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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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Dear Editor, In the light of comments by both reviewers, while keeping the descriptive data section essentially the same, we have made moderate to substantial changes to the main topic of discussion in the paper to put in context the sequence of structures we report from Masanteo. Rather than focussing the manuscript in discussing whether the channel flow is applicable or not to explain the tectonic evolution from eclogite facies to mid crustal conditions in the gneisses within the Cabo Ortegal nappe, we have

decided to take an alternative approach, which is to characterize from the tectonic point of view the exhumation path of rocks from eclogite facies to greenschist facies, linking the progressive development of structures to evolving metamorphic conditions in gneisses but also in neighbouring metabasic granulites, which tell a similar story. Indirectly, we have removed unnecessary parts of the regional component which may obscure those key features of the exhumation that may be of interest to a wider audience. We would like to portray the gneisses in Cabo Ortegal nappe, but also the whole crustal section, as a key location to study widespread thinning of lower crustal section under evolving conditions from high pressure to mid crustal depths. In this new version we provide robust figures for the exhumation path in PT space, based in the gneisses and supported by those in the literature from neighbouring metabasic rocks with well reported metamorphic evolution during exhumation. The amount of cooling during decompression we report is comparable to what it is estimated for subducting slabs in present day subduction zones and numerical models. This is certainly a feature characteristic of the geodynamic setting. By comparing the metamorphic record during the exhumation of Cabo Ortegal gneisses and mafic granulites with present day examples of subduction zones another important point stands out, which is that the exhumation path is also substantially hotter, in excess of 200 °C, implying that for a great part the exhumation is in slab melting conditions. The lower crustal sequence that at some point was at eclogite facies conditions thinned to just below a kilometre in thickness. In contrast to other high pressure terranes, Cabo Ortegal nappe as a whole shows evidences of intense widespread deformation and this is clearly shown in the various structures described in the gneisses. Given the relative high temperature environment in which the deformation occurred during the ascent though the crust, it is perhaps easy to envisage the amount of strain that must have been accommodated in the development of a widespread foliation in relation to slab stretching at high T. Our new approach in the Introduction and the Discussion has much to do with the suggestion by N Mancktelow to give the paper a topic of interest for a wider audience. Our intention is to provide constraints to a tectonic process based on available data in well documented natural rocks, as we believe is the case in Cabo Ortegal. To understand the sequence of structures described in Masanteo gneisses we found very useful to acknowledge the overall metamorphic record during the exhumation, bearing in mind that this set up is in which the main structures developed in Cabo Ortegal gneisses and neighbouring rocks. We particularly highlight the parallelism of trends in PT evolution between gneisses and neighbouring rock units, and their similarity in slope to present day active subduction zones to conclude that subduction was active during exhumation, as it has been shown in numerical models of subduction zones. Below we provide a more detail reply to specific comments by the reviewer. We feel strongly that the new version of the manuscript tackles the main points of criticism to the earlier version and that in consequence it has improved considerably. We hope you will find it suitable for publication in Solid Earth. Best regards,

General Comments: 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.

We provide additional reply to general comments made by the reviewer

Castiñeiras (2005) confirmed the 0.5 GPa difference in pressure between metasedimentary gneisses and migmatitic Qz-Fsp gneisses and critically assessed the possible errors involved in establishing this pressure difference. Since we consider his treatment correct we regarded not necessary to study further this point.

More detailed comments: Abstract line 10: it is difficult to envisage how “bulk flattening” can produce exhumation

We have rephrased the sentence, it is true flattening does produce exhumation. We have interpreted before that the flattening during the exhumation had to be related to slab stretching during its ascent.

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

We have rephrased the sentence, bulk flattening of the high-grade rock sequence was accommodated first by the development of the main foliation, then localized to a network of anastomosing shear bands that evolved to planar shear zones.

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

Qz-Fsp Migmatites develop initially under similar eclogitic conditions and then evolved to amphibolite facies conditions during exhumation.

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.

We say “Subsequent partial migmatization of the mafic granulites” so the upper part of the mafic granulites transformed in eclogites and occasionally, eclogitic blocks are intruded by mesocratic melts within the same age range 400-390 Ma (Fernández-Suárez et al., 2007).

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?

Yes. It is related to exhumation and decompression so “thought to form” is changed by “formed”.

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.

In the revised text we added these references (Gil-Ibarguchi et al., 1990; Fernández, 1997; Mendia 2000) in Page 3548 line 9.

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.

Locally, the structural relationships between the blastomylonitic S2-foliation and felsic dioritic/granodioritic dykes allow to constrain the relative timing of events in these gneisses. The felsic dykes are buckled by metric folds that are transposed by the S2-foliation (Figs. 4a and 4b), evidencing that intrusion and folding of the felsic dykes occurred before the D2 deformation. The S2-foliation shows parallelism with the migmatitic layering and bounds concordantly the eclogite blocks (Fig. 4c). Castiñeiras et al (2010) sampled one of these eclogitic boudins (sample COZ-4; Fig.2) and ob-

tained a U-Pb zircon age of 390±2 Ma. Field relations suggest that local anatexis and migmatization must have occurred during the early stages of D2, after eclogitization. Thus, the migmatitic Qz-Fsp gneisses apparently have recorded an early intrusive event related to the injection of the pre-D2 dioritic/granodioritic dykes in the gneisses, and a latter anatectic melting event that produced the migmatitic layering, which is preserved within the less deformed lozenges bodies surrounded by anastomosing D2-shear bands.

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

The felsic dykes have an age ca. 100 Ma older than the eclogites, accordingly with the relative age established in the sentence immediately above. Therefore, only the upper part of the mafic granulites was eclogitized. Field descriptions are consistent with the ages obtained in our work because S2-foliation transect the minor folds of the felsic dykes.

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.

Yes, it does, the felsic dykes are buckled by metric folds that are transposed by the S2-foliation (Figs. 4a and 4b), evidencing that intrusion and folding of the felsic dykes occurred before the D2 deformation.

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.

Yes, St still stable at 700 °C.

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?

It is true we don’t know the initial shape of the eclogite blocks, however by measuring the final shape and its orientation with kinematic indicators we can establish relations between different stages of structural development.

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.

“Poiseuille flow” is substituted by “non-lithostatic pressure gradient” in the revised text.

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

The early Variscan eclogite event re-equilibrated in amphibolite facies conditions in less than 10 Ma and it seems that only eclogitized the upper part of the mafic granulites and plagioclase still stable in granulites and felsic dykes.

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.

Since we cannot establish synchronous development of both structures, we have avoided this interpretation in the new version.

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? the + and – error bar of the P-T data are represented in Figure 5. Thermobarometry of the metasedimentary gneisses is reported in detail in the PhD work of Castiñeiras (2005). The other P-T data are

consistent with the obtained in the mafic granulites reported by Galán and Marcos (2000).

Changes in the manuscript: The Title is modified to: “The insights of high-grade deformation in Qz-Fsp gneisses during the early Variscan exhumation of the Cabo Ortegal nappe, NW of Iberia”.

The reference list is updated to include the new references in the discussion. Figures 5 and 16 have been modified, the first to include for reference PT trajectories in other subduction zones.

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