RESPONSE TO DETAILED COMMENTS OF REFEREE #3 (Anonymous)

Referee's comment on old line 40: *the paper by Kruse and Stunitz (1999) should be cited here, too.*
Author's response and change in manuscript: We added the citation.

Referee's comment on old line 103: *omit one of the "alkali".*
Author's response and change in manuscript: We followed referee's suggestion.

Referee's comment on old lines 158-160: *how thick were the samples? This number enters the Beer-Lambert law.*
Author's response: The sample thickness was 500 µm.
Change in manuscript: We added this information in the revised manuscript.

Referee's comment on old line 191: *rephrase: "...estimate differential stress for plagioclase, we used...".*
Author's response and change in manuscript: We rephrased the text following referee's suggestion.

Referee's comment on old line 200: *"light" instead of "optical".*
Author's response and change in manuscript: We changed the text.

Referee's comment on old lines 223-225: *The statement of equilibrium boundaries is somewhat problematic as there are clear indications of disequilibrium in the rock, such as corona microstructures, symplectites, and reactions (see lines 230-235). This point should be discussed in the discussion section.*
Author's response and change in manuscript: We refer to microstructural rather than chemical equilibrium, although the two types of equilibrium may be related. The microstructures we report here are from the internal part of an olivine lens. Chemical reactions seem not to have influenced the olivine grains within the lens. Moreover, the microstructures related to chemical disequilibrium, such as symplectites and melt-rock interactions, postdate the main ductile deformation in the granulite xenoliths. For example, the observed melt patches and symplectites seem to be unaffected by deformation. Thus, we do not consider the statement of equilibrium microstructures as problematic.

Referee's comment on old line 225: *better: "reaction" instead of "interaction".*
Author's response and change in manuscript: We changed the text following referee's suggestion.

Referee's comment on old lines 250-253: *it is stated that An40-60 is measured for small grains, and then An65 is given as the lowest composition, which is clearly higher than An40-60. Please clarify.*
Author's response: We acknowledge an error in the reported An content of the recrystallized grains. The composition is An36 rather than An65.
Change in manuscript: We corrected and rephrased the text.

Referee's comment on old lines 269-271: *Please indicate the stability fields in figure 6. This applies to all the P,T-ranges of stability given for the assemblages (further below).*
Author's response and change in manuscript: We added the stability fields in the three plots of Figure 6.

Referee's comment on old lines 288-305: *The labels in Fig. 7 are different from what is referred to in the text. Please correct this. In addition, please give the rock types in the figure caption and in Table 2.*
Author's response and change in manuscript: We changed the text and the corresponding labels. Following referee's suggestion, we added the rock types in the caption of Figure 7 and in Table 2.

Referee's comment: *There is quite a range of H_2O contents in the plagioclase, even in what is referred to as "dry" samples. It is mentioned in the methods-section that these are maximum values, but the upper range of these values definitely does not represent "dry" material. So, the statement that plagioclase is dry in the granulites cannot be made as bluntly as it is done here. Please qualify the statement.*
Author's response: We interpreted the plagioclase grains in the mafic granulites as dry because: 1) the upper range of the reported H_2O content is expected to overestimate the actual H_2O content in the analyzed plagioclase grains; 2) compared with IR results of feldspars from other studies, we are dealing with relatively dry plagioclase; and 3) in support to our interpretation, we did not observe the existence of any hydrous phase in the studied mafic granulite xenoliths. However, we agree with the referee that some of the analyzed plagioclase grains with OH contents as high as 317 ppm (by wt) may have mechanical properties similar to those of wet plagioclase.
Change in manuscript: We rephrased the text acknowledging that the amount of water in plagioclase varies significantly and can locally be high.

Referee's comment on old lines 314-326: *It is unclear what the plots of much smaller sizes in figure S5 refer to. Are these recrystallized grains? Or are these plots enlargements of the low-size-end of the grain size plots? Their modes are much more frequent than the rest of the distribution. Please explain.*
Author's response: The plots are logarithmic grain size distributions.
Change in manuscript: We provided a clearer explanation for the plots in the revised figure caption.

Referee's comment on old lines 314-326: *Very important point: Why are mean sizes quoted and used in the text? It seems to be a meaningless number as the modes of these distribution are very prominent and clear. It seems more appropriate to use the mode of the grain size distribution, because this grain size fraction dominates the microstructure and therefore is expected to be representative for the stresses. This point is critical, because the modes will yield considerably higher stresses.*
Author's response: We acknowledge that the mode or the geometric mean of a grain size distribution may be the best measure of applied stress (e.g., Ranalli, 1984, JSG). However, the arithmetic mean of a recrystallized grain size distribution has traditionally been used in paleopiezometry to estimate the flow stress. In our study, we chose to use the arithmetic mean for consistency and comparison with other published results.

