bulk CPO.

Figure 9: It is hard to read the yellow text, even with the black outlines.

Figure 10: It is hard to see the tiny volcanoes on the surface. Perhaps just make

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them one color (and bigger). It is also difficult to read the text associated with those volcanoes (in red and blue). It might help to have a white box behind each of them.

Overall, again, this was an excellent paper and I hope these comments/suggestions prove to be useful to the authors.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-32>, 2019.