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Solid Earth
Discussions

Interactive comment on "Structural and rheological evolution of the Laramide subduction channel in southern California" *by* Haoran Xia and John P. Platt

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The manuscript describes the tectonometamorphic history of the Pelona schist and infers tectonic conditions for the Cretaceous/early Tertiary subduction system in California. The manuscript touches on quite a few issues, i.e. deformation analysis, thermobarometry, fission track dating, exhumation processes, subduction zone dynamics and return flow, flow laws for quartz etc. The various topics the authors address in their paper should be listed and properly introduced. Currently the Introduction is very brief and the various goals of the paper are not well explained. The rambling style of the paper is, in part, also reflected by the two Discussion sections in the paper. Overall, the manuscript is not written very well and in place arguments contradict each other

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Discussion paper

(see detailed comments below). I think the paper would be much stronger if written more concisely and the vague and speculative parts were dropped.

My specific comments are a bit painstaking and might be considered harsh (at the end of the day I am still German…). If so, I apologise for that, at least I hope they help to improve the paper.

Specific points:

p.1, l.26-28: To me the second sentence reads as if a subduction channel only exists in the uppermost parts of a subduction zone where sediment is still unconsolidated. The following sentence is also a bit awkward as it is totally obvious that not all sediment is subducted into the mantle – we know for ages about high-pressure rocks.

p.2, l.6: As mentioned above, this paper addresses a lot of different issue but not ‘depth of seismicity’ in subduction zones. Why do bring depth of seismicity up in the Intro and not the many other issues you actually address in the paper?

p.3, l.12-13: You contradict yourself here by saying ‘greenschist facies’ in line 12 and then provide temperatures of 620-650°C in the next sentence. (Later on the metamorphic temperature is given at about 520°C).

p.3, l.21-22: I do not understand this sentence at all. The Pelona schist formed in the trench prior to 68 Ma. Why should there be Late Cretaceous intrusive rocks in a trench sediment? Are you envisaging that a mid-ocean ridge got subducted? Or are you referring to detrital input of arc intrusives into the trench? Why would the arc directly overlie the Vincent fault, which by some authors are regarded as a subduction thrust? Where are forearc high, forearc basin etc.?

p.5, l.3-9: For phengite barometry the phengite should not be zoned and should coexist (in texturally-verifiable equilibrium) with the correct assemblage (see Massonne and Schreyer, 1987). See also p.9, l.26.

p.5, l.25-27: The section on zircon fission track (FT) analysis is much too short and confusing. More details would be needed. I think the authors must have made a mistake when referring to the Pb content here, and they actually mean Uranium, unless they also calculated U-Pb ages? Actually, there are some issues doing laser ablation for U content in zircon FT samples, including zoning. One usually gets a lot of scatter. Using an external mica detector and irradiation is more reliable and accurate. You need to say something about the inferred closure temperature for the ZFT system and have to use cooling rates for inferring a closure temperature. I suggest reading back to Reiners and Brandon (2006, Annual review of Earth and Planetary Sciences). On p.15, l.19-20 you use ZFT ages in some way to argue about the time the rocks crossed the brittle-ductile transition. The way the reasoning goes and the lack of any detailed account on which closure temperature you envisage for the ZFT system makes your argument quite arm-waving.

p.6, l.5-9: For my taste, zoned garnets etc. do not fit well under "Field occurrence".

p.6, l.11-13: Confusing. Are those folds first generation folds that are related to the inclusion trails? I doubt that but the way this is written here would in a way suggest that. Later on it becomes clear that these are F2 and F3 folds. “axis-parallel” stretching lineation should be expressed more clearly, i.e. stretching lineations are parallel to the fold axes, which for sheath folds is implied anyway. It should also be made clearer that both stretching lineations and sheath folds are of the same deformation phase. Descriptions are a bit brief here.

p.6, l.26: You repeat the info on the inclusion trails too often (i.e. p.6, l.11, p.6, l.26, p.7, l.3).

p.6, l.31: “included quartz” reads a bit odd.

p.6, l.26-31: Not much hard evidence here for pressure solution being the dominant deformation mechanism in quartz.

p.7, l.7: “trails of minerals included in albite were crenulated” – I see the crenulation in Fig.4b but not really the albite. If the inclusion trails within a porphyroblast are deformed, then this porphyroblast should have been fully ductile during the crenulation phase, shouldn't it? See p.13, l.16: 'Albite does not develop any crystal-plastic deformation microstructures. . .'.

p.7, l.10-14: Nice to briefly define P domains, would be good to do the same for the Q domains.

p.7, l.16: "deeper than the Iron Fork" is a bit confusing. Do you mean north of Iron Fork? Structurally deeper?

p.7, l.29-31: I have a hard time seeing a shear sense in Fig.6c, in Fig.6b and 6f it is also not too obvious.

p.8, l.26-27: What does "sense of shear. . . is complicated" mean? How can a shear sense be complicated? Is the shear sense alternating between top-SE and top-NW, or often not clear?

