TEKTONIKA

Review Report

Gremmel et al., Constrictional Flow and Strain Partitioning During Oblique Deformation: Insights From the Variscan Tanneron Massif, SE France, TEKTONIKA, 2024.

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1st Round of Revisions

Decision Letter

We have reached a decision regarding your submission to ТЕКТОНІКА, "Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France".

Our decision is to: Resubmit for Review

This decision was reached based on to thorough and constructive reviews, which both conclude that this work is an important contribution, but moderate edits are required. We encourage you to carefully address all of their comments, documenting all changes made, and we look forward to reviewing a revised version soon.

Comments by Reviewer 1

The paper “Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France” by J. Gremmel, G. Duclaux, M. Corsini and J. Bascouby provides very interesting data about Tanneron Massif of Southern France. This area is characterized by the occurrence of the metamorphic rocks belonging to the Variscan belt. The authors provide a detailed structural analysis through precise digital mapping combined with microstructural and quantitative finite strain data of this area in order to constrain the deformation history of the Tanneron Massif. The paper is well-written. The provided data are accurate that is very important for a full understanding of the paper. The interpretations as well as the conclusions are coherent with the data reported in this paper. The references and the figures are adequate but must be improved. I think that this contribution is very important to understand the history of the Variscan Belt.

However, the paper requires some moderate improvements that are listed hereafter. See also the comments in the annotated text.

1) I suggest to replace the term Sv and Sh. You described a geometrical crosscut relationship with Sh cutting the Sv under different metamorphic conditions. So why do not use S1 and S2 foliation? or, alternatively, if you consider a progressive deformation, S1a and S1b.2) in the Geological setting and Discussion sections, the authors must insert more references. They often conclude or interpret structures without inserting reference citations. For example, in the discussions (Pag. 12), they write that there are two different models to explain the rheological-dependent finite strain shape variation without inserting citations or providing natural examples described in the literature.

3) Section 3.2. The authors cannot cite fig. 7 before figures 3, 4, 5 and 6.

4) the authors described “complex foliation trajectories with a high variability of trending directions”. They must consider that the direction of planes dipping less than 20° can change a lot but this large variability is not representative since a horizontal plane has no direction.

5) the description of the late folding event must be more detailed. Authors must insert more geometrical constraints (trend of axis, strike, and dip of axial plane)

6) The linear fabrics section must be improved. Authors used the mineral and stretching lineation to make their reconstruction but they never wrote what mineral or “object” mark the lineation. If the foliation developed under different metamorphic conditions, the minerals that mark the stretching lineation could be different. In addition, they have to describe more accurately the relation between the lineation and the two foliations. In other work, they must clearly state if the lineation was measured on the Sv foliation or on the Sh. It is not clear at all if the different strain fabric is related to Sh or Sv domain. For the authors it is clear but for the reader, it is not. Discriminate it, it is fundamental for the reader to follow the discussion and understand the final conclusions.

7) Also the microstructures section must be improved. The authors describe the fabric in orthogneiss, paragneiss, and migmatite. However it is crucial to discriminate the microstructure in Sh and Sv domains. The described microstructures are typical of what fabric? Determine it, it is fundamental to discriminate the deformation temperature for both fabrics.

8) In the abstract is reported that the L>S is the first stage of late Variscan transtensive deformation and was followed by S-L tectonics associated with sub-vertical foliation. Then they said that the two textures (L>S and S-L) are rheological controlled by lithology. If the two textures are documented in the two lithotypes (meta-igneous and metà-sedimentary), why do the authors consider them as two steps of deformation and not a simultaneous and heterogeneous repartition of deformation? In the abstract they wrote about progressive deformation and phases of deformation…it is a little bit confusing: is it a progressive deformation or not?

Figures.See also comments in the attached file.

1) In fig. 1, insert a small France showing the Variscan belt and the location of the MTM.

2) I suggest increasing the size of the symbols used in the maps of Figure 1b and 3. When adapted to the final figure size, the symbols in the map (Fig. 1b, 3 and in the legend of Fig. 1b), and the text in Fig. 1a will be impossible to read.

3) I had a problem in reading the map of foliation (Fig. 1) and lineation (Fig. 6) and understanding the relationship between these two major structural elements.

