Dear Editors, dear Andréa Tommasi, dear Jacques Précigout,

First of all, the authors want to thank both reviewers, Andréa Tommasi and Jacques Précigout, for their detailed and constructive comments, which help to improve the manuscript. The authors decided to reply in one statement because the major remarks of both referees largely overlap or complement each other. In general, all proposed changes and comments of both reviewers were considered in the revised version of the manuscript. The changes tracked by line number are in the second part of this reply. However, at first we will briefly comment on the main points of criticism.

According to both reviewers, the three main points of criticism are:

(1) The nature of metasomatism: Refertilizing or late stage, fluid-rich melt?
(2) The timing of the metasomatism and its effect on deformation.
(3) The insufficient quantification of the olivine grain size.

The nature of metasomatism:

We agree with both reviewers, that the irregular grain/phase boundaries and grain shapes as well as extensive phase mixing are robust microstructural evidence for metasomatism in the entire NW Ronda shear zone. A metasomatism of parts of the investigated mylonite unit by refertilizing melts was postulated by Soustelle et al. (2009). This process was adapted in the original manuscript for our transect. However, both reviewers reject the interpretation of a refertilizing melt because of low syn-kinematic temperature estimates (800–900 °C, 1.95–2.00 GPa (Garrido et al. (2011)) and the missing modal change to increased fertile components. The authors agree with both objections and discuss the nature of the metasomatism in the reviewed version by taking into account the suggestions made by the reviewers. Following the annotations made by Andréa Tommasi, the microstructural similarity as well as the matching PT-estimations for the grt/spl-mylonites from Rondas counterpart from the Morrocan limb of the Gibraltar arc, the Beni Bousera peridotite massif, point to a consistent genesis. Frets et al. (2012, 2014) suggested a metasomatism of small fractions of fluids or highly evolved melts, which did not reset the quilibrium temperatures in Beni Bousera. Matching all observations made in our samples, the authors agree, that the metasomatism is most likely attributable to highly evolved melt. A fluid-driven metasmomatism as proposed for the plagioclase-tectonite unit in Ronda by Hidas et al. (2016) is in the authors opinion less likely because of the low abundance of amphibole in the dominant mixed matrix and the absence of ultramylonites, which were obsereved to form by fluid-rock reactions. Based on these observations, the rewritten section 5.1 “Microstructural implications – Formation” now includes a discussion and evaluation of the different potential metasomatic agents. In this regard, the authors agree with Jacques Précigouts remark of the small geochemical data base for a geochemically based model of the shear zone’s evolution. To resolve the geochemical trend in detail, an additional study would be needed with the focus on the transitional area between the mylonite and tectonite unit. According to Jacques Précigouts suggestions, the reviewed discussion was focused on the microstructural evidence.

The timing of the metasomatism and its effect on deformation:

The rewritten section 5.2 “Microstructural implications – Deformation” now discusses the timing of the metasomatic event and its effect on the deformation. Microstructural similarities especially of the film/wedge-shaped orthopyroxenes in the mylonitic part of the shear zone to mylonites and ultramylonites investigated by Dijkstra et al. (2002) and Hidas et al. (2016) indicate a syn-kinematic metasomatism with dissolution-precipitation reactions being active. This assumption is supported by Frets et al. (2014), who argued for the corresponding grt/spl-mylonites of Beni Bousera for syn- to late kinematic metasomatism. As both reviewers criticize an overinterpretation of the data in terms of the importance of the metasomatic event for the genesis of the NW Ronda shear zone, the discussion was fundamentally shortened in this regard. Therefore, the main focus of section 5.2 lies now on the active deformation mechanisms (dislocation creep, dissolution-precipitation creep), the dominant deformation mechanism (dislocation creep) and the potential impact of phase mixing and melt presence on the deformation. The authors agree that an irrevocable argument for the trigger of the shear zone by metasomatic processes cannot be given. However, the comparison with other upper mantle shear zones (section 5.4) indicates a general strong relation between reactions and localized deformation in the upper mantle. With the data presented, the NW Ronda shear zone lines up or at least does not contradict this picture.

The insufficient quantification of the olivine grain size:

The complete data was reprocessed to quantify the original olivine grain size using the method suggested by Andréa Tommasi. The new data were added to the microstructural data of figure 3 and of supplementary data S2. However, even with a larger spread and coarser grain sizes, olivine follows the general trend of constant grain sizes in the entire mylonite unit formerly reported by Johanesen & Platt (2015). Moreover, Frets et al. (2014) report for the Grt/Spl-mylonite unit of Beni Bousera a similar range of mean olivine grain size (90-160 µm). The statistics of 7375 olivine grains analyzed in the mixed matrix and the consistency with the published data indicate a robust data set of constant olivine grain size over the entire mylonite unit with local variations but no obvious trend.