p.8, l.30: "quartz new grain shape fabric" is also not too easy to understand. Are you referring to recrystallized grains?

p.9., l.22-23: Did peak-T and peak-P occur at the same time? It is not clear to me when exactly PTmax occurred relative to D1/D2. Qtz-c-axis fabrics apparently developed during D2 and the isoclinal fold as well. Later on you infer that D2 formed during exhumation and decreasing P and T and caused the complete obliteration of garnet and biotite.

p.9, l.8-10: as far as I can tell the data for the qtz-c-axis thermometer are nowhere presented. Would be good to know how many samples you did and how the average of 54718°C relates to the uncertainty of 50°C.

p.9., l.11ff: You refer to muscovite here, then mention the phengite barometer and then say that phengite is part of the mineral assemblage. I think it would help to be a bit more consistent with the mineral names here. Phengite is similar to muscovite but with addition of magnesium.

p.9, l.20: “old muscovite grain compositions” reads odd.

p.9, l.23: might be good to say here that the depth estimate is based on a rock density of 2700 g/m3.

p.9, l.25-26: This reads a bit odd to me, “mylonitized greywacke… muscovites are usually free of strain”.

p.10, l.4-13: Your interpretation of the Ar/Ar hornblende ages is a bit too simplistic for my taste. Metamorphic hornblende usually contains considerable amounts of excess Ar, which you also imply by discarding the 73.4 Ma hornblende age. Another problem is that you uncritically assume that the closure temperature concept can be applied. Have a look at Villa (2015: Geochronology at the crossroads: Disequilibrium textures versus equilibrium modelling. Geological Society, London, Special Publications 2010, v.332; p1-15, doi: 10.1144/SP332.1) for a good review on that. The latter is also important for interpreting the white mica ages and phrases like ‘… and maybe approximately coeval with mylonitization” (wording is so vague that it apparently is not meant to mean much). The new ZFT ages should be presented properly, Table 5 is not really useful as there apparently is no caption. The huge spread in ages should be discussed more thoroughly.

p.10, l.23ff: This is the first of two Discussion sections.

p.10, l.26ff: The lack of biotite might simply have a compositional reason? Why should the Pelona schist in the two discussed areas be compositionally similar? You envisage that garnet is retrograded in the matrix; however, then you should probably find pseudomorphs after garnet. It is highly unlikely that all garnet in the various rocks you sampled has been completely retrograded and no trace of the garnet is left. If biotite and garnet have been replaced by chlorite and white mica during exhumation, why is the white mica zoned?

p.11, l.4-6: The reasoning here is a bit awkward. You showed that s3 is crenulating s2 and this is a robust overprinting criterion which allows to separate s3 from s2. Now you weave in D3 mylonitization and make it sound that this suggests that s3 is younger than s2. Why if s2 formed during exhumation is s3 related to mylonitization? What is the reasoning here?

p.11, l.6-8: Here you refer to your own unpublished data in a sweeping fashion and try to argue that D3 mylonitization occurred at the same time as low-stress mylonite in the upper plate. This conclusion is basically pulled out of a hat and this is not acceptable. I wonder what “low-stress” mylonite means? A mylonite is usually a high-strain rock with small grain size. Are you saying that the small recrystallized grains indicate low-stress conditions when the mylonite formed?

p.11, l.10-12. Awkward sentence. It would help the reader if you reworded this sentence and wrote it much more direct.

p.11, l.16: where are the 51819°C coming from? You refer to RSCM and this is consistent with p.9, l.5-6. But what with T of 54718° as inferred from the qtz-c-axis fabrics? Are you regarding your own analyses useless?

p.11, l19-20: The normal sense movement at the Vincent fault has not been demonstrated. I accept that you infer a shear sense reversal towards the top of the Pelona schist but no good case has been made for normal sense motion on the Vincent fault. Referee Ruth Keppler mentioned the reorientation of the section you studied and you should at least address when and why the section has been rotated. This has clear implication for thrust vs normal fault interpretations for any structure.

p.11, l.22-23: See comments above about closure temperature and Ar/Ar ages and your vague statement on the ages and mylonitization. This is becoming a fact now. . .

p.11, l.24ff: The Discussion of the inverted thermal gradient comes out of the blue as inverted metamorphism of the Pelona schist has only briefly been mentioned before. I have a hard time seeing how this section fits into the paper and what it is good for?

p.12, l.1ff: I am confused about ”channelized extrusion” and “return flow”. Is there a difference? Your writing in line 2 would suggest that there is one but later (about l.20ff) you use the terms more or less synonymously. In line 18 you consider channelized extrusion unlikely but in the following it seems you prefer return flow as the most likely option of schist exhumation (e.g. p.13, l.8). In the Introduction you mentioned a number of exhumation models and you never come back to them in the various Discussion sections. However, the exhumation model you propose for the Pelona schist is much akin to what we proposed for high-P rocks in the Aegean (Ring et al., 2007 as mentioned in the Intro, see also Ring and Glodny 2010, Geol.Soc.Lond. 167, 225-228, doi 10.1144/0016-76492009-134).

