Dear Florian,

Thank you for your work on the manuscript and your fast processing. We fundamentally agree with all the points you have raised in your letter:

As the first review was very constructive a second opinion of Andrea would have been optimal for us, but even so she has her improving share in the manuscript.

In this second revision we focused on a clear separation of facts and hypotheses. On a structural level we divided more speculative parts of the discussion into separate sections (timing of the metasomatism (5.2.2) and the comparison to other upper mantle shear zones (5.5)). Additionally, we clearly indicated the limitations of this research in terms of geochemical data base and sampling density around the tectonite-mylonite transition whenever necessary. Finally, the complete discussion including the headings was rewritten to emphasize the main points and strengthen the argumentation. Furthermore, three research questions were formulated in the introduction which are answered by the trimmed bullet point conclusion.

Additional geochemical analysis would be desirable. The focus of this additional analysis, however, should lie on the above-mentioned tectonite-mylonite transition which would require another sampling campaign. Even though we fully agree with J. Précigout, this is unfortunately no longer feasible within the scope and possibilities of this project. However, in the more speculative part of the discussion, we want to point out the potential implications of the geochemical trends and overprinting relationships that may inspire future work exactly on this transition.

Regarding J. Précigout's second review, we had of course hoped that the corrective work done on the basis of Andreas and his review would also be constructive in his opinion. As one of his main points of criticism is the potential impact of melt on deformation, we propose a change in title to

"On the interaction of melt, phase mixing and deformation in a large-scale mantle shear zone (Ronda peridotite, Spain)"

which leaves more room for discussion. If you think that title is more convenient for the manuscript, the authors would agree on a change. The controversial parts J. Précigout addresses were moved to the more hypothetical sections of the discussion. The comparison of different upper mantle shear zones across different P-T conditions, Ronda included, indicates a significant role of reactions for the localization of strain in the upper mantle. This section does not aim on a proof for melt-enhanced deformation in Ronda. All his minor changes were edited (see below).

On behalf of the authors,
Sören

Minor comments/concerns by Jacques Précigout

Line 34 : delete « the » shear zones

Done.

Line 49: Deformation-induced

Done.

Line 53: Reaction-induced

Done.

Line 69-70: Sorry but this sentence is wrong: we never proposed that stress increased where strain is localized. On the contrary, our model involves that strain localizes because of a stress drop due to grain size reduction. The difference of stress you see is related to the lithosphere strength profile.

Thanks for the remark. We changed the sentence accordingly (Line 65-66).

Line 70: What does « mixed peridotites » means ? I think you are mentioning « mixed phases », but please, rephrase.

Paragraph was rephrased (Line 67-71).

Line 202: ODF means Orientation Distribution function, not O. Density F.

Corrected.

Line 200-215: By the way, there is something wrong there, because no one of your figure shows an ODF; there are only pole figures with or without texture, depending on the amount of data point. In your case, the ODF is only used to calculate the Jindex. Please, correct this paragraph accordingly.

Sentence was rephrased.

Line 203: The Mindex is not based on the ODF, but on the relative distribution of uncorrelated misorientation angles between the measured and theoretical ones (Skemer et al., 2005).

Sentence was changed accordingly.

Line 208: The color scale of pore figures refer to « multiple of UNIFORM distribution », not « random distribution ».

The color scale used in MTEX is based on the multiple of random distribution. At least as "mrd" function in MTEX itself and in the associated work flows (e.g. https://mtex-toolbox.github.io/ODFTutorial.html).

Line 210: Please, expand on how you resolved the GND. Maybe you did it this way, but I don't think you have implemented the GND in MTEX directly from the paper of Pantleon (2008). I suppose instead you have used the functions « fitDislocationSystems », which has been implemented by the MTEX team. If I am correct, please modify your text accordingly.

Sentence was changed.

Line 253: the same here: it seems that you confound ODF with pole figure. Please, clarify.

Corrected. ODF was changed to pole figures.

Figure 3: If you excludes the olivine-rich matrix from grain size calculations, which represents a significant part of the peridotites, how do you know that grain size does not reduce with increasing strain ? And what about the pinning effect on grain size ?

To clarify, the dominant microstructure of all samples is the mixed matrix (Fig.2). In the mylonitic part, ol-rich matrix areas are only forming lenses in the finer-grained mixed matrix. It must be pointed out again, that in previous studies, ol-rich and mixed matrix have not been differentiated. As described in the text, ol-rich matrix domains are in fact rather limited in size and distribution and also "affected" by interstitial pyroxenes but just in a smaller amount than the mixed matrix. Phase mixing, smaller grain sizes, pinning by secondary phases and microstructures implying strong deformation are dominant in the mixed matrix. That is why we focus on the mixed matrix rather than on the olivine grain size of potentially annealed ol-rich matrix lenses. If the ol-rich matrix were of major importance for deformation one would not expect it to form competent lenses and rather interconnected layers like the mixed matrix (Fig. 2). The analysis of the ol-rich lenses could of course give grain size data (the data obtained indicates similar or bigger grain sizes than the mixed matrix), perhaps even smaller grain sizes towards the NW but as it is isolated in the mixed matrix grain size reduction or pinning in the ol-rich lenses does not seem decisive for the overall deformation. We did not exclude Ol-rich matrix domains from the start of this study, in fact it is the small abundance and it's strong serpentinization which hinders the analysis. Fortunately, this domain seems not to be decisive for the phase mixing and its influence on deformation. As the focus of this research lies on these aspects it is, from the point of the authors, legitimate and scientifically justifiable to exclude this domain.

