

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.

Anonymous Referee #1

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This manuscript presents new data and interpretation of the Parautochthon sections in NW Spain. In particular, the existence of HP-LT relics led the author to suggest that the uppermost parautochthonous nappe is indeed another nappe of the Lower Allochthon. Moreover some PT conditions are presented both for the Parautochthon and the Autochthon and new tectonic contacts are proposed. The work is worth published in this special paper after some important issues being solved. I have provided a commented PDF, with details and doubts.

At a first view the idea is appealing, but it is not surprising that some parts of the Lower Allochthon appear imbricated around the uppermost section of the parautochthonous

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pile. As cited by the authors, several contractional and extensional pulses overlap in the Iberian Massif and reactivation is somehow frequent. In this context the authors present three small maps without any single detail to explain the reinterpretation of previous and mixing different criteria to define new Lower Allochthonous slices. For example in the Figure 1 the Bragança complex Lower allochthon is enlarged by including part of Parano Gp at the top of the Verin synform, but in the Ordenes and Cabo Ortegal complexes this new Lower allochthon is classified out of the Allochthon with a confusing name: Uppermost Parautochthon/Lowermost Allochthon nappes. In the first case the presence of alkaline/peralkaline gneiss appear to be the criteria to include those rocks in the allochthonous ensemble. Meanwhile in the case of the red unit right below the Lalin-Forcarei thrust is the presence of albite porphyroclast with rare white mica inclusion. 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?

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? 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. 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.

Similarly the definition of new tectonic boundaries like the Arnoia detachment and the

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Fumaces thrust although appealing they are not supported by the information provided 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?). What is the basal thrust of the Bragança complex?

Overall the findings of the authors are important but need to be explained with more data, and discuss in depth previous contributions. 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 Quesada&Oliveira 2019 book, for example).

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 not 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.

I encourage the authors to review the manuscript and introduce solid arguments 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.

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Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2020-25/se-2020-25-RC1-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-25>, 2020.

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