- The foliation represented in Figure 1b, is Sh or Sv? You use different colors to discriminate different foliation dips. I found it much more useful to use different colors to discriminate

different generations of foliation and numbers or different symbols to discriminate different dips.

-The lineation represented in Fig. 6 is a measure of which foliation? It is impossible to understand from the map. Does it is developed on the Sh foliation? You must specify. You put too many lineation symbols in the map, it is impossible to discriminate the different colors if the symbols are superimposed.

4) Insert in the map of Figure 7 the samples Tr1 and Tr2.

5) In figure 11, if in step 2, the main deformation produced S-L tectonites, why in the 3D block diagram represent Phase 2, there are strongly elongated objects like those represented in the block diagram of Step 1?

6) I also suggest to discriminate in the figure and clearly indicating in Table 1, which sample is representative of fabric Sh or Sv. Without this differentiation, it is very difficult to understand your discussion and conclusion.

The main merit of the paper is that to provide a full and detailed structural analyses through precise digital mapping combined with microstructural and quantitative finite strain data of this area in order to constrain the deformation history of the Tanneron Massif. The data presented by the authors cover an area of about 25 km2 where the density of the collected structural data is very high. The integration of all the provided data is another impressive feature of this paper. The conclusion seem to be fully supported by the collected data.

Some description must be improved to make more easy the understanding of the readers. The presentation of the data, as for instance the meaning between the Foliation Sh and Sv required more dteails. In the Geological setting and Discussion sections, the authors must insert more references. Some figures can be easily made more effective.

In the abstract is reported that the L>S is the first stage of late Variscan transtensive deformation and was followed by S-L tectonics associated with sub-vertical foliation. Then they said that the two textures (L>S and S-L) are rheological controlled by lithology. If the two textures are documented in the two lithotypes (meta-igneous and metà-sedimentary), why do the authors consider them as two steps of deformation and not a simultaneous and heterogeneous repartition of deformation? In the abstract they wrote about progressive deformation and phases of deformation…it is a little bit confusing: is it a progressive deformation or not?

The last part of Introduction can be reduced. It seems like the method section.

The data and the methods are the strong points of the paper

In the results some conclusions are provided in advance. So, some sentences must be moved in the discussion

Comments by Reviewer 2

This paper aims at characterizing and quantifying the finite strain experienced by a segment of the Variscan belt of Western Europe (Tanneron Massif in the SE part of France) during its late stage of evolution using field mapping, structural geology, and Anisotropy of Magnetic Susceptibility (AMS). They particularly focused their attention on the eastern part of the Tanneron massif that, characterized by a huge amount of migmatitic paragneisses and orthogneisses, granites and pegmatites, is considered to represent the internal domain of the belt. They also look at low-grade metamorphic rocks from the eastern domain that highlights the presence of N-S trending intramontane basins filled by products of erosion of the hinterland. These Late Carboniferous basins are parallel to major N-S oriented shear zones identified by previous studies.

At the regional scale, structural results allow to identify two main foliations: 1. A near horizontal foliation affecting both, the western and eastern parts of the study area, which for the latter one, is particularly well developed in the migmatitic orthogneiss. Although most foliations are near horizontal or shallowly dipping to the north, they note that they are more heterogeneous to the east in the “Super Cannes” area, displaying triple point junctions and concentric structures. 2. A sub vertical foliation observed at the regional scale, but which is well observed in the (migmatitic) paragneisses of the Western domain.

In agreement with previous studies, N-S trending lineations are constant at the regional scale and plunge shallowly to moderately to the south in the western domain, and shallowly to the north in the eastern domain.

Also, the analysis of the finite strain ellipsoids indicates contrasting style and magnitude of strain with a well pronounced flattened shape fabric (S-Tectonites) in the extreme east of the study area (“Super Cannes”) which contrasts the stretched shape fabric (L-Tectonites) identified further west in the orthogneiss of this eastern domain in the “Croix des Gardes” and “Roquette”. Based on the high-grade metamorphic nature of the “Super Cannes” rocks, the authors propose that these S-Tectonites could reflect the roof of domes structures that developed as a result of internal buoyancy-driven flow rather than tectonic forces. In contrast, the L-Tectonites associated with near vertical foliations are interpreted in term of tectonic forces products.

Plane strain fabrics are widespread at the scale of the study area (western and eastern domains) and very often associated with paragneiss showing a sub-vertical foliation and/or N-S trending shear zones.

