

Interactive comment on “Deformation mechanisms in mafic amphibolites and granulites: record from the Semail metamorphic sole during subduction infancy” by Mathieu Soret et al.

Vasileios Chatzaras (Referee)

vasileios.chatzaras@sydney.edu.au

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Dear Authors and Editor,

This manuscript presents a microstructural study of the deformation mechanisms in the basic amphibolites of the metamorphic sole beneath the Oman peridotite. The microstructures were analysed with Electron Backscatter Diffraction (EBSD), and mineral composition was estimated and mapped with Electron Probe MicroAnalysis (EPMA).

The aim of the manuscript is clear. Study the deformation history of the amphibole-bearing rocks of the metamorphic sole, to understand how crustal material of the sub-

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ducting plate is being accreted to the base of the overlying mantle wedge. The Authors analysed a suite of six samples from three different locations of the Semail ophiolite. The samples were collected from the HTa and HTb sub-units defined by Soret et al. (2017), and from distances up to 40 m beneath the thrust fault contact with the overlying peridotites. The Authors described and quantified a range of microstructural parameters (e.g., mineral assemblage, microstructural configuration, grain size, shape preferred orientation, grain shape aspect ratio, and crystallographic preferred orientation) and amphibole composition, to understand how they vary with distance from the peridotites, and thus, degree of retrogression. The combination of the microstructural and chemical data led to the interpretation that a range of mechanisms accommodated deformation during the formation of the metamorphic sole. The Authors provide a conceptual model of how the variations in the strength of the basic amphibolites and the overlying peridotites may control the mechanical coupling between the two plates and the accretion of the crustal material at the base of the mantle wedge.

This is a very interesting manuscript, on a topic that is timely and of broad interest, given the long-standing interest of the Earth Sciences community in subduction processes. However, I feel that: 1) some of the interpretations regarding the dominant deformation mechanisms are not fully supported by the data, 2) the manuscript could benefit from a more reader-friendly way of presentation of the results, and 3) this manuscript has significant overlap with the illustrations published in Soret et al. (2017), which reduces the originality of this research article.

The data presented in this work should be published in a revised manuscript, in which the Authors could consider addressing the following points.

Specific comments:

1) Figure 1a,e,f, Figure 2 (entire), and Figure 5b,e,f,g have already been published in Soret et al. (2017). I justify the repetition of Figure 1 because it is the intro Figure and summarizes existing knowledge, and Figure 2d because it is used as a base-image for

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showing the locations of the EBSD maps, however, I find the repetition of the rest field photographs and photomicrographs included in Figures 2 and 5, not “flattering” for an original article. Moreover, I am wondering why the photomicrographs in Fig. 2d, and Fig. 5e,g have been reversed compared to how they have been previously published (I am comparing with Fig. 5a,d,f in Soret et al., 2017).

2) Following on from (1) above. In page 3, lines 24-29 and Figure 1e, the Authors present data for the Ti-content of amphibole in the metamorphic sole of the Semail ophiolite. However, Figure 1e, which corresponds to Figure 7k of Soret et al. (2017), only includes data from the exposure of the metamorphic sole in Sumeini. Instead, the Authors could compile the data included in Figures 7j, k, l in Soret et al. (2017) to make a new plot that summarizes the Ti-content of amphibole from the three studied areas of the Semail ophiolite.

3) I would strongly recommend that the Authors include in the manuscript EBSD maps and/or CPO data from all the samples that they analysed from the three studied locations of the metamorphic sole. The number of samples (six in total) that they have analysed with the EBSD technique, allows them to do so. Particularly, data from the only sample from the HTb zone, which was collected from Khubakhib, should be included in the manuscript rather than the supplementary information. Also, it is not clear to me what complexities the Authors try to overcome by presenting in the current version of the manuscript only data from the Sumeini area (Page 4, Lines 14-16).

