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Solid Earth
Discussions

Interactive comment on "Constraints on the rheology of lower crust in a strike-slip plate boundary: Evidence from the San Quintin xenoliths, Baja California, Mexico" *by* Thomas van der Werf et al.

Anonymous Referee #3

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The manuscript describes an extensive set of EBSD, EDS, and FTIR data, which is used to infer deformation mechanisms and strengths of the lower crust and upper mantle. The data is of good quality, the methods are employed with a very good knowledge and background, and many of the conclusions are supported by the data. The text is well written and properly organized. Some discrepancies exist and have to be clarified and revised before the manuscript can be published. These aspects are described in the section of detailed points below.

Detailed comments:

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line 40: the paper by Kruse and Stunitz (1999) should be cited here, too.

line 103: omit one of the "alkali"

lines 158-160: how thick were the samples? This number enters the Beer-Lambert law.

line 191: rephrase: "...estimate differential stress for plagioclase, we used..."

line 200: "light" instead of "optical"

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.

line 225: better: "reaction" instead of "interaction"

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.

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).

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.

There is quite a range of H2O 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.

lines 314-326: It is unclear what the plots of much smaller sizes in figure S5 refer to. Are

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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. 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.

lines 349-363 : 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.

line 361: Ji et al. 2014 is not in the references.

line 369: "as" instead of "to"

lines 386-395: see comment of lines 349-363

line 414: "is consistent with" instead of "highlights"

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.

lines 537-538: it is difficult to rule out diffusion as an active mechanism.

lines 539-544. Here is a 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.

line 547: Now, diffusion creep is inferred (see points immediately above)

line 556: Kruse and Stunitz 1999 should be cited here, too (also in line 574).

lines 572-573: ...or diffusion creep

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 H2O contents are not necessarily those of dry rocks (see above). The "dry" plagioclase experiments of Rybacky and Dresen (2000) were carried out with H2O contents of 33 pm H2O. This value is in the range of the low values determined here, but the upper range of H2O 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.

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 H2O contents.

lines 685-687: Viscosities cannot "reverse". Please rephrase.

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