By comparing their results with previous studies, the authors propose that these contrasting fabrics involving constrictional strain and flattening, as well as N-S trending strike-slip shear zones, developed in a transtensional regime during the thinning of the Variscan continental crust at ca. 320-300 Ma. The authors state that this oblique tectonic regime supports the pull-apart shape of Late Carboniferous basins that formed in the upper part of the crust.

In the end, the authors propose a continuous tectonic model with a first phase dominated by constrictional strain associated with high-temperature subhorizontal flow, followed by plane strain that led to the development of vertical foliations and near horizontal lineations in medium temperature conditions. The dextral strike-slip tectonic regime remained active until the opening of pull-apart basins.

This work represents an important contribution for the better understanding of oblique tectonic regimes affecting rocks in the middle and lower crust and deserves to be published in Tektonika.

However, in its present state, the paper is too long, contains some repetitions making it difficult to follow and understand.

Below are my major concerns:

AMS Data: To complement the structural work, the authors have conducted AMS measurements on 5 sites only. How and why, these specific sites have been chosen? Except that these few AMS data are generally in good agreement with structural data, I am wondering if it is worth to include them in this article, especially that the correlation between finite strain ellipsoids and AMS shape remains poor. What do we learn from them? Also, can the authors link the AMS results with microstructures?

Moreover, these magnetic foliations and lineations and associated stereo should appear on a map (No map on Fig. 9)?

To make it short, I would suggest to delete this AMS part.

Microstructural study: The authors looked at some microstructures to show that a (deformation) temperature gradient exists from east to west. However, the objectives of this microstructural study are not clearly stated.

It makes sense that a migmatitic rock that got “freezed” at depth shows microstructures highlighting a deformation temperature above 550°C with absence of chlorite …and that a similar migmatite affected by an high strain zone displays different episodes including low temperature deformation. Sorry but it is unclear to me where you want to go with this. Do these rocks/structures highlight a different crustal level rather than an E-W cooling gradient?

Also, it would have been good to mention the orientation and name of the studied samples and their locations on a map in order to make the link with the finite strain ellipsoid. As an example, were you able to correlate elongated minerals / directions with constrictional fabrics (can see some photo on Figure 5 but this should be make it big)? As you did for AMS, I have the feeling that this microstructural study was not conducted in-depth. I am aware that it would have required more work but think that you could have extract more information from them to support your model.

Tectonic model: The finite strain analysis allowed you to define flattened shape fabrics in the Super Cannes area that you interpreted in term of dome roofs. When do these domes emplaced in your model; during stage1?

Can you correlate these 2 main stages with tectonic events that occurred in different parts of the Variscan belt of western Europe (other than Corsica and Sardinia)? In the paragraph 5.2 you have tried to correlate and discuss part of your results with other studies conducted elsewhere but did not go really further…

It would have been good to integrate and discuss your model in the scope of the Variscan geology affecting western Europe. It would render your model more substantial.

A table including sampling sites with structural data should be added ideally with those from previous authors.

Some references are missing.

Summary: In its present state this manuscript is too long and very sprawling. It should be re written in a more concise way, better structured and focus on key information that could help to support the final model. Figures (see below specific remarks) need to be improved and a table added.

I am happy to look at a new version if needed and remain at the disposal of the authors if they want additional feedback.

I have included some in-text comments in the attached files, but have mentioned below some others regarding the figures.

Figure 1:

- Plan de la Tour and Reyran should appear on the map to indicate the Late Carboniferous basins.
- Ductile shear zones should clearly appear on the map. As an example, there is one to the east of the Tanneron village that structured the Orthogneiss (see MSc thesis Gébelin, 1999). These shear zones bounding units such as orthogneiss should also be seen on the cross-section. The orthogneisses should extend further down and not appear as a square/rectangle shape! Only gravity would allow you to draw them such a way if it was the case.
- Add a scale / depth on the cross-section.
- Samples locations + their names from this study should be observed on the map in addition to those from previous authors (e.g. Crevola…)

Figure 2:

- Sample/outcrop locations should be added on each photo. I can see some GPS coordinates (ok in a table) but a name would be more useful so to directly correlate the photo with a location on the map (Fig. 1).

Figure 3:

- Are the foliation trajectories only from the authors' data?
- I think that a mistake has been made in the caption: Sv overprints Sh and not the other way around from what I could read in the manuscript.

