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Solid Earth
Discussions

Interactive comment on "High-grade deformation in quartzo-feldspathic gneisses during the early Variscan exhumation of the Cabo Ortegal nappe, NW of Iberia" *by* F. J. Fernández et al.

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The manuscript presents many observations, so many in fact that it becomes quite confusing to the reader, especially to one not familiar with the local geology. In the end, it is not really clear to me what the overall aim of the work is and what the new conclusions are. The title in itself is quite general and does not indicate what may be new and of general interest in the manuscript. In both the abstract and the conclusions section, the major point would seem to be that extrusion to the SE involved coeval activity of a basal thrust and top detachment, but the study was not really focussed on establishing either the kinematics or timing of these bounding structures. New information presented

comes mainly from a new map of the area, new U-Pb ages on zircon and monazite grains separated from two felsic dykes, and some CPO determinations on quartz-rich samples, which do not show any consistent pattern or sense of shear (I suspect in many cases in Fig. 9 because the figures are not actually correctly aligned relative to the real kinematic lineation – the authors note the lineation is weak and difficult to discern). Crucial information on an apparent 0.5 GPa pressure difference to either side of the top shear zone comes from the literature and no new data or critical (re-) assessment of the possible errors involved in establishing this pressure difference (or whether the metamorphic pressures determined to either side of the structural boundary are synchronous) is presented. The English could do with significant improvement but the main criticism of the presentation is the lack of a clear red line to follow through the many observations toward some well-defined aim that would be of interest to a broad audience. In its current form, the manuscript is more appropriate for a regional journal than for Solid Earth.

More detailed comments:

Abstract line 10: it is difficult to envisage how "bulk flattening" can produce exhumation

Abstract line 12: on the same point, localization of strain along the boundaries reflects localized shearing, rather than "bulk flattening".

Page 3545 line 18: are the eclogites relict pods within the migmatites or did the migmatites develop under similar eclogitic conditions?

Page 3546 lines 6-9: in answer to my question above, you say "subsequent partial migmatization" but then quote an age for this migmatization as 397-390 Ma, which actually, within error, is identical to the 400-390 Ma range you quote for the HP-HT event.

Page 3546 lines25-26: "thought to form during exhumation" – what does this mean? What is your opinion considering you are working almost exclusively with the D2 history

– is it related to exhumation and decompression or not?

Page 3548 line *: so this is the direct answer to my question immediately above – although you just state it as a fact without any real supporting evidence.

Page 3549 lines 21-28: OK so here the relative age relationships between the felsic diorite dykes, the eclogite blocks, the migmatization and the D2 deformation are established.

Page 3550 lines 25-28: now I am lost... What is the relationship of the felsic dykes to the eclogite blocks? Is the eclogitization related to the 400-390 Ma HP-HT event of page 3546 line 6? If so these 480-488 Ma old dykes must be considerably older than the eclogites, which doesn't seem to make any sense from your field descriptions.

Page 3551 line 11: is St still stable at 700C? There is a staurolite-out isograd toward higher T but maybe at 1.2 GPa it is stable at 700C – I have not checked.

Page 3533 lines 22-26: I am not sure what this analysis is supposed to show. The eclogite blocks were almost certainly ellipsoidal to start with and not spherical so what is the Flinn diagram of their final shape supposed to indicate?

Page 3554 lines 1-12: I do not really see what you are interpreting here as "Poiseuille flow", which by the way is laminar pipe or channel flow driven by a pressure gradient, so in you sketch it would imply a lateral, non-lithostatic pressure gradient from left to right.

Page 3555: so my interpretation from before was correct? The felsic diorite dykes predate the HP-HT eclogite facies metamorphism. So is there any evidence for this in the dykes themselves – e.g. plagioclase should no longer be stable under eclogite facies conditions

Page 3558 lines 15-18: yes, but there has been little or no evidence presented to establish that movement on the bounding thrust and the detachment structures were synchronous, which is implicit in any extrusion or channel flow model. They could have

developed sequentially, e.g. first thrusting and then the extensional detachment, which would be a very different model for exhumation of the unit in between.

Page 3559 line 20: this is an important piece of the puzzle. What are the + and – error estimates on these values and how well is it established that the metamorphic conditions showing this jump were actually coeval?

Interactive comment on Solid Earth Discuss., 7, 3541, 2015.