Reply to Dr. Ring's Comments

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We thank Dr. Ring for the constructive comments. The manuscript has been modified following the suggestions. Below is our reply.

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.
[Reply] Revised as suggested.

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?
[Reply] Depth of seismicity is added in the main text (Sect. 7.2). The introduction has been revised to cover detailed issues involved in the manuscript.

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).
[Reply] This paragraph starts with "The Pelona schist occurs in the Sierra Pelona and in the eastern San Gabriel Mountains of the Transverse Ranges" (L. 4-5 on P. 3). Greenschist facies was for the Pelona schist in the East Fork of the eastern San Gabriel Mountains as stated in L. 10 of P. 3, and the 620-650°C was for the Pelona schist in the Sierra Pelona as stated in L. 13 of P.3.

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.?
[Reply] The Pelona schist was formed in the trench no earlier than 68 Ma as constrained by the youngest detrital zircon U/Pb age in the Pelona schist (L. 15-16 of P. 3), and hence post-dates all the magmatic activity in the arc, beneath which it was subsequently emplaced.

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.
[Reply] When discussing zoned phengites, Massonne and Schreyer (1987) concluded that "the sluggishness of reequilibration of potassic white micas, which prevents the

establishment of true equilibrium reversals in the experiments as described in this manuscript, is actually the good fortune of the petrologist, who is interested in reconstructing the complicated history of polymetamorphism in natural rocks." The Si content in white mica changed from D2 to D3 may reflect the change of P-T conditions. It is likely that there was K-feldspar and biotite in the Pelona schist at peak conditions. Massonne and Schreyer (1987) also stated that "from natural phengites that do not coexist with the limiting assemblage studied here but still with a Mg, Fe-silicate, at least minimum pressures can be derived with the use of the data presented." This is discussed in Sect. 5.2.

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.
[Reply] This section is revised. Yes, it should be Uranium content here though both U and Pb contents were measured using LA-ICP-MS and U-Pb ages were also calculated. The fission track results are fairly consistent, indicating that laser ablation was not a problem. Determination of zircon fission track closure temperature has been added in the new Appendix A.

p.6, l.5-9: For my taste, zoned garnets etc. do not fit well under "Field occurrence".
[Reply] Titles in Sect. 4 have been revised.

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.
[Reply] The folds are not related to S1. This paragraph has been revised to eliminate possible confusions.

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).
[Reply] Deleted as suggested.

p.6, l.31: "included quartz" reads a bit odd.
[Reply] Revised as "quartz inclusions"

p.6, l.26-31: Not much hard evidence here for pressure solution being the dominant deformation mechanism in quartz.
[Reply] S1 is cryptic as it has been largely modified by later deformation. However, the lack of quartz CPO and the differentiated cleavage support that it was dominantly deformed by pressure solution during D1.

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…'.
[Reply] The crenulations are included in albite porphyroblasts in Fig. 4b. It has been replaced by a crossed polarized light image in the revised manuscript to show albite more clearly. If a porphyroblast is post-tectonic, the porphyroblast may not be deformed even if the inclusions were deformed.

p.7, l.10-14: Nice to briefly define P domains, would be good to do the same for the Q domains.
[Reply] Revised as suggested.

p.7, l.16: "deeper than the Iron Fork" is a bit confusing. Do you mean north of Iron Fork? Structurally deeper?
[Reply] Yes, structurally deeper. Revised to make it clearer.

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.
[Reply] The sense of shear is not clear in Fig. 6c because it does not show a clear asymmetric fabric. Rose diagrams were added to show the shape preferred orientation in Figs. 6c and f.

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?
[Reply] It was supposed to be "is not consistent". Revised.

p.8, l.30: "quartz new grain shape fabric" is also not too easy to understand. Are you referring to recrystallized grains?
[Reply] Yes, new grains here refer to recrystallized grains. Revised.