Figure 4:

- Same comment than Figure 2.
- Remove the arrow next to the N (if the north is to the right, just put a N on the right hand side).

Figure 5:

- Good to see some observations supporting cigare-shape or pancake-shape ellipsoids at the thin section scale. However, this should be clearly stated on the manuscript.

- Again, please add the name of sample locations.

Figure 6:

In the case AMS data remain in the manuscript, you need to add the magnetic foliations and magnetic lineations on this map. Also, difficult to differentiate your work from the work done by previous authors (e.g. Crevola…).

Figure 8: A real shame not to be able to locate the samples on a map.

Figure 10: Simplistic (see my comment above). Same, not sample names…these photos could come from someplace else.

Figure 11: Some additional info should be added:

– Pancake shape ellipsoid

– Geological time period

– orientation- some key sample locations.

– shear zones (we can see the kinematics but not clearly associated to shear-zones). How do you exhume high-grade rocks from phase 1 to phase 2?

Table 1: ok but you should include a table with your sample locations and structural measurements + those perhaps from previous authors.

Authors' Reply to Reviewer 1

In addition to the changes related to the reviewers' comments, we have also taken the initiative of adding 3D scans corresponding to some of the outcrop photos shown in Figures 4 and 5 to provide more complete views of structural elements (the scans can be viewed online via a web link).

RESPONSE TO REVIEWER 1's COMMENT

General comments:

1) I suggest to replace the term Sv and Sh. You described a geometrical crosscut relationship with Sh cutting the Sv under different metamorphic conditions. So why do not use S1 and S2 foliation? or, alternatively, if you consider a progressive deformation, S1a and S1b.

-> We thank reviewer 1 for this relevant suggestion and we have modified our description of planar fabrics throughout the text end figure to clarify this point. We changed Sh foliation to S2a and Sv to S2b because we consider a progressive deformation event. We have chosen to identify it as an "S2" foliation instead of "S1" because of the presence of folds in the S2a foliation (with axial plane parallel to the foliation) meaning that a former fabric was already present and folded.

2) in the Geological setting and Discussion sections, the authors must insert more references. They often conclude or interpret structures without inserting reference citations. For example, in the discussions (Pag. 12), they write that there are two different models to explain the rheological-dependent finite strain shape variation without inserting citations or providing natural examples described in the literature.

-> We would like to thank reviewer 1 for pointing this out and we agree that it required some clarification. We have answered each "in-text" comments related to this concern (see section below) and added references when necessary. In the last part of the geological setting we describe the geology of the study area based on our own observations, this is why there are only few references. And for the "Discussion" section, a number of suggested interpretations are from our own ideas (as the example given in Page 12) and do not represent previously published models, explaining the small number of references there. We have modified the relevant sentences to make it clearer.

3) Section 3.2. The authors cannot cite fig. 7 before figures 3, 4, 5 and 6.

-> We corrected this.

4) the authors described "complex foliation trajectories with a high variability of trending directions". They must consider that the direction of planes dipping less than 20° can change a lot but this large variability is not representative since a horizontal plane has no direction.

-> We do agree with reviewer 1 that gently dipping foliations (0-20°) in such migmatites can change very rapidly. In the Super-Cannes hill (where we describe "complex foliation trajectories…") the foliations dip range between 0-40°, and also there is a ill-defined NW-

SE trend visible in Fig. 3 map, local domal shape structures generate deviations of the gently dipping foliation at the origin of the high variability of trending directions which appears to be very significant and must be considered. In comparison to Super Cannes Hill, other parts of the study area (Fig. 3) with foliations dipping with the same order of magnitude (0-40°) have a clear and constant E-W direction and do not show these complex foliation patterns supporting the significantly different structure in the Super-Cannes and is not due to overinterpretation of subhorizontal dips. We modified the text accordingly in section 4.1.

5) the description of the late folding event must be more detailed. Authors must insert more geometrical constraints (trend of axis, strike, and dip of axial plane)

-> Following the related “in-text” comments, we have modified this part and brought more details on these folds as suggested by reviewer 1.

