

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

Anonymous Referee #1

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This is an interesting manuscript, and the figures in case studies are nice. But it suffers from several major problems that make it inappropriate to be published as an original research article.

I listed three major issues that may help the authors: 1) The fundamental problem is that most of the ideas and concepts have been published by the same authors previously, e.g., Cooper et al. (2010), Platt and Behr (2014), and Behr and Platt (2011), etc. I do not think the new contribution in the paper warrant a new research article. If the authors believe so, they should write a clear statement in the manuscript about what is new, and how it compares to their papers in the past.

2) The two major questions the authors pledge to address in the manuscript does not

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get addressed by the contribution from the manuscript, but get partially answered by a synthesis of previously published papers. In addition, the three case studies are more of a synthesis rather than original research. I think the authors should specifically state and focus on the contributions from this manuscript, and on how the new contributions help to answer the questions.

3) The definition of LDT need to be refined and be more specific. I do not see how this definition help us to understand the rheology of the middle crust. It essentially says that below an localised narrow shear zone there exists a zone with more distributed deformation. The thickness of the zone is essentially unknown, or at least not specified by the authors. One can understand that it is really not easy to specify a thickness. But on the other hand, one can also argue that such a vague concept does not help us to understand the rheology at all. There are no standards proposed to specify what is localised and what is distributed deformation. LDT also has dynamic effects inherent in its name itself, but the authors seldom mention it. The author make a great deal of the relationship between depth and LDT. It causes major problems since depth itself says little about the lithology, temperature, strain rate, and stress. The authors may need to add some discussions about the dynamic history and/or some discussions about the geothermal gradient. A loosely defined geothermal gradient and almost no touch on dynamic history during exhumation cannot support the strong depth restriction specified in the manuscript which the authors make a great deal of.

Based on the above three major issues, I do not recommend publication of the manuscript in Solid Earth.

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