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.
[Reply] Precise timing of peak pressure and temperature is not easy to constrain. For simplicity, we here assume that peak pressure and temperature occurred at the same time. Samples for quartz c-axis fabric measurement were collected from the deepest structural level and recorded a slow cooling history, as supported by the younger white mica Ar-Ar ages and zircon fission track ages compared to those of the Pelona schist immediately underneath the Vincent fault.

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 547±18°C relates to the uncertainty of 50°C.
[Reply] Three samples were measured for quarz c-axis fabric, and the details are added. 50°C is the calibration uncertainty.

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.
[Reply] Muscovite and phengite have been changed to white mica to keep consistency. The "phengite barometer" is not changed as it is the name of the barometer.

p.9, l.20: "old muscovite grain compositions" reads odd.
[Reply] Jacobson (1983, 1984) reported two sets of muscovite grains, and interpreted them as old and recrystallized, respectively. The results in this paragraph agree with the muscovite compositions of old grains. So it is revised as "with the compositions of old white mica grains."

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

p.9, l.25-26: This reads a bit odd to me, "mylonitized greywacke…muscovites are usually free of strain".
[Reply] Revised to keep clear.

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.

[Reply] Revised as suggested.

p.10, l.23ff: This is the first of two Discussion sections.
[Reply] The deformation conditions and histories are critical to understanding the exhumation mechanisms and discussion of the rheological properties, so it is necessary to summarize here. Title of Sect. 5.5 has been changed to eliminate possible confusion.

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?
[Reply] There is relict biotite in a few Pelona schist samples from the San Gabriel Mtns. Garnets occur in the albite porphyroblasts but were not observed in the matrix, which suggests that the garnets in the matrix were removed during retrograde metamorphism. Zonation of white mica may reflect the change of P-T conditions.

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?
[Reply] Mylonitization of the Pelona schist is characterized by quartz recrystallization. As shown in Fig. 8d, the recrystallized quartz shows a shape preferred orientation parallel to S3 defined by the axial plane of crenulated white mica, so mylonitization was coeval with crenulation. That is, both crenulation and mylonitization occurred during D3 (L. 16-17 of P. 8).

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?
[Reply] The mylonite zone in the upper plate of the Vincent fault shows internal variations in stress, pressure and temperature conditions, and “low-stress/high-stress” was a way to describe the variations. “Low-stress” will be removed as it is not closely related to this manuscript. The results of our work on the mylonites have been presented in conference abstracts, and will be submitted for publication shortly.

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

p.11, l.16: where are the 518.9°C coming from? You refer to RSCM and this is consistent with p.9, l.5-6. But what with T of 547.18° as inferred from the qtz-c-axis fabrics? Are you regarding your own analyses useless?
[Reply] 518.9°C is the average of the RSCM results from 16 samples (P.9 L. 5-6). RSCM has a smaller calibration uncertainty than quartz c-axis opening-angle thermometer, and it is not reasonable to compute the average of the temperatures obtained by those two methods.

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.
[Reply] 1) The original orientation of the Vincent fault. S2, S3, and the Vincent fault are subparallel and dip SW (S2 is slightly more gently dipping than S3 and the Vincent fault). As discussed in the manuscript, S2 was formed in the subduction channel, parallel to the channel boundary. The channel boundary was initially E-dipping as the Pacific Ocean plate was being subducted eastward during the Laramide subduction. Therefore, the Vincent fault was likely to dip E initially.
2) Rotation and tilting of the San Gabriel area. The San Gabriel block has been rotated and tilted. Paleomagnetic study of the Neogene volcanic rocks in the San Gabriel block bounded by the San Gabriel fault and the San Andreas fault yielded a net clockwise rotation of 37.1 ° ± 12.2 ° since the early Miocene (Terres and Luyendyk, 1985). May and Walker (1989) argued for a southerly tilting of the San Gabriel Mountains based on biotite K-Ar cooling ages reported by (Miller and Morton, 1980), and decreases in both the white mica Ar-Ar ages (Grove et al., 2003) and the apatite fission track ages (Blythe et al., 2000) from south to north of the San Gabriel Mountains between the San Gabriel fault and the San Andreas fault support a southerly tilting. The East Fork area lies on the SW-dipping limb of a faulted antiform that was produced during Neogene motion on the San Andreas and Punchbowl faults.
3) Motion direction of the Vincent fault. D3 fabric only occurs right beneath the Vincent fault and shows a top-to-SE sense of shear, so D3 is likely to be related to the Vincent fault. That is, the motion direction of the Vincent fault was top to SE (also supported by the sense of shear in the upper plate rocks). After correcting clockwise rotation, a top-to-SE sense of shear was top-to-E initially. The Vincent fault was dipping E initially and its sense of shear was top to E. The Vincent fault therefore had a normal sense of motion.