6) The linear fabrics section must be improved. Authors used the mineral and stretching lineation to make their reconstruction but they never wrote what mineral or “object” mark the lineation. If the foliation developed under different metamorphic conditions, the minerals that mark the stretching lineation could be different. In addition, they have to describe more accurately the relation between the lineation and the two foliations. In other work, they must clearly state if the lineation was measured on the Sv foliation or on the Sh. It is not clear at all if the different strain fabric is related to Sh or Sv domain. For the authors it is clear but for the reader, it is not. Discriminate it, it is fundamental for the reader to follow the discussion and understand the final conclusions.

-> We thank reviewer 1 for these numerous sound suggestions. All these considerations were taken into account following the “in text” comments provided. We have specified what type of object marks the lineation whenever we describe it. We also added sentences to precise the relationship between lineation and foliation (S2a-b) and named this lineation “L2” to make things clearer. In fact, the lineations are very homogenous in the whole study area and are similar on both Sh and Sv foliation planes (now S2a and S2b).

7) Also the microstructures section must be improved. The authors describe the fabric in orthogneiss, paragneiss, and migmatite. However it is crucial to discriminate the microstructure in Sh and Sv domains. The described microstructures are typical of what fabric? Determine it, it is fundamental to discriminate the deformation temperature for both fabrics.

-> We understand reviewer 1 comment and reworked this section to clarify our point. We described the microstructures following the different lithologies rather than foliation types because the deformation temperature evolves mainly between lithologies. We specified in the section “planar fabric” that S2a (Sh) are mostly developed in the Cannes migmatite and orthogneiss units while S2b (Sv) foliations are more developed in the paragneiss unit. We have added details to reiterate this point in the section on microstructures. We have also shifted this section from 4.3 to 4.5 as we believe it fits better at this location in the updated manuscript.

8) In the abstract is reported that the L>S is the first stage of late Variscan transtensive deformation and was followed by S-L tectonics associated with sub-vertical foliation. Then they said that the two textures (L>S and S-L) are rheological controlled by lithology.

If the two textures are documented in the two lithotypes (meta-igneous and metà-sedimentary), why do the authors consider them as two steps of deformation and not a simultaneous and heterogeneous repartition of deformation? In the abstract they wrote about progressive deformation and phases of deformation…it is a little bit confusing: is it a progressive deformation or not?

-> We would like to thank reviewer 1 for pointing out the lack of clarity on this issue that required our attention. The abstract resumes this study's main results and their interpretations, the principal one being the proposed transtension model divided in two phases progressing during the same event (i.e. progressive deformation). These phases are increments of deformation or "intermediate" phases during a single event (in a general transtensional regime) but have not to be considered as two separate events with different regional tectonic forces. We precised this point in several sentences to clarify the message throughout the discussion.

->In the order of the article, we first describe two sets of planar fabric with the S2a (Sh) and S2b (Sv) foliation specifying that S2a are more developed in the meta-igneous lithologies and is somewhere overprinted by S2b foliations which are more developed in meta-sedimentary lithologies. Then, we describe that deformation temperatures are higher for meta-igneous lithologies (and thus for S2a foliations) than for meta-sedimentary lithologies (and thus S2b foliations). And finally the finite strain section shows that L>S tectonites belong to meta-igneous lithologies (with S2a foliation) while S-L tectonites are developed in meta-sedimentary ones (with S2b foliation). With all these results, in the first section of the discussion (5.1), we discuss the two solutions you mention : *"There are two ways to explain this rheology-dependent finite strain shape variation: strain path partitioning with each lithology accommodating a single deformation phase differently, or each lithology recording two separate deformation phases."* So to sum up, one step vs 2 steps of deformation.

And then we argue for the 2 phases solution (2 phases during the same event and not 2 stages which mean 2 different events) because the deformation temperature and the overprinting relation (between S2a and S2b) highlight a chronology with a first high-temperature deformation (S2a+L>S tectonites) progressively evolving to a medium to low-temperature deformation (S2b+S-L tectonites). (line 582-591).

In the last section of the discussion "*5.4 Progressive deformation and strain partitioning during exhumation*", we summarise all evidences (line 791-805) and detail our model of a progressive deformation evolving in two phases during exhumation. It is a progressive deformation because the two phases are included in the common tectonic framework of a transtensional regime. (Line 804 "*These observations suggest a two-phases progressive deformation during the retrograde metamorphic evolution associated with the collapse of the belt*".) But during the second phase, there is indeed a local strain partitioning/strain localisation specific to these phase because: "*Deformation is partitioned into the paragneiss unit, enveloping ortho-derived lithologies that preserve their stretched shape from the first phase and are generally not or less overprinted*".

in text comments :

Line 24 (first manuscript release): ".... L>S tectonites associated to....."