Detailed list of corrections for remarks of Jacques Précigout sorted by line numbers. In the authors answers the first line number refers to reviewed manuscript without changes marked, second line number to the version with changes marked.

Jacques Précigout:

Additional comments:

1. Better synthesizing the micro-structural features, documentations of which are a bit unbalanced with respect to chemical features.

    The complete discussion was rewritten with the focus on the microstructural implications. The authors agree, that for solid geochemical interpretation additional measurements are necessary on samples that are not present at the moment. The focus of the geochemical investigation should be on the transition between mylonites and tectonites.

2. Strengthening the point of Mg# gradient by performing chemical analyses on more samples (this would help to better document the melt-rock interactions front).

    The authors decided to focus with this manuscript on the microstructural analysis of the mylonite unit. However, agreeing with the remark a future study in the geochemistry is planned.

3. Reconsidering the main axis/interpretation of the paper by focusing on the evidence of melt-rock interactions front across the shear zone, and not speculating - although it could be discussed - on the role of melt in triggering strain localization in Ronda (based on the data presented here, it is not plausible)

    In accordance with the answer on the main points of criticism and the comments above the discussion was rewritten with the focus on the microstructural implications for the formation of and deformation in the mylonitic unit.

Minor comments to the authors

Geological setting: You could be interested in having a look at the two papers of Bessière et al. (2021) that expand on the geodynamic of the Ronda peridotite.

Thanks for the interesting suggestion.

Line 142: underlying not underlaying.

Changed (l. 143/ 160)

Line 159: Citing a paper rather than the PhD thesis of Dirk Van der Wal would be more appropriate here. And I think some other hypothesis (and references) need to be mentioned, including the one described in our paper (Précigout et al., 2013).

The authors decided to delete the emplacement hypothesis from the introduction as the focus lays on the microstructures and not on the overall tectonics (l. 160/ 183). As the reviewer annotates correctly otherwise more hypotheses should be discussed.

The reference was changed to the most relevant paper.

Line 167: were, not was.

Changed (l. 168/ 192)

Line 202: 100 grains is not enough to calculate a J or Mindex (Skemer et al., 2005). You could also see our recent paper dealing with this feature (Précigout et al., 2022, sci. rep.)

A minimum of 150 grains was set for the M- and J-Index (l. 202/ 226).

Line 204: We commonly use 10° as halfwidth angle. Otherwise, it smoothes a lot the data.

Thanks for the remark. Using 15° as halfwidth was adapted from previous studies (Tholen et al 2022, Linckens et al 2021). For the next dataset we will compare 10° and 15°.

For this dataset, the CPOs are mostly strong if present and therefore the changes in the dominant olivine CPO for example are easily recognizable.

Line 223: Plane, not plain.

Changed, thanks (l. 229/ 257)

Line 242: This feature has been already described in Précigout et al. (2013), so it should be cited here.

Citation was included (l. 244/ 276).

Figure 4a: Where are the olivine dots? The olivine-rich matrix represents the major part of the peridotite, so you cannot exclude it from the grain size dataset, whatever the reason.

Now figure 3A. As both reviewers requested to include the olivine grain size, we recalculated all EBSD data to reconstruct the original grain size. The reconstruction was performed using the proposed method in MTEX (Matlab) by Andréa Tommasi.

Line 334: B-type fabric has been documented in Précigout et al. (2014), so it has to be mentioned here.

Citation was added (l. 354/ 403).

Figure 6: The number of grains (or datapoint) has to be shown by each pole figure.

Numbers of grains and color bars for ODFs were added to all orientation figures.

Line 387: what do you mean by « tend to be higher »? The AR is higher or not.

Changed (l. 411/ 460).

Figure 8A: Avoid writing labels up side down.

Now figure 9. Label was flipped.

Line 527: Discussing about processes of mantle refertilization, the paper of Le Roux et al. (2007) should be discussed, at least cited, somewhere.

Refertilization was discussed in section 5.1.1. The citation was added.

Line 539: what do you base on to say that this CPO is atypical ? Is there any reference that mention that.

Reference was added (l. 690/ 747). The dominant CPOs for opx in deformed mantle rocks commonly have [001] in the lineation.

Line 581: when you discuss a feature that is not described in your paper, you have to cite the publication that describe it. For instance, boudinaging of pyroxenite layers has been described in Précigout et al., 2013.

Very correct. Thanks for the annotation, citation was added (l. 628/ 684).

Line 652: Saying constant grain size is not correct here, but constant dynamically recrystallized grain size may be correct.

Section was rewritten (now 5.2)

Figure 12 and 13: to be frank, your model is very difficult to understand based on the figures. Could you please make them more clear?

Complete discussion was rewritten with the focus on the microstructural implications. The model was dismissed.

Line 701: what trend?

See comment above.