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…
[Reply] The closure temperature is addressed as suggested.

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?
[Reply] As introduced in L. 13-15 of P. 3, “an inverted thermal gradient in the Pelona

schist was reported both in the Sierra Pelona (Graham and Powell, 1984) and in the East Fork of the San Gabriel Mountains (Jacobson, 1983, 1995)", and the peak temperature data reported in this manuscript were able to quantitatively test whether if there was an inverted thermal gradient. For clarification, the discussion of inverted thermal gradient is moved to Sect. 9 and a brief introduction is added before the discussion.

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).
[Reply] In the channelized extrusion model discussed by Chapman et al. (2010) the entire subduction assemblage is brought up toward the trench "en masse" and coaxial deformation is the dominant flow pattern. This does not fit our observations in the Pelona schist, which suggest intense non-coaxial deformation. The lack of driving force and quantitative dynamic analysis of the extrusion wedge model (Ring et al., 2007a, 2007b; Ring and Glodny, 2010) sets it apart from the return flow model discussed in this manuscript, which is based on mechanical analyses by (Beaumont et al., 2009).

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

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.
[Reply] Revised as suggested.

p.13, very first word: What does "that" refer to? The new paragraph may confuse things?
[Reply] It refers to the change of the sense of shear discussed in the previous paragraph (P. 12 L. 30-31). Revised as suggested.

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!
[Reply] No changes were made.

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?
[Reply] The results of our work on the Vincent fault have been presented in conference abstracts, and will be submitted for publication shortly. When the Pelona schist was exhumed in the return flow during D2, there must have been an upper boundary above the schist, but the present-day Vincent fault was not this boundary. The Vincent fault was formed during D3 and was accompanied by the mylonitization of the Pelona schist and the Narrows synform. The overprinting relationship between S3 and S2 suggests that D3 was a separate deformation event from D2, i.e., return flow had stopped during D3. As in the reply to the comments on L. 19-20 on P. 11, the Vincent fault had a normal sense of motion.

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?
[Reply] L. 15-17 on P. 13 reads "sheet silicates were mainly deformed during S3."

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'?
[Reply] Different deformation mechanisms do not imply that the strain rates have to be different. The field observations indicate that the three types of rocks in the Pelona schist was coupled during deformation, as explained in L. 17-20 on P. 13 that "metachert interlayers in metagreywacke do not show boudinage or buckle folds without the surrounding metagreywacke or greenschist being involved, implying that metachert and metagreywacke were coupled during deformation. This suggests that the 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.
[Reply] S1 is cryptic as it has been largely modified by later deformation. However, a clear case has been made that the dominant deformation mechanism was pressure solution based on the lack of quartz CPO, the differentiated cleavage, and lack of evidence of crystalline plastic. The piezometer was not applicable as the grainsize formed during D1 has been modified, but it is reasonable to use the stress of D2 as a proxy to estimate the stress at the end of D1. For simplicity, the peak P-T conditions were assumed to occur at the end of D1.

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.
[Reply] Revised.