-> Done

Line 28: “.... partly....”

-> Done

Line 68 “This part of paragraph can be reduced.It seems like the method section.”

->The entire paragraph has been reduced and combined with the previous one.

Line 75: “This sentence doesn't work. Rewrite it.”

-> Done

Line 93 “These authors don't describe a gradient increasing toward E. Please check.”

-> This is our mistake, the reference has been replaced.

Line 103 “Oblique deformation and transpressive event are used to indicate the same tectonic event. It is better to use everywhere the same definition.”

-> Oblique deformation includes both transpressional and transtensionnal regime. We thoughtfully decided to use the term oblique deformation early in the manuscript as at the scale of the MECS variable examples of either late-stage transpression or late-stage transtension have been proposed.

Line 106 “Giacomini et al. describe a little bit different evolution. At 320 ma K melts and dextral shear zones that coupled garnulites to gneisse and at 312-308 HT-LP event related to post-Variscan granitoids emplacement. Check it.”

-> This sentence has been modified.

Line 111 “These sediments are folded during the opening of the pull-apart basins or after?”

-> Good question. There are only a few available studies on the basin’s structure except a recent MSc report that we advised (Maillet, 2021, added in the text), and few paragraph in a PhD thesis (Morillon, 1997, not available online), as well as in Toutin-Morin et al. (1994) . This latter study describes ante-Permian faulting and folding in the basin contemporaneous to postdating sedimentation. We completed our description of the basin’s structure with a sentence referring to Toutin-Morin et al. work.

line 122 “In this paragraph there are very few references. It is possible to add more?”

-> This part presents the geology of the study area by our own observations from field mapping, this is why there are only few references. Moreover, the few references cited are the only published data available, there has been very little different mapping work in this area.

Line 176 “Delete ...horizontal and vertical…”

-> Done

Line 179 “Better coeval”

-> Done

Line 193 “Fig. 7 is cited before the figures 3,4 and 5. They must cited before.”

-> Corrected

Line 243 “add ...dominant.....”

-> Done

Line 245 “Trending direction? Rewrite it.”

-> Done

Line 246 “How do you discriminate lineation to Sh and to Sv. Please explain it.”

-> See comments below in the “linear fabrics” section.

Line 248 “I understand the Sv cuts Sh. It is? If it is, why the authors don't use S1 and S2? Please explain.”

-> Yes foliation S2b overprint foliation S2a, we changed “Sh-Sv” to “S2a-b”, see general comment above.

Line 261 “This statement must be reported in the data section and not here.”

-> The sentence has been modified following Reviewer 1 suggestion.

Line 270 “Change in .....(Fig. 4E,F). Sinistral indicators.....”.

-> Done

Line 273 “The paper by Carosi et al 2012 is focused on the Sardinia.”

-> Reviewer 1 is correct. The sentence has been modified to enlarge it to the Maures Tanneron + Sardinia.

Line 279 “I understand that the folding event is after Sv foliation. It is that so?”

-> No, all described folds are coeval with Sh/Sv, now S2a and S2b foliations, we have modified the sentence to clarify this point.

Line 285 “Please add the trend of the axial planes as well as the trend of the axes of the folds.”

-> As we write “Axial planes are mainly parallel to the surrounding foliation.” and fold axes “being parallel to the local stretching lineation” therefore their trend changes depending on foliations and lineations domain (S2a/S2b foliation). We changed the sentence to add more precision on these measured variations.

Line 294 “A description of the lineations must be added. What are they are represented by? Please specify.”

-> A more detailed description of the lineation has been added.

Line 299 “This lineation is associate to Sh or Sv foliation? Please discuss this point.”

-> Lineations are associated to S2a and S2b foliations without differentiating two sets of lineation; the lineation pattern is homogenous in the whole area. A sentence has been added to clarify this point.

Line 304 “This statement must put in the discussion, not here.”

-> We followed reviewer 1 suggestion and the interpretation of lineations’ general orientation in terms of sub-horizontal crustal flow is now only presented in the discussion rather than in the result section.

