

## ***Interactive comment on “From subduction to collision in the Parautochthon and autochthon of the NW Variscan Iberian Massif” by Francisco J. Rubio Pascual et al.***

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Reply to the comments by Anonymous Referee #1 (28 March 2020)

We want to thank the work and constructive comments made by Anonymous Referee #1. We will try to reply here the general comments. Most of the suggested corrections in the RC #1 annotated PDF supplement are reasonable and will be accepted in a corrected version of the manuscript.

1) On the comment “without any single detail to explain the reinterpretation of previous [tectonic contacts] and mixing different criteria to define new Lower Allochthonous

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slices”:

We probably have not been clear enough in the text. Following point by point on the proposed examples:

a) About “...the Bragança complex Lower allochthon is enlarged by including part of Paraño Gp at the top of the Verín synform [...] the presence of alkaline/peralkaline gneiss appear to be the criteria to include those rocks in the allochthonous ensemble”:

We tried to explain in the text these characteristics as suitable criteria for a correlation that we consider indubitable:

- Cartographic continuation between the top of Verín and the lower structural levels (Basal Units for some authors) of the Centro-Trasmontan Domain (allochthon), including field evidence of continuity between the Centro-Trasmontan Thrust and the Fumaces Thrust. We report the presence of HP-LT rocks in the Basal Units of the Centro-Trasmontan Domain.

- Presence of albite-porphyroblast schists in the Verín Synform, which are similar to the most abundant (volumetrically) lithology that can be found in the Basal Units (HP-LT) of the NW allochthonous massifs, and mostly lacking in the rest of the allochthons.

- Presence of alkaline gneiss in the top of the Verín Synform, including not previously described riebeckite-bearing types, which are also exclusive for the Basal Units of the NW allochthonous massifs.

b) About “Meanwhile in the case of the red unit right below the Lalin-Forcarei thrust [the criterion] is the presence of albite porphyroblast with rare white mica inclusion”:

Referee #1 is right: the presence of albite-porphyroblast (not -clast) schists and the calculated PT conditions on them are here the single element to propose a possible correlative to the Basal Units of the allochthonous complexes.

c) The “confusing name: Uppermost Parautochthon/Lowermost Allochthon nappes”

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is deliberately employed just because both interpretations are possible depending on the preferred criteria to apply: parautochthonous lithostratigraphic similarities versus allochthonous tectono-metamorphic features. One example of this duality is what we have proposed for the Ramallal Phyllonites and some related rocks beneath Cabo Ortegal Complex, which are considered allochthonous by some authors or parautochthonous by others. Another good example is the lower structural levels of the Centro-Transmontan Domain (*sensu* Ribeiro, 1974), whose possible parautochthonous nature has been recently proposed (Dias da Silva et al., 2014a, b, 2016).

2) About the comment “The Figure 2 provides some zoom into the LFT area, but not real structural details beyond the unfortunate location of samples 1 and 2 on a NE-SW fault to the E of the Forcarei synform. Late offset have been mapped in the past in this area in connection with those faults: is it possible your samples (1&2) to be part of the Forcarei unit as the result of the offset of one of those faults?”

You are right about it. The work does not include detailed structural descriptions and data beyond localized stretching lineations with criteria of relative movement. It was out of our scope. In relation to the possibility of a NE-SW late fault repeating Forcarei materials, those levels of albite-porphyroblast schists have been always interpreted to belong to the Paraño Group. Moreover, you may consider that your proposal also would need some additional tectonic elements, such as some part of a klippe plus a N-S trending synform or a N-S trending fault that are not recognized.

3) With regard to the comment “In addition a strong sinistral strike-slip shear zone is widely visible in the western limb of the Forcarei synform as well as in the vicinity of the Beariz granite (Gonzalez Cuadra et al, 2006; Fernandez et al. 2011). Why are those data not incorporated into the discussion and interpretation?”