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

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.
[Reply] It is likely that the Franciscan accretionary wedge was largely decoupled from the down-going slab, but the Pelona schist was subducted to a greater depth. The source of non-volcanic tremors in the subduction zone is near the down-dip limit of megathrust earthquakes (Ide et al., 2007), and this limit was estimated at 25 km for Cascadia (Chapman and Melbourne, 2009). The Pelona schist was deeper than 25 km during most of D2, and deformation was entirely ductile, so it is likely that the base of the Pelona schist was coupled to the down-going slab.

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.
[Reply] As explained in L. 19-21 of P. 15, a linear cooling history was assumed between the temperature of D3 and the closure temperature of zircon fission track. The detailed calculation is added in the new Appendix A.

p.15, l.24ff: Second Discussion!
[Reply] The first discussion has been revised as a summary.

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.
[Reply] The chemical composition of the North American lithosphere is the key to estimating the density of the rocks above the subduction channel. As argued by Saleeby et al. (2003), there was a high-density sub-batholithic root attached to the bottom of the Mesozoic magmatic arc, but this root was removed during the Laramide subduction, resulting in a much less dense lithosphere. The remaining lithosphere consisted of mainly felsic batholithic rocks (Saleeby et al., 2003), and its density is estimated as 2750 kg/m^3.

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.

[Reply] There are a number of issues that arise from the data presented in this manuscript. One of the issues is the flow law of quartz pressure solution which is poorly constrained, and our data can shed light on this issue from naturally deformed rocks. An introduction is added at the beginning of Sect. 9. The dominant deformation mechanism of quartz in metagreywacke during D2 was pressure solution but dislocation creep also contributed to the bulk deformation, as described in L. 29-30 of P. 13 and L. 1-5 of P. 14

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?
[Reply] Deformation mechanism is a function of temperature, grainsize, water fugacity, stress, and other variables. Temperature is only one of the variables.

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?
[Reply] A satellite image has been added. Rotation and tilting was discussed in the reply to the comment on L.19-20 of P.11. The cross-section was drawn in its current orientation because that the samples were collected perpendicular to the foliation and lineation along the East Fork, and it shows the profile of the Narrows synform. An illustrative transect is added in Fig. 11d.

Fig.3: The qtz blobbs in (c) are asymmetric and show top-NW shear. I cannot see the arrows in (f).
[Reply] Yes, the quartz boudinage in Fig. 3c appears to indicate top-to-NW shear.

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).
[Reply] Revised as suggested. The internal fabric of the epidote does not continue through the matrix in Fig. 4d, so the sense of shear cannot be inferred.

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.
[Reply] Revised as suggested.

Fig.7: Top-left shear sense in a,b?
[Reply] The sample in Figs. 7a and b was not oriented. Photomicrographs of an oriented sample are added.

Reference:

Beaumont, C., Jamieson, R. A., Butler, J. P. and Warren, C. J.: Crustal structure: A key constraint on the mechanism of ultra-high-pressure rock exhumation, Earth and Planetary Science Letters, 287(1–2), 116–129, doi:10.1016/j.epsl.2009.08.001, 2009.

Blythe, A. E., Burbank, D. W., Farley, K. A. and Fielding, E. J.: Structural and topographic evolution of the central Transverse Ranges, California, from apatite fission-track, (U-Th)/He and digital elevation model analyses, Basin Research, 12(2), 97–114, doi:10.1046/j.1365-2117.2000.00116.x, 2000.

Chapman, A. D., Kidder, S., Saleeby, J. B. and Ducea, M. N.: Role of extrusion of the Rand and Sierra de Salinas schists in Late Cretaceous extension and rotation of the southern Sierra Nevada and vicinity, Tectonics, 29(5), 1–21, doi:10.1029/2009TC002597, 2010.

Chapman, J. S. and Melbourne, T. I.: Future Cascadia megathrust rupture delineated by episodic tremor and slip, Geophysical Research Letters, 36(22), L22301, doi:10.1029/2009GL040465, 2009.