Figure 3: Furthermore, plotting the grain size data for all phases in one graph is difficult to read. Please, separate them, at least for grain size. To me, it seems that there is a slight reduction of the mean size in any case .

Grain size data for all major phases was plotted separately.

Line 311-312. Only giving one axis with respect to the foliation plane is not enough. You also have to say that [010] is normal to the foliation.

[001] was given as it is indicative for deformation induced CPO for opx. [100] was added for clarification.

Line 325: What is your percentage threshold in terms of secondary phases to say that you are dealing with a olivine-dominated matrix or a mixed matrix? Even for mixed matrix samples, most of the ones you described have olivine as the far dominant phase (up to 80%). Please, clarify.

Ol-rich matrix is characterized by >90% olivine. The mixed matrix is defined by less than 90 % olivine and a secondary phase content dominated by interstitial secondary grains (pyroxenes). The latter characteristic is the more important one, as it points to the formation by melt infiltration. Thanks for the remark, this definition was added to the matrix section.

Line 337: The phase abundance you describe does change with the strain gradient, so how melt can enhance strain localization?

> *Based on the microstructural results, the melt was apparently present in the entire shear zone. It rheologically affects therefore the entire melt affected area = all analyzed samples. The word "enhance" is probably wrong. The fact, that melt presence facilitates deformation does not have to be discussed here. Our point is that melt is present in the entire shear zone together with deformation indicative microstructures. Based on all obtained data and compared to Beni Boussera, it is harder to think these two points apart from each other, melt and deformation, than together. Renaming of the manuscript is considered together with the editor.*

Line 338: Coarse-grained olivines

> *Changed.*

Line 359: (mrd, M) ??? Please, give the values.

> *The sentence was rephrased. M and mrd values are mostly similar for both pyroxenes for a given microstructure. Values are given above and in the appendix.*

Line 407-410: in line 407, you say that grain size is constant through the whole transect, but in line 410, you mention that « in contrast to grain size, AR remain constant… ». Please, clarify.

> *Grain sizes do show an excursion towards bigger sizes around 250 m distance to the NW-B. This excursion is not present for AR. Sentence was rephrased.*

Figure 11: In terms of chemistry, no data has been added to confirm the chemical gradient. This is yet a major point of this paper.

> *We totally agree that this is a very interesting point. However, for a solid database more EPMA measurements would be necessary, as the reviewer pointed out in his first review. A solid evaluation of the trend in Mg# is not possible with the samples collected for this study. In this manuscript, the focus lies on the mylonites, their microstructures and genesis. Fortunately, the research has led us to these geochemical findings which, however, can only be investigated sufficiently by collecting additional samples from the tectonite-mylonite transition. This and the resulting geochemical focus are clearly beyond the scope of this manuscript.*

Line 684: You claim that grain size is different between tectonite and mylonite, but you say the opposite in the abstract. Please, clarify.

> *"The constant grain sizes with local variations independent on the distance the deformational centre indicate a broad scale deformation with nearly constant stresses in the entire mylonitic area." (now:"* Instead, extensive phase mixing under near steady-state conditions is documented by the constant grain size and by phase boundary percentages > 60% for the entire mylonitic unit and all the microstructural domains.") *The abstract is talking about the mylonitic part, not about a contrast between tectonites and mylonites. In the abstract, only the change in grain shape between tectonites and mylonites is adressed.*

Line 717: You say that there is a strong CPO along the transect, but in Précigout and Hirth (2014), we documented a decreasing CPO with increasing strain in the top mylonite. And this CPO get close to complete randomization nearby the shear zone boundary.

*A discussion on the "*Strain localization in the northwestern mylonites" was added in section 5.2.3.

Line 720: A-type is not dominant in the top mylonite, where AG-type and B-type mostly occur (Précigout and Hirth, 2014).

A differentiation between top mylonites and mylonites is added. However, in the matrix we only found one B-type CPO. For the B-type genesis please see comment above, section on neoblast tails and CPO data.

Line 734: Grain size reduction is a matter of neoblast amount, OK, but still, there is a grain size reduction with a possible rheological effect.

Yes. A sentence was added in line 774-779 and the effect of smaller grain sizes by increased neoblast abundance is discussed in section 5.2.3.

Line 745: De Ronde and Stünitz is not relevant here, because there is no plagioclase, nor other phase transition across the ronda shear zone (Spl stability field).

We are referring to the mechanism of grains being transported from the reaction interface during deformation. The type of reaction should not be important for this process.

Line 768: Yes, Tommasi is able to localize where melts are localized, which is apparently not the case here, based on what you show.

The discussion was rearranged to get the main points clearer. The shear zone is where melt was present. This melt crystallized apparently under deformation as visible by the grain shape. Therefore, its presence apparently shaped the shear zone. We do not claim that it was there during the final stages of deformation, but it definitely formed the peridotites in regard to its mixed and reacted microstructure we examined in this manuscript.

Line 793: What do you mean by « shaped the shear zone »?

Please see the comment above.