p.12, l.24ff: I reckon it would help if Couette and Poiseuille flow was briefly explained. Almost reads like a French menu...

p.12, l.9ff: The discussion on core-complex normal faulting is general and the depth estimate of about 20km is probably a ballpark estimate. Note that core-complex-style normal faulting exhumed rocks from 8-11 kbar on Naxos Island in the Aegean (see review in Ring et al., 2010, Annual Review of Earth and Planetary Sciences, 38, 45-76, doi 10.1146/annurev.earth. 050708.170910), i.e. depths close to the 37km you report for the Pelona schist. I doubt that core complexes form in subduction complexes but the reasoning might be sharpened a bit.

p.13, very first word: What does “that” refer to? The new paragraph may confuse things?

p.13, l.4-6. Interesting thought and speculation. Would be great if one could get a more quantitative handle on that. This is not a criticism!

p.13, l.10-11: Here you refer again to your unpublished data and treat them as facts. Even if D3 exhumation was by normal faulting along the Vincent fault, why does this normal faulting be caused be regional upper-plate horizontal extension? Couldn't the normal fault be the upper boundary of an extrusion (return flow) wedge forming during overall horizontal shortening?

p.13, l.16-17: Here you say that the sheet silicates mainly formed during D3 but you use them for inferring metamorphic pressure for D1 and D2. Why would they nor deform during higher temperature deformation?

p.13, l.19-20: You said that deformation mechanisms in greywacke and chert were different (pressure solution vs crystal-plastic deformation). How does that fit with '...values of strain rate measured from one type of rock can be applied to another'?

p.13, l.21-26: The paragraph on D1 is very vague and speculative.

p.13/14, up to l.9 on p.14: You actually go back and forth about whether pressure solution or dislocation creep was the dominant deformation mechanism of quartz during D2. In this paragraph you need to come up with a shear stress estimate and use the size of recrystallized quartz for that, elsewhere you argue that pressure solution is the dominant deformation mechanism.

p.14, l.27: "thoroughly intense deformation" is sort of a white white horse, isn't it?

p.15, l.4-9: It appears to me that you are assuming that the 6km thick basal section of the Pelona schist was fully coupled to the subduction zone. Please note that Ring and Brandon (1999: Ductile strain and mass loss in the Franciscan subduction complex: Implications for exhumation processes in accretionary wedges, in Exhumation Processes: Normal Faulting, Ductile Flow and Erosion, edited by U. Ring et al., Geol. Soc. Spec. Publ., 154, 55-86) argued that the coupling was <1% in the Franciscan Subduction complex, which is the same subduction zone as the one you are studying. The difference is so stark that you need to discuss/explain your argument.

p.15, l.18-20: again you refer to your own unpublished data. See above for ZFT closure temperature. Here you apparently use 230°C (which would be well above the brittle-ductile transition, which is usually considered to be at 300-350° (see Sibson papers). This is all very speculative and vague.

p.15, l.24ff: Second Discussion!

p.16, l.1-7: The estimates for average density are vague, especially the 2850 kg/m3 for the overriding plate, which renders the 150 kg/m3 density contrast extremely unconstrained, actually to a point where you can start arguing almost anything. In line 3 you refer to "removal of high-density batholithic root" – well, this is also wholly pulled out of a hat here.

p.16, l.19ff: Now all of a sudden you get into flow laws for pressure solution of quartz. This topic has not been introduced before. The next problem is that I am pretty confused whether dislocation creep or pressure solution is considered the main deformation mechanisms for quartz during D2? See paragraph on p.17, l.19-22.

p.17, l.24-25: Here you say pressure solution is the dominant mechanism for quartz deformation during D2 and then switched to dislocation creep during D3. Why would dislocation creep become dominant during decreasing temperature?

Figures: Fig.2: would help if you increased the line width for the Vincent fault to make it stand out a bit. Why has the cross section not been drawn in the tectonic transport direction? Then it would be clearer what the shear sense reversal geometrically looks like and also how the Narrows synform relates to the Vincent fault, the latter would reappear if the cross section was NW-SE. Reviewer Ruth Keppler asked for contour line, which would indeed also help to visualise the structures better. How much reorientation of the original subduction zone structures occurred during San Andreas related dextral and sinistral (I assume the Coldwater and San Antonia faults are sinistral?) strike-slip faulting?

Fig.3: The qtz blobbs in (c) are asymmetric and show top-NW shear. I cannot see the arrows in (f).

Fig.4: Please indicate how many data points there are in (c), and also label X, Y, Z. looks like there is a top-right shear sense in (d).

Fig.6. It would be good if X, Y and Z were indicated in the quartz CPO's, otherwise it is hard to follow the text.

Fig.7: Top-left shear sense in a,b?

Uwe Ring, Stockholm, 16 November 2016

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