Line 307 “Please, explain better”

-> The sentence has been modified.

Line 311 “I don't understand this statement. Please explain better.”

-> The sentence has been modified.

Line 317 “... are used to constrain the deformation temperature....”

-> Done

Line 325 “Please rewrite. Something is missing.”

-> The sentence has been modified.

Line 337 “Move in the discussion”

-> The sentence has been modified.

Line 343 “Biotite strongly replaced by chlorite is rarely documented in the orthogneisses.”

-> This sentence does not refer to the previous work in the bibliography, the observation of biotite replaced by chlorite is from our own samples. We keep the sentence.

Line 366 “derived from”

-> Done

Line 370 “Related to which foliation? The foliations are described separately and must be discussed separately also for the deformation temperarture.”

-> Here the deformation temperature descriptions are separated following the different lithological units as we discussed previously because changes in the microstructures are more representative between different lithologies. As we mentioned earlier in the “Planar fabric” section (and illustrated by Fig. 3) S2a foliations are more developed in the Cannes migmatite and orthogneiss units while S2b foliations are more developed in the paragneiss unit. We added a sentence and several precisions in this section to remind the relation between lithologies and S2a-b foliation.

Line 404 “ In this part there is a mixing between data and interpretations. They must be separated. Move the interpretations in the discussion.”

-> We do not agree with the comment of reviewer 1 and believe that this section only describes the results obtained from the analysis of finite deformation and their graphical representation in a Flinn diagram. In a Flinn diagram, the different fields of the diagram are represented by "flattening- plane strain -constriction" or equally by the associated tectonite type "S>L - SL - L>S", which are both descriptive terminologies and do not represent interpretations.

Line 421 “Please check these values. In fig. 9 seem different.”

-> We have double checked the values which are in line between the text and the figure.

Line 471 “insert reference” “insert reference”

-> The two proposed models to explain the “rheology-dependent finite strain shape variation” represent our own ideas and not published models. We have modified the sentence to clarify this point.

Line 556 “Please rephrase”

-> Done

Line 568 “This sentence in unclear. Please rephrase”

-> Done

Line 573 “Please explain the meaning of ...different types of folds…”

-> We refer to all the possible variations of fold geometry with upright-inclined-recumbent folds, symmetrical or not fold, cylindrical or not… We have modified the sentence with “different geometries”.

Line 575 “Insert references”

-> Done

Line 641 “Very long paragraph. Can be reduced?”

-> We believe this paragraph is the most significant in the discussion, taking up all the evidence developed previously that led us to propose our final model and then detailing this model of a progressive two-phases deformation based on the final figure. We have tried to find a way of reducing it, but unfortunately we feel that all the information in this paragraph is essential and we prefer not to reduce it.

Figures.See also comments in the attached file.

1) In fig. 1, insert a small France showing the Variscan belt and the location of the MTM.

->We have added an insert with the location of the MTM in the context of European Variscan massifs.

What is the meaning of this dark line?

->The dark line mentioned represents a fault, which is the major fault bounding the Reyran Carboniferous basin to the East.

2) I suggest increasing the size of the symbols used in the maps of Figure 1b and 3. When adapted to the final figure size, the symbols in the map (Fig. 1b, 3 and in the legend of Fig. 1b), and the text in Fig. 1a will be impossible to read.

->The size of the symbols and the text have been increased. The size of the text follows the journal guidelines (“nothing smaller than 6pt”).

Why all foliation and not Sv-Sh as in the other areas?

->Because in the Super-Cannes area there is almost only S2a foliations so we had simplified by grouping all foliations in one stereogram. Now, we have changed to a stereogram with only Sh (now S2a) foliation and named it “S2a” to make it clearer.

3) I had a problem in reading the map of foliation (Fig. 1) and lineation (Fig. 6) and understanding the relationship between these two major structural elements.

- The foliation represented in Figure 1b, is Sh or Sv? You use different colors to discriminate different foliation dips. I found it much more useful to use different colors to discriminate different generations of foliation and numbers or different

symbols to discriminate different dips.