Yes, you are right. We have no data on kinematics west of Forcarei and even paid too much little attention to those works. We will correct it, thank you. About the content of your comment, Fernández et al., 2011 propose the development of a flat-lying S2

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foliation during D2 tangential tectonics, where stretching lineation L2 indicates early top-to-the-south thrusting preserved in its eastern limb (just as González Cuadra et al., 2006), but late top-to-the-north shear in its western limb. Thus, both limbs show sinistral criteria once uprighted by D3 folding, and our LE1 observations from the eastern limb of Forcarei could be equivalent to their late D2 stage ones. An early D2 top-to-the-south thrusting would correspond to our C2, but then, the Lalín-Forcarei Thrust which is top-to-the-E in Lalín (Martínez Catalán et al., 1996; 2002), would be (reactivated?) top-to-the-S in Forcarei. Another possibility is that top-to-the-S and top-to-the-N could be conjugate movements of our E1 stage, as proposed for the Redondela-Beariz Detachment in Díez Fernández et al. (2012b), and for Bande-Celanova Dome in this manuscript. This one is a more likely option, as top-to-the-south (sinistral) shearing in the eastern limb affects the Beariz granite as well (González Cuadra et al., 2006). Top-to-the-north kinematic criteria are also found by Gloaguen et al., 2014 (and references therein) to the east of Forcarei, where the authors relate both, stretching lineations (our E1) and upright folds (our C3), with a post-D2 (main thrusting) stage D3.

4) “On the other hand the authors show in the Fig.2 two different foliations and stretching lineations related to C1 and E1 stages. What criteria have been used to distinguish between them? There is neither description nor microstructural analysis to confirm it.”

Yes, you are right. C1 criteria for the Lalín-Forcarei Thrust are taken from Martínez Catalán et al., 1996; 2002 (quartz c-axis, asymmetrical boudins and S-C microstructures), and point to a top-to-the-east sense of thrusting for the Lalín-Forcarei Basal Unit in the Lalín Synform, related to early stages of collisional thickening. It is commonly accepted and we do agree with it. You are right and we must specify this in the figure caption, the text or both. However, our new observations in the “red unit” beneath Lalín point to a top-to-the-north shear sense, just similar to our data in eastern Forcarei and other areas. Our field criteria are based on asymmetrical biotite mica-fish, boudinaged quartz veins and drag-folds. We agree that this information was skipped from the text and we will correct it. Respecting to the discrimination between both

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sets of orientations, we have tried to make it clear in the manuscript that mainly top-to-the-north E1 deformation shows a subtractive character where there are expected stratigraphic markers that are (tectonically) missing (Bande-Celanova Dome) and it is sometimes (when deep enough) related to low-P, high-T metamorphic overprinting (Arnoia Detachment), so it very likely represents syn-collisional extension, as opposed to C1 thickening.

5) “Besides, it is clear that E1 lineations in the LFT area are parallel to the Carrio recumbent fold, so how do you know that those lineations are stretching/transport directions and not intersection lineations? Microstructural analysis of Fernandez et al 2011 points to a composite fabric (i.e. intersection lineations), so please explain those points.”

Well, that is not exactly that way. Fernández et al., 2011 points to two different lineations: an intersection lineation L3 related to their D3 upright folding (our C3), and the previous L2 stretching lineation that we have commented above. Both lineations usually show the same trend and are sometimes difficult to discriminate in the internal zones of the Iberian Massif, especially in sections affected by late, tight, upright folds such as Forcarei, where axial-plane schistosity may be very penetrative. Not the case in the Lalín Synform, which is relatively open. Anyway, the Carrio recumbent fold is above the LFT, that flattens and crosscuts its basal reverse limb, but our E1 lineations have been observed in Lalín from the LFT downwards. The asymmetrical shapes of deformed elements, showing their best asymmetry over sections parallel to the lineations, would also indicate that are not intersection lineations. Similar stretching lineations, which occur alongside top-to-the-north shear sense criteria, are common in other areas of the GTMZ (Marcos and Farias, 1999; Martínez Catalán et al., 2002; Gómez Barreiro et al., 2010; Fernández et al., 2011; Díez Fernández et al., 2012b; Gloaguen et al., 2014; this work).

