

Interactive comment on “High-temperature metamorphism during extreme thinning of the continental crust: a reappraisal of the north Pyrenean paleo-passive margin” by C. Clerc et al.

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Reviewer's comment's to manuscript:

High-temperature metamorphism during extreme thinning of the continental crust: a reappraisal of the north Pyrenean paleo-passive margin by C. Clerc, A. Lahfid, P. Monié, Y. Lagabriele, C. Chopin, M. Poujol, P. Boulvais, J.-C. Ringenbach, E. Masini and M. de St Blanquat

First and foremost: this is an excellent and well documented paper, which reviews previous data and results as well as presents a wealth of new data (field observations, RMCM, geochronology), which contribute to further understanding of the pre-Pyrenean

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passive margin evolution in the North Pyrenean zone. The paper is well organised and the presentation of data and interpretations likewise. After my first quick reading it is immediately obvious to that the paper should be published in 'Solid Earth'.

There are, however, some issues (mostly technical) that need to be addressed and corrected. I will provide details below with reference to the page and line numbers as well as to figures which probably need some modification.

First some general comments: The authors mention in their introduction the importance of recognising hyperextension and mantle exhumation in exposed palaeo-margins within orogenic belts because these provide direct insight into the geological processes involved in modern margins. I would like to remind the authors that this also applies to the margins of "real" palaeo-oceans in the case of the Appalachian-Caledonian orogen (Iapetus ocean; see Andersen et al. 2012, *Jl. Geol. Soc. London*, v. 169, p. 601–612, and Chew and Van Staal, 2014, *Geoscience Canada*, v. 41, <http://dx.doi.org/10.12789/geocanj.2014.41.040>).

This paper only mentions the Pyrenees and the Alps which both formed by closure of very small basins with no or very minor real oceanic lithosphere, respectively. Perhaps the scope should be widened to include the ancient large equivalents of today's Atlantic passive margin? The authors should perhaps remind themselves as well as the readers that such new interpretations have been presented from both the Appalachians and the Caledonides.

Another topic that could be introduced and discussed is whether the NPZ is unique because of the early HT-LP metamorphism that can be attributed to the extension? Is this a feature we should expect universally in such tectonic setting and if so, why is this not commonly described from elsewhere?

The manuscript apparently use 'exhumation' and 'uplift' of mantle synonymously several places in the text. My understanding of these terms is 'exhumation' describes the approach of a rock-unit towards or all the way to the paleo-surface, whereas 'uplift' is

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just upward motion of the paleo-surface with respect to a reference level. This is quite an important difference because one may have 'exhumation' during both 'uplift' and 'subsidence' of the paleo-surface and vice versa.

Use of stratigraphic nomenclature: The authors use some, to me and probably most geologists very strange stratigraphic units, particularly in the legend of figure 7. I needed to consult various internet cites to find these names of and was even unsuccessful to find some of them. Are they correctly spelled? I think the paper should be revised to use only internationally recognised stratigraphic names, please change this to use the geological time scale as presented in for example Gradstein et al. 2012, "The Geological Time Scale" see: <https://engineering.purdue.edu/Stratigraphy/> Please correct figure 7 so that it can be understood and I also suggest the legend should be in stratigraphic order (old below young).

Details: Page 800: line 8, correct to Alpine with capital letters page 804: line 26 'pressure shadow' is not a good term here, better use 'boudin necks' chapter 2.3 Magmatism You write page 805 line 8 that there is 'widespread magmatism', this is apparently correct, but gives the reader the impression it is voluminous as well, this is not really the case, is it? Perhaps a slight modification would clarify this point. Page 807, line 14, I believe this should read 'key localities' (not kea) Page 813, line 4, uplift vs exhumation, see above and explain. Page 814, lines 8-9, I think you need to quickly explain why you think thermal fluxes are more prominent in strike-slip (transcurrent) than other tectonic domains, not just give some refs without more. Perhaps rewrite this and add one or two sentences (in addition to the refs). Page 814 line 20. An example with quantification of the cooling in (some) sedimentary basin(s) by fluid circulation is given by Souche et al. 2014, Geofluids 14, 58–74 Page 816, line 2, uplift or exhumation?

Comments to figures and figure captions: Figure 2. Correction: Inset frames showing Fig. 5 and Fig. 6 are wrongly numbered, they should be Fig. 7 and Fig. 8. Figure 4, Lacks a scale Figure 7. The Legend needs modification according to accepted stratigraphic nomenclature and should be in stratigraphic order (see comments

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above). Figure 10. Perhaps better change the caption to 'Early Jurassic' rather than the abandoned Liassic? Figure 11: Comment to the caption; I believe it is normal to use $^{40}\text{Ar}/^{39}\text{Ar}$ (not the other way around) Also a number of places in the text you use Ar-Ar or just Ar, you better change this and be consequent throughout the paper.

This conclude my comments. The paper is great and I really liked reading it. I hope you find my comments useful and that the contribute to make the paper even better.
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