

Dear Authors,

After having examined the revised manuscript and your response, I have noticed that some important issues have not been corrected and need further consideration before the acceptance of the manuscript. I will explain below the points:

### **1. Classification of Variscan Zones into autochthon or parautochthon - allochthon**

In your response you said:

*"Taking into account that the topic editor proposed us to follow one of two options (either subdividing the "autochthon" or the "allochthon-parautochthon"), we have added a subdivision of the "allochthons" in figure 1A. The topic editor did not indicate which "allochthon-parautochthon" had to be subdivided from those included in the figure (i.e., the Galicia-Trás-os-Montes Zone, the northern French Massif Central and the northern edges of Sardinia and Corsica) so we have subdivided all of them. The final result is, in our opinion, hardly readable. The reason why we added figures 1B-to-1D was to help any potential reader to find the samples reported in the paper, but the addition of more information in figure A gives rise to an extremely complex figure that is not necessarily useful."*

It seems to be a misinterpretation here. In my last and previous comments I clearly explained the need to include the variscan zones (all of them) either in "autochthon" or the "allochthon-parautochthon". In no case has it been requested a subdivision of the "allochthon-parautochthon", but a proper classification of the zones (all of them). In no case was the identification of the complexes, individual units or sub-domains requested, as in a confusing way it appears now in the new version. Considering the scale of the map, this would be a nonsense

Besides, in the last uploaded version, in the Fig. 1A, there are none of the requested changes. The authors maintain their categories: "Variscan autochthons-parautochthons" and "Variscan-allochthons" without any single explanation about the grouping criteria. In the caption it is said this figure is a modification from Pouplet et al. (2017), which indeed is a modification of Ballevre et al. (2009). There are new and updated representations of such a reconstruction (e.g. Ballevre et al. 2014). Some of them come from the authors of this paper, like the Fig 1 of Casas, J.M., Murphy, J.B. 2018. *Unfolding the arc: the use of pre-orogenic constraints to assess the evolution of the Variscan belt in Western Europe*. Tectonophysics 736, 47–61, where variscan zones' grouping criteria into parautochthon, autochthon and allochthon categories, are clearly defined and show scientific consistency.

I have reviewed again the manuscript text and there are not a single data, at its present form, which support a change in the scheme of variscan zones proposed by Ballevre et al. (2014), Casas and Murphy (2018), Martínez Catalán et al. (2019), Azor et al. (2019)... to name a few ones, so It has to be assumed the authors does not have any scientific evidence to support his new classification. As a consequence please modify the Fig 1A as requested in the previous revision.

Is nevertheless surprising that, instead of making the required changes, the authors have chosen to make a free and imaginative interpretation, adding more errors and confusion to the old figure 1A. I cannot see the purpose of incorporating in the Fig 1A, the name of, for example, the allochthonous complexes of the NW of Iberia, but at the same time leaving as part of the allochthonous, the schistose domain (parautochthon). In parallel, I cannot find any argument in the text supporting the inclusion of e.g. Essarts, Ile-de-Groix or Bois-de-Céné units (Armorican Massif) as part of the autochthon, having been classified as allochthon long time ago (e.g.

Ballevre et al. 2014 and references therein). Similar errors are found in the French Central Massif (e.g. Parautochthon Micaschits, etc).

What is the intention of the new addition in the Iberian Allochthon, the Verín-Bragança synform? Should we consider it a new allochthonous complex? Seeing those kind of things, I suspect that not all the co-authors have had access to this version of the article, because it represent a complete nonsense, equivalent to states that the Main Himalayan Thrust is a Neoproterozoic structure.

The authors have to have clear that the problem of the Fig 1A is not, *the addition of more information*, but the number of errors it has and the absence of arguments supporting their new ideas.

## 2. Cambro-ordovician Lithostratigraphy

A) *"The second question is focused on the (litho-)stratigraphic units previously published exclusively in the Salamanca area of the Central Iberian Zone. As already explained in a former response, we cannot understand why the topic editor requests us to cite some previous works related only to his area of work and ignores other similar reports proposed for other areas included in our work."*

It seems obvious that a request has been made to correct what is lacking. What is correct, or what the reviewers have already pointed out, is not included in my report.

*What are about other stratigraphic units that, like the Salamanca area, which are not reported in this paper because we selected no samples from there? No interest about them..."*

Again the problem here is not a personal interest. The authors are trying to make large-scale correlation based on previous and new datasets. However, the criteria for using some data (particularly stratigraphic) and not others are not clear. In the answer now it seems that they are only the data of the samples directly obtained for the study. This is not strictly true since they use previous results and data from the literature. In this line, for example, the information published about the Salamanca sector was not selected, but at no point in the text do the authors indicate the criteria they followed in the selection. In the replies the authors give some reasons but never include them in the manuscript.