6) “Similarly the definition of new tectonic boundaries like the Arnoia detachment and the Fumaces thrust although appealing they are not supported by the information pro-

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vided by the authors. It is critical to show some detailed map with structural data and showing, for example, the telescoped isograds. It is a very good contribution but needs to be supported by data. Similar aspects can be objected to in the case of the Fumaces thrust or the new interpretation of the LFT as an extensional detachment (data?).”

You are probably right. It means at least two new figures (Arnoia, Fumaces...) that we will try to include. Anyway, a previous sketch with the telescoped isograds (excluding kyanite) can be found in Barrera Morate et al. (1989). We will work on it, thanks. Respect to the LFT, we do not propose that it is an extensional detachment as you say, but there are kinematic criteria indicating a possible extensional top-to-the-north reactivation of the top-to-the-E thrust (see Fig. 2). The complex nature of the structure is also pointed out by the apparent sinistral indicators that you cite above about Lalín (and our observations). From your comment, we realize that our treatment of LFT in Fig. 2 is unfortunate, so we will change it. Thanks again.

7) “What is the basal thrust of the Bragança complex?”

We think this point is clear in the manuscript: the basal thrust of the Centro-Trasmontan Domain by Ribeiro (1974) (wonderful map), whose possible continuation into Verín we propose to be the Fumaces Thrust. Our work shows top-to-the-ENE criteria, probably corresponding to the same stage as the LFT.

8) “Overall the findings of the authors are important but need to be explained with more data, and discuss in depth previous contributions.”

We will try to add detailed maps of the Fumaces Thrust and the Arnoia Detachment. We will include the references that you cited above and some discussion about them in the terms of this reply.

9) “A special point is the definition of the Parautochthon sequence; it is confusing and need some reorganization, including a critical presentation of the differences Spanish and Portuguese parautochthon (there’s some up-to-date papers included in the

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Quesada&Oliveira 2019 book, for example).”

We are afraid that the definition of the Parautochthon sequence, as you ask for, is not simple. It is explained in the introduction that there are several different descriptions from geologically (and other reasons) disconnected sections. Correlation between them will be for long a matter of debate. We think that the two most representative and accepted subdivisions in Spain and Portugal (you can search both countries geological survey webs) are correctly exposed in the text, though it can be hard to follow for many readers. Even correlation just between this two is problematic. About the Quesada and Oliveira 2019 book, the corresponding chapter is a great review, but the number of up- to-date (e.g. post-2014) regional references cited is less than ours. Anyway we will check it, thank you.

10) “Is particularly worrying an ill-advised use of argument in several parts of the manuscript than must be corrected before to considering it for publishing. For example in the introductory part of the manuscript we can find several circular arguments (lines 78, 100), where data that are part of the results and discussion of the manuscript are included as part of the introduction. Please, do no mix up introduction (previous, published, consolidated scientific knowledge) with Results (new data presented in this paper) and Discussion (interpretation of the Results confronted or not to the scientific mainstream). The problem persists along the discussion where a simple presentation of a new contact is used as a demonstration of its existence (see Line 238). The abrupt end of the manuscript in a sort of condensed discussion-conclusion chapter does not help to clarify the doubts.”

You are probably right about taking out from the introduction both references in lines 78-79 and 100-102, and lines 238-242 from the tectono-metamorphic constraints. Thank you. About discussion & conclusions chapters, it is not a rare practice, but we will consider separating them for the final version as you suggest.

11) “I encourage the authors to review the manuscript and introduce solid arguments

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and scientific data to fully support their ideas, on the other hand very interesting, with the inclusion of more detail and a deeper discussion of the previous literature avoiding the use of circular reasoning.”

We agree on the interest of presenting more detailed maps, particularly from both new structures. Probably we will replace the equivalent old figures in the final version. We acknowledge very sincerely your suggestions. About more detail in the discussion, please note the regional amplitude of the work and the multidisciplinary matters to deal with from more than 70 regional references currently used here. Perhaps your idea is more appropriate for a larger format.

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