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Solid Earth Discussions

Interactive comment on "Rheological transitions in the middle crust: insights from Cordilleran metamorphic core complexes" *by* Frances J. Cooper et al.

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<Specific Issues> Reviewer comment 1: It would be good to have a clearer statement of the main contribution of this study and its difference from other papers previously published by the same authors.

Response 1: In order to emphasize the new contributions in the paper, we have added a few sentences to the end of the abstract and to the Introduction (page 3, lines 25–33) as follows:

"Using field observations, microstructural analyses, and thermobarometric data, we (i) document the exhumation of footwall rocks from the middle crust to the surface in

each core complex as they pass through different rheological transitions; (ii) document the exhumed LDT in the Whipple Mountains, show why it is not easily identified in the northern Snake Range, and has not been exhumed in the Ruby Mountains–East Humboldt Range; (iii) present a prediction of, and a mechanical explanation for, the geometry of the detachments in these three core complexes and many like them; and (iv) show that the detachments in all three core complexes formed during the Miocene, and post-date early phases of extension and exhumation in the exhumed mid-crustal metamorphic rocks."

Reviewer comment 2: The importance of geothermal gradient is emphasized in several places but there is little discussion in the main text. For the calculations of shear zone width values are assumed without justification. Some discussion of this issue would be an improvement.

Response 2: In order to address this, we have added an extra sentence to the caption to Figure 3 as follows: "Vertical scale is drawn for an average geothermal gradient of 25°C/km, although in reality the geothermal gradient will change during exhumation. We have chosen this value because it represents a reasonable average during slow extension of the continental lithosphere".

<Detailed Points> Reviewer comment 3: P.3, l.8 Rapid exhumation and snapshots makes this sound almost instantaneous. The important thing is that the time scales for exhumation are short enough that early-formed microstructures are not destroyed by re-equilibration on the way up. Is this really a characteristic of core complexes? I think we see the same thing in metamorphic domains exhumed in different tectonic settings.

Response 3: We agree that the rapid exhumation means that early-formed microstructures are preserved rather than destroyed and we have clarified our statement here accordingly (page 3, lines 8–9): "Rapid exhumation of most core complex footwalls means that early-formed microstructures are not destroyed by re-equilibration, and they can thus preserve a snapshot through the middle and upper crust prior to exhumation".

Reviewer comment 4: P.3, l.21-23 These questions are important, but it is not clear to me in what sense this manuscript really takes things further than already covered in other papers published by the same group of authors. Platt et al. GSL special publication have covered the rheological implications of core complexes and Cooper et al 2010b have already made the arguments for two rheological transitions in the crust. In addition, the theoretical considerations concerning shear zone width have been covered by Platt & Behr 2014. I think it would be best to present this as a review paper. If the authors wish to emphasize some original contribution of this manuscript it should be made clear by highlighting the differences with their earlier work. The main new data seem to be the zircon age data. These are interesting but unfortunately do not seem to add significantly to what we knew before.

Response 4: As mentioned in our response to comment 1, we have added a few sentences to the end of the abstract and tp the Introduction (page 3, lines 25–33) in order to emphasize the new contributions of this paper.

Reviewer comment 5: Fig. 2 The ornament used for the mylonitic lineation suggests all the lineations are perfectly aligned. It is only meant to be schematic, but it would be better to give some indication of the variability in orientation.

Response 5: Given the small scale of the figures, it would be difficult to do this adequately. Instead, we give references to the sources of the data in each case study, and we have added a sentence to the figure caption stating: “In each case, the mylonitic lineation is shown schematically”.

Reviewer comment 6: P.5, l.10 Co-axial extension... Since the rest of the manuscript discusses non-coaxial deformation within mylonite zones this expression is rather confusing. It presumably refers to deformation on a crustal or lithospheric scale. I am unclear why the degree of non-coaxiality of regional extension is relevant: extension concentrated along shear zones will cause exhumation and overprinting irrespective of the large-scale kinematics.

Response 6: To avoid confusion here, we have deleted the word “coaxial” from the sentence.

Reviewer comment 7: P.8, l.21 FTIR measurements are useful because they can distinguish between structurally bound OH and water present in fluid inclusions.

Response 7: While we agree that FTIR measurements can be useful, they are highly sensitive to grain orientations and are near impossible to conduct on samples this fine-grained without incorporating grain boundaries.

Reviewer comment 8: P.9, Fig.4. B Presumably this should be G2 and Q2.

Response 8: Yes, this is correct and Figure 4 has been revised accordingly.

Reviewer comment 9: l. 19 this wording suggests that the thickness of the shear zone is 19 km!

Response 9: This has been corrected to read “DRX creep in quartz increases down to a depth of $\sim$19 km”.

Reviewer comment 10: P.10, l.11 The figure in the supplementary material does not give the rock types for the grey layer at the top of the outcrop nor the mylonite zone. Silicic dyke is rather vague. Granitic? If the pink dykes are synkinematic then strictly speaking the ages do not bracket the timing of mylonite formation. The strongly deformed dykes could also be synkinematic. The caption actually says that the age of one dyke does this bracketing by itself. This is clearly wrong.

Response 10: We have revised Figure S1 and added annotations showing the different lithologies present. Rather than using the generic term “silicic”, we have specified that the pre-kinematic dikes are granite and the syn-kinematic dikes are tonalite. We have also labeled the mylonite zone lithology as orthogneiss.

We have also revised the figure caption and removed the statement saying that the age is bracketed by one dike. The caption now reads: “Figure S1. (A) Field photograph and

(B) line drawing of deformed granite (blue) and tonalite (pink) dikes that we interpret to be pre- and syn-kinematic with respect to development of the mylonitic foliation. Dike PW82 ($\sim$65.7 Ma) is boudinaged and oriented parallel to the foliation, suggesting that it was already present at the onset of mylonitic deformation. Dike PW81 ($\sim$24.2 Ma) cross-cuts the mylonitic foliation, but is itself mylonitized under high-temperature conditions of >450°C, based on the presence of dynamically recrystallized feldspar."

Reviewer comment 11: Supplementary material: 'Dates at the confidence level...' 2 sigma errors or 95% confidence limits?

Response 11: Individual U/Pb dates are quoted at 2σ, as stated. However, the weighted mean ages are reported at the 95% confidence limit. We have added this statement after reporting the weighted mean ages as follows: "Analyses of zircon rims, excluding older inherited cores, give weighted mean 206Pb/238U ages of 24.2 $\pm$ 1.0 Ma for PW81 and 65.7 $\pm$ 3.6 Ma for PW82 (reported at the 95% confidence limit)."

Reviewer comment 12: P. 10, l.13 'K/Ar' implies division. K–Ar is better.

Response 12: We have replaced "K/Ar" with K-Ar.

Reviewer comment 13: l.14 Some comments about the temperature expected to be recorded by this dating method and temperatures required for mylonite formation would help make this clearer.

Response 13: We have added the closure temperature and reference used by Foster and John (1999). The sentence now reads: "The mylonites in the Whipple footwall cooled through the K-Ar closure temperature of K-feldspar (180–200°C; Lovera et al., 1993) by $\sim$21 Ma (Foster and John, 1999)."

Reviewer comment 14: '_26–21 Ma' The data presented suggest a range of 31–21 Ma.

Response 14: This is true if we use the original 26 $\pm$ 5 Ma age range given by Wright et al. (1986). However, we note that this poorly resolved constraint was later refined by Foster and John (1999) to 24 $\pm$ 0.5 Ma, which agrees closely with our new U-Pb data.

We therefore take $\sim$26 Ma to be a more realistic maximum constraint on the timing of mylonitization than 31 Ma.

Reviewer comment 15: p.10, l.18 Appear to be Tertiary in age... Tertiary covers rather a long period of time and is rather vague. Tertiary is also no longer officially recognized by the International Commission on Stratigraphy.

Response 15: In order to better constrain our point here, we have replaced "Tertiary" with "Late Oligocene-Early Miocene": "...all the fabrics we describe here appear to be Late Oligocene-Early Miocene in age."

Reviewer comment 16: p.12, l.20 subparallel but truncates at low angle.. rather tautologous.

Response 16: We have simplified the text here to: "The brittle shear zone truncates the main foliation at a low angle."

Reviewer comment 17: p.13, l.11 This does not have to represent diachronous exhumation. It does record the age at which a particular temperature contour is crossed assuming all the other things that affect closure temperature are equal.

Response 17: Yes, it is true that the ages simply reflect cooling and not necessarily exhumation. We have clarified this by modifying the discussion as follows: "40Ar/39Ar mica cooling ages of footwall mylonites across the range obtained by Lee and Sutter (1991) and Lee (1995) show a systematic increase from 20 Ma in the east to 50 Ma in the west. This led these authors to suggest that the footwall was progressively exhumed from west to east along an E-dipping brittle NSRD that was active over this period of time."

Reviewer comment 18: P.15, l.9 Is the issue the fine grain size or the difficulty in obtaining a homogenous domain large enough to measure?

Response 18: It is the latter, but we do not feel it necessary to elaborate on this point in the text as it is not pertinent to our interpretation.

Reviewer comment 19: p.16, l.21 ‘directly comparable’ is not a very clear expression. Geometry?

Response 19: To improve the clarity here, we have modified the caption to Figure 6F as follows: “Attenuated fold in sub-detachment Marble Wash calc-mylonites, with a geometry comparable to the winged inclusions found in ultramylonites of the Ruby Mountains-East Humboldt Range footwall (Fig. 7E) (view to N).”

We have also updated the caption to Figure 7E as follows: “Ultramylonite beneath the main detachment at Secret Pass with winged inclusions (Grasemann and Dabrowsky, 2015). Similar features are found close to the detachment in the northern Snake Range (Fig. 6E).”

Reviewer comment 20: Fig. 6 H This does not show the characteristics of a gouge zone very clearly.

Response 20: We agree, and have modified the caption to read: “Folded and disrupted gouge zone along the NSRD, representing the final step in the exhumation path (view to N).”

Reviewer comment 21: P.18, l.10 Intensely high-strain -> intensely deformed or very high-strain.

Response 21: To avoid confusion here, we have removed the word “intensely" and the sentence just refers to “a zone of high-strain mylonite. . .”.

Reviewer comment 22: P.20, l.16 presumably the variability is in orientation.

Response 22: Yes, this is correct and we have changed this sentence to: "Stretching lineations are weak with variable orientations, but generally trend N-S.”

Reviewer comment 23: P.20, l.21 Using a forward slash to separate two words can be useful shorthand, but it is not generally accepted in formal writing. What’s wrong with ‘or’?

Response 23: We have replaced “structural/rheological” with “structural or rheological”.

Reviewer comment 24: L.21, l.24 not really detailed, more of a summary of detailed work.

Response 24: We agree and have deleted the word “detailed".

Reviewer comment 25: p.20, l.26 have we discussed differences in thermal gradient?

Response 25: Indeed we have not, and it is difficult to put precise figures on the thermal gradients in each of the three examples or put any real constraints on what the conditions were exactly at the start of exhumation. Therefore, we have removed reference to thermal gradient. The sentence now reads: “This implies that, despite complications of lithology or pre-existing structure„ core complexes offer a consistent window into the rheological structure of the middle crust.”

Reviewer comment 26: l.26 ‘Unique but consistent’ These seem to refer to different aspect of the work and it would be better to separate them. Do the authors mean that extensional core complexes are unique or that each of the individual areas is unique but discussed together they have identifiable common characteristics?

Response 26: In order to avoid this confusion, we have removed the word “unique”, so the sentence now only refers to the consistency between the three core complexes in providing a window into the middle crust: “This implies that, despite complications of lithology, pre-existing structure, and differences in geothermal gradient, core complexes offer a consistent window into the rheological structure of the middle crust”.

Reviewer comment 27: p. 21 l. 29 kilometers?

Response 27: We have corrected this omission, so the sentence now states: “upper crustal rocks were buried and then exhumed from depths of $\sim$48–67 km”.

Reviewer comment 28: p. 22 l. 10 ‘active at vey gentle dips ...’ reference?

Response 28: We have added references here to:

Davis, G. A.: Rapid upward transport of mid-crustal mylonitic gneisses in the footwall of a Miocene detachment fault, Whipple Mountains, southeastern California, Geol. Rundsch., 77, 191–209, 1988.

Davis, G. A., Anderson, J. L., Frost, E. G., and Shackelford, T. J.: Mylonitization and detachment faulting in the Whipple-Buckskin-Rawhide Mountains terrane, southeastern California and western Arizona, Geological Society of America Memoir 153, 79-129, 1980.

Scott, R. J., and Lister, G. S.: Detachment faults: Evidence for a low-angle origin, Geology, 20, 833–836, 1992.

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