-> The Fig.1b is a general geological map more than a structural map to show lithologies and we add few representative foliations to illustrate the general structural context, thus there is a mix of S2a and S2b foliations (previously Sh-Sv). We use different colors to discriminate foliation dips in order to apply the same symbology to all the figures in the article. But we do not want to interpret the structure in an introduction figure (S2a-b), and because the section “planar fabric” where we introduce S2a and S2b foliation comes later in the article. We added the term “S2 foliation” to show that we are referring to all foliations (S2a and S2b). For detailed structural maps see Fig. 3 where S2a and S2b foliations are discriminated with blue and red colors.

-The lineation represented in Fig. 6 is a measure of which foliation? It is impossible to understand from the map. Does it is developed on the Sh foliation? You must specify. You put too many lineation symbols in the map, it is impossible to discriminate the different colors if the symbols are superimposed.

-> As we discussed previously for similar comments in the text, lineations are the same for all foliations (S2b and S2a) and represent a unique set. This is why they are not discriminated like the planar fabrics. We chose to present a large number of measurements (not all) to highlight the ubiquity of lineations in the area and to highlight the limited local measurements variations surrounding the superimposed flow pattern drawn in the figure. Nevertheless, the symbology was chosen to have contrasting colors which are still visible despite the large number of symbols. We have reduced the opacity of the base layer (geological map) to better bring out the different colors of the lineation.

4) Insert in the map of Figure 7 the samples Tr1 and Tr2.

-> Done

5) In figure 11, if in step 2, the main deformation produced S-L tectonites, why in the 3D block diagram represent Phase 2, there are strongly elongated objects like those represented in the block diagram of Step 1?

-> Because the meta-igneous lithologies (orthogneiss and Cannes Migmatites) have preserved their stretched shape from the phase 1 (dominated by the constrictional flow and L>S tectonites) and are almost not overprinted by the plane strain (S-L tectonites) deformation of the phase 2 which is preferentially partitioned in the meta-sedimentary units.

See line 806-816: “*We propose a two-phase model (Fig. 11). The first phase involves the deformation at mid-crustal depth of all lithological units by a gently-dipping constrictional strain, creating L>S tectonites (...) The second phase is characterised by a plane strain flow that develops the S2b foliation and associated strike-slip shear zones. Deformation is partitioned into the paragneiss unit, enveloping ortho-derived lithologies that preserve their stretched shape from the first phase and are generally not or less overprinted.*”

-In the text the word " phase" is not reported. The phases are referred to Sh and Sv foliation? Please specify this point.

-> In the section 5.4 related to this figure, the word “phase” is reported 14 times. For the relationship between phases and S2a and S2b foliation see our explanation above.

6) I also suggest to discriminate in the figure and clearly indicating in Table 1, which sample is representative of fabric Sh or Sv. Without this differentiation, it is very difficult to understand your discussion and conclusion.

-> The type of planar fabric between S2a and S2b foliations has been added in Table1.

Fig.8 “enlarge the dots” “use oblate and prolate as in Fig.9”

-> We have enlarged the dots following the request of reviewer 1. We prefer to retain the terms “Constriction” and “flattening” for this figure because we use these terms in the text which are more appropriate for describing the corresponding results. We use the terms “oblate” and “prolate” in Fig. 9 because they are part of the standard vocabulary used for AMS data.

Fig.10 “These photos are related to Sv or Sh?”

-> These photos are related to S2a and S2b foliations (photos A-B= S2a, C= transition between S2a/S2b, D= S2b), but as explained above microstructures are presented following the different lithologies rather than foliation types because the deformation temperature evolves mainly between lithologies. This being said, following the comment of reviewer 1, we have added details on the type of foliation for each photo in the figure caption.

Authors' Reply to Reviewer 2

RESPONSE TO REVIEWER 2's COMMENT

General comments :

AMS Data: To complement the structural work, the authors have conducted AMS measurements on 5 sites only. How and why, these specific sites have been chosen? Except that these few AMS data are generally in good agreement with structural data, I am wondering if it is worth to include them in this article, especially that the correlation between finite strain ellipsoids and AMS shape remains poor. What do we learn from them? Also, can the authors link the AMS results with microstructures?

Moreover, these magnetic foliations and lineations and associated stereo should appear on a map (No map on Fig. 9)?

To make it short, I would suggest to delete this AMS part.

-> It is precisely because the correlation between finite strain ellipsoids and AMS shape remains poor especially for the L-tectonites that we want to keep this part in the article. We conducted the AMS measurements to study the finite strain shape of the deformation with another method than the microstructