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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.

S. R. Wallis

swallis@eps.nagoya-u.ac.jp

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<General Comments>

This is a succinct and well-written review of the rheological implications of deformation exposed in metamorphic core complexes of the western USA. The approach is clear and the manuscript does an excellent job of weaving together information from structural geology, metamorphic petrology and geochronology with a good range of suitable references. The subject material is a summary of previously published work and its novelty is in presenting the material from numerous individual examples in an accessible format. Overall this is an very good contribution and deserves to be widely read.

<Specific Issues>

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.

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.

<Detailed Points>

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.

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.

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.

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

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

P.9, Fig.4. B Presumably this should be G2 and Q2.

l. 19 this wording suggests that the thickness of the shear zone is 19 km!

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.

'Dates at the confidence level...' 2 sigma errors or 95% confidence limits?

P. 10, l.13 'K/Ar' implies division. K–Ar is better.

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.

'∼26–21 Ma' The data presented suggest a range of 31–21 Ma.

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.

p.12, l.20 subparallel but truncates at low angle.. rather tautologous.

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

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

p.16, l.21 'directly comparable' is not a very clear expression. Geometry?

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

P.18, l.10 Intensely high-strain -> intensely deformed or very high-strain

P.20, l.16 presumably the variability is in orientation

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'?

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

p.20, l.26 have we discussed differences in thermal gradient?

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?

p. 21 l. 29 kilometers?

p. 22 l. 10 'active at vey gentle dips ...' reference?

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