Graham, C. M. and Powell, R.: A garnet-hornblende geothermometer: calibration, testing, and application to the Pelona Schist, Southern California, Journal of Metamorphic Geology, 2(1), 13–31, doi:10.1111/j.1525-1314.1984.tb00282.x, 1984.

Grove, M., Jacobson, C. E., Barth, A. P. and Vucic, A.: Temporal and spatial trends of Late Cretaceous–early Tertiary underplating of Pelona and related schist beneath southern California and southwestern Arizona, Geological Society of America Special Paper, 374, 381–406, doi:10.1130/0-8137-2374-4.381, 2003.

Ide, S., Shelly, D. R. and Beroza, G. C.: Mechanism of deep low frequency earthquakes: Further evidence that deep non-volcanic tremor is generated by shear slip on the plate interface, Geophysical Research Letters, 34(3), L03308, doi:10.1029/2006GL028890, 2007.

Jacobson, C.: Relationship of deformation and metamorphism of the Pelona Schist to movement on the Vincent thrust, San Gabriel Mountains, southern California, American Journal of Science, 283, 587–604, 1983.

Jacobson, C.: Qualitative thermobarometry of inverted metamorphism in the Pelona and Rand Schists, southern California, using calciferous amphibole in mafic schist, Journal of Metamorphic Geology, 13, 79–92, 1995.

Jacobson, C. E.: Petrological evidence for the development of refolded folds during a single deformational event, Journal of Structural Geology, 6(5), 563–570, doi:10.1016/0191-8141(84)90065-8, 1984.

Massonne, H.-J. and Schreyer, W.: Phengite geobarometry based on the limiting assemblage with K-feldspar, phlogopite, and quartz, Contributions to Mineralogy and Petrology, 96(2), 212–224, doi:10.1007/BF00375235, 1987.

May, D. J. and Walker, N. W.: Late Cretaceous juxtaposition of metamorphic terranes in the southeastern San Gabriel Mountains, California, Geological Society of America Bulletin, 101(October), 1246–1267, doi:10.1130/0016-7606(1989)101<1246:LCJOMT>2.3.CO;2, 1989.

Miller, F. and Morton, D.: Potassium-argon geochronology of the eastern Transverse Ranges and southern Mojave Desert, southern California, Geological Survey Professional Paper, 1152, 1–30, 1980.

Ring, U. and Glodny, J.: No need for lithospheric extension for exhuming (U)HP rocks by normal faulting, Journal of the Geological Society, 167(2), 225–228, doi:10.1144/0016-76492009-134, 2010.
Ring, U., Glodny, J., Will, T. and Thomson, S.: An Oligocene extrusion wedge of blueschist-facies nappes on Evia, Aegean Sea, Greece: implications for the early exhumation of high-pressure rocks, Journal of the Geological Society, 164(3), 637–652, doi:10.1144/0016-76492006-041, 2007a.
Ring, U., Will, T., Glodny, J., Kumerics, C., Gessner, K., Thomson, S., Güngör, T., Monié, P., Okrusch, M. and Drüppel, K.: Early exhumation of high-pressure rocks in extrusion wedges: Cycladic blueschist unit in the eastern Aegean, Greece, and Turkey, Tectonics, 26(2), TC2001, doi:10.1029/2005TC001872, 2007b.
Saleeby, J., Ducea, M. and Clemens-Knott, D.: Production and loss of high-density batholithic root, southern Sierra Nevada, California, Tectonics, 22(6), doi:10.1029/2002TC001374, 2003.
Terres, R. R. and Luyendyk, B. P.: Neogene tectonic rotation of the San Gabriel Region, California, suggested by paleomagnetic vectors, Journal of Geophysical Research, 90(B14), 12467–12484, doi:10.1029/JB090iB14p12467, 1985.