4) I found it difficult to follow how the different microstructural parameters change with distance beneath the contact with the peridotites. My suggestion would be to make a new figure (modified version of Fig. 9c) that will include in the vertical axis the distance from the peridotites, and in the horizontal axis the range of the microstructural parameters (J-index, M-index, grain size, axial ratio, deviation of the mean orientation of the grain long axis from the foliation -SPO-, etc.) that were quantified for each mineral. Data from all six samples should be included in this plot.

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5) The manuscript would benefit from the addition of detailed geological and structural maps of the three sampling areas. The Authors could plot on each map the rock fabric (i.e. foliation and lineation) for each of the analysed samples. Importantly, these maps would allow them to describe the sense of shear in the geographic reference framework, rather than the thin-section framework as they currently do.

6) Some additional information could be included in the Methods section. A description of the method (e.g., linear intercept, equivalent circular diameter) that was used to estimate grain size should be included. Moreover, the Authors need to explain which mean (arithmetic or geometric) of the grain size distribution they report in Table 1. How were the thin sections, and thus the EBSD maps, produced relative to the rock fabric (i.e. foliation and lineation)? This information should be added in all the captions of Figures with either EBSD maps or photomicrographs. Also, the lower hemisphere projections of CPO data are not in the typical plotting framework with vertical foliation and horizontal lineation. Could the Authors comment why is that? The plots could be rotated so as the CPO to be reported into the usual plotting framework.

7) The preferred interpretation for the concentration of low-angle misorientation axes around the [001] crystallographic axis in amphibole, is micro-fracturing along the [001] axis combined with small rigid rotations (Page 12, Lines 10-12). In this case, there should be a spatial correlation between subgrain boundaries and intragrain microfractures, e.g., the healed microfractures shown in Fig. 5h. Although such correlation is implied in Fig. 6c,h, the photomicrograph and the EBSD map are too small to observe such features. The Authors could provide some more detailed EBSD maps that would support their preferred interpretation. It would also be interesting to explore what is the orientation of the misorientation axes associated with the low-angle boundaries in the new EBSD maps, as well as in Figures 6e,h,i. An alternative interpretation for the observed distributions of the misorientation axes, could be the formation of low-angle boundaries with both tilt and twist components, due to operation of more than one slip systems (e.g., Díaz Aspiroz et al., 2007 also cited in the manuscript). Is there any-

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thing to rule out this interpretation? If not, and considering the strong concentrations of low-angle misorientation axes subparallel to the [001] axis in the samples from the HTa sub-unit, what would be the implications on the preferred interpretation that dislocation creep does not contribute significantly to the high temperature deformation of amphibole?

8) Following on from (7) above. The Authors attribute the patchy distribution of the Ti-high areas and the crystallization of Ti-medium/low amphibole to dissolution-precipitation creep. One of the main arguments is the spatial correlation between microfractures and Ti-medium/low areas. Microfracturing enhances permeability, fluid circulation and retrograde hydration, promoting dissolution-precipitation creep. Although this is a valid interpretation, the Authors should discuss and exclude alternative interpretations. Specifically, I am thinking the possibility that low-angle boundaries (potentially not associated with microfracturing) have acted as elemental pathways that enhanced in-grain element diffusion (e.g., Chapman et al., 2019). The manuscript would benefit from a more detailed characterization of the spatial relationships between Ti-medium/low areas and subgrain boundaries, which is relevant to comment 7, above, regarding the relationship between microfractures and low-angle boundaries.

9) Unless I missed something, I did not find in the supplementary information any figure captions or text describing the data from the samples in Khubakhib and Wadi Tayin areas. The Authors provide only figures. At a minimum, figure captions should be added.

More specific comments and technical corrections (on text and figures) are provided on an annotated version of the manuscript.

Papers cited:

Chapman, T. , Clarke, G. L., Piaolo, S. , Robbins, V. A. and Trimby, P. W. (2019), Grain-scale dependency of metamorphic reaction on crystal plastic strain. *J Metamorph Geol.* Accepted Author Manuscript. doi:10.1111/jmg.12473

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Best wishes, Vasilis Chatzaras

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2019-28/se-2019-28-RC1-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-28>, 2019.

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