Referee's comment on old lines 349-363 and 386-395: *the pole figures seem to be somewhat over-interpreted. The maxima referred to do not lie in the lineation or normal to the foliation direction, and the CPO's are weak. So, at least, the inference of slip systems should be made with some qualifying statements and it should be pointed out that this is a very tentative interpretation.*
Author's response and change in manuscript: We rephrased the text avoiding any over-interpretation of the CPO plots, and added qualifying statements for the interpretation of slip systems.

Referee's comment on old line 361: *Ji et al. 2014 is not in the references.*
Author's response and change in manuscript: We included this paper in the references list.

Referee's comment on old line 369: *"as" instead of "to"*
Author's response and change in manuscript: We adopted referee's suggestion.

Referee's comment on old line 414: *"is consistent with" instead of "highlights"*
Author's response and change in manuscript: We changed the text following referee's suggestion.

Referee's comment on old lines 522-526: *The misorientation axes orientation consistent with tilt boundary orientations for known slip systems is probably the best evidence of crystal plastic deformation here and should be mentioned here.*
Author's response and change in manuscript: We included this criterion to the evidence of crystal plastic deformation.

Referee's comment on old lines 537-538: *it is difficult to rule out diffusion as an active mechanism.*
Referee's comment on old lines 539-544: *Here is again the question of equilibrium (see comment lines 223- 225). The curved boundaries point to diffusion processes to adjust grain boundaries - and this may suggest some diffusion relevant to the point of lines immediately above.*
Referee's comment on old lines 547: *Now, diffusion creep is inferred (see points immediately above).*
Referee's comment on old line 572-573: *...or diffusion creep*
Author's response and change in manuscript: We modified the text to make clear that diffusion creep is one of the contributing deformation mechanisms.

Referee's comment on old line 556: *Kruse and Stunitz 1999 should be cited here, too (also in line 574).*
Author's response and change in manuscript: We added the citation.

Referee's comment on old lines 638-640 and 642-643: *Yes, the rates are too low. Probably this is due to using a dry flow law for plagioclase. This can be revised by considering a wet flow law in addition, because the H_2O contents are not necessarily those of dry rocks (see above). The "dry" plagioclase experiments of Rybacky and Dresen (2000) were carried out with H_2O contents of 33 pm H_2O. This value is in the range of the low values determined here, but the upper range of H_2O contents found here is considerably higher and lies in the range of what would be considered "wet", at least for olivine and quartz. Please revise this point. In this whole discussion, there is also the point of grain sizes and stresses, which are important. It seems that the stresses are too low, because a mean value of the grain size is used instead of the mode of the distribution, and a dry flow law is used instead of a wet one (at least the wet flow law should be use in addition to give upper possible values). The values could change substantially (and thus the interpretation), if the true ranges of possible values are properly considered.*
Author's response: We acknowledge that a wet plagioclase flow law has to be considered for the mafic granulites, to account for the upper range of H_2O content in some plagioclase grains. We used the wet plagioclase flow laws of Rybacki and Dresen (2000), and Dimanov and Dresen (2005). The new strain rate and viscosity estimates are presented and discussed in the section "5.4 Viscosity structure of a strike-slip plate boundary". The main result from the use of the wet plagioclase flow laws is that the application of the Dimanov & Dresen (2005) flow law on the San Quintin mafic granulites gives considerably low viscosities (10^{15} – 10^{16} Pa s), while the Rybacki & Dresen (2000) flow law gives viscosities (10^{17}– 10^{19} Pa s), which are comparable (10^{17}– 10^{20} Pa s) to those estimated with the dry plagioclase flow law of Dimanov and Dresen (2005). These results do not change significantly the viscosity structure of the Baja California lithosphere, and therefore our interpretations. In the calculations mentioned above, we used the arithmetic mean rather than the mode of the grain size distributions, for the reasons mentioned in the reply to the comment on the old lines 314-326.
Change in manuscript: In the revised manuscript, we present and discuss the results of the wet plagioclase flow laws.

Referee's comment on old lines 669-675: *These interpretations, as some others of this paper, may change if wet conditions are assumed. It should at the very least be mentioned how the conditions change for higher H_2O contents.*
Author's response and change in manuscript: Following the reviewer's suggestion we described the strain rates and viscosities estimated using wet plagioclase flow law. Moreover, we have revised some of our interpretations.

Referee's comment on old ines 685-687: *Viscosities cannot "reverse". Please rephrase.*
Author's response and change in manuscript: We rephrased the text.