In connection with this issue, it is surprising what the authors say in their response:

*"This paper offers no lithostratigraphic revision of the Central Iberian Zone; this should be obvious for any reader"*

And in the caption of the Fig 2 authors said: *"Stratigraphic comparison of the Cambro-Ordovician successions from the Central Iberian Zone [...]"*.

The text and the figure 2 invite the reader to find a correlation of the cambro-ordovician successions across the orogenic zones as a whole (CIZ, GTMZ...). But what he finds is a correlation of certain parts of the zones, taken as a whole. I have no problem with showing a correlation between the specifically sampled zones, but it must be clearly explained in the figure and/or its caption. This can be done by indicating which parts of the existing information in an area have been omitted and why (my proposal), or by indicating that the correlation is made only among the areas sampled in this work, and therefore does not represent a correlation with all the existing data.

Besides, the comment introduced in the Fig 2 caption: "*the northern Central Iberian Zone, in the vicinity of Salamanca, is not included here (Díez Balda et al., 1990)*" is not correct, because the Diez Balda subdivision applied not only in the Salamanca city, but to the province.

**B)** *"However, we disagree in citing Valladares et al.'s works. Our position is not related to any animosity against the author, Prof. Isabel Valladares, whom we know well. Our refusal is based on a conceptual misleading that was explained in a previous response to the topic editor, but we have no troubles to repeat it."*

*In her PhD thesis and following papers, Isabel Valladares defined FACIES ASSOCIATIONS (labelled I to XV), not FORMATIONS, and she suggested some broad correlations with previous stratigraphic units. As you know (or you should), a facies association (characterized by an assemblage of facies and useful for environmental interpretations) and a lithostratigraphic unit (characterized by its lithology and useful for mapping) are two completely different things. Therefore, we disagree with your insistence on citing works that describe facies associations as if they were lithostratigraphic formations and thus hampered any stratigraphic correlation. This sounds a serious misconception of geological concepts."*

I sincerely appreciate the explanation about facies and lithostratigraphic units, but again, the problem here is not a "conceptual misleading", but the absence of clear criteria to include some data and refuse others. It seems like the reader has to blindly believe on authors' personal selection. In the response is clear that the authors have a reason for not to use Valladares' works, but refuse to explain them in the main text. Beside, it is important to note that the authors in the Figure 2 caption said:

*"Stratigraphic comparison of the Cambro-Ordovician successions [...]"*

It might be obvious for the authors that "Stratigraphic... successions" means "only lithostratigraphic units", but, should not be more precise to say "Comparison of the Cambro-Ordovician lithostratigraphy...? It would suddenly clear up the confusion that for an interested reader of a wide-audience journal like Solid Earth could generate.

At this point the affair is still up:

1. - The authors have to complete the required changes in Figure 1A and make sure to include all the variscan zones either in the autochthon or in the allochthon-parautochthon. I suggest following one of the co-authors reconstruction (Fig. 1 Casas & Murphy, 2018) or, e.g. Martínez Catalán et al (2019), see references at the end of this letter.
2. - Please remove the names added in the last version of the Fig. 1A: OC, MC, COC, BZ, Mv, Gu, Br, Ma, Mo, VC, IZ and particularly the "psychedelic" Verín-Bragança syncline (VBS) allochthon.
3. Consider to change in Fig 2 caption "Stratigraphic... successions" by formal stratigraphic definitions like "Cambro-Ordovician lithostratigraphy". In the same caption change "*the northern Central Iberian Zone, in the vicinity of Salamanca...*" by "*the northern Central Iberian Zone, in Salamanca...*"
4. Clarify whether in the main text or in the caption of Fig 2 the criteria for stratigraphic data selection. This might be something like "lithostratigraphic data used in the correlation was restricted to the sampled areas".

I am therefore returning the manuscript with a request of minor revision.

Sincerely,

Juan Gómez Barreiro

#### References:

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Azor, A., da Silva, I.D., Barreiro, J.G., González-Clavijo, E., Catalán, J.M., Simancas, J.F., Poyatos, D.M., Pérez-Cáceres, I., Lodeiro, F.G., Expósito, I. and Casas, J.M., 2019. Deformation and Structure. In *The geology of Iberia: A geodynamic approach* (pp. 307-348). Springer, Cham.

Martínez Catalán, J.R., Collett, S., Schulmann, K., Aleksandrowski, P., and Mazur S., 2019 "Correlation of allochthonous terranes and major tectonostratigraphic domains between NW Iberia and the Bohemian Massif, European Variscan belt." *International Journal of Earth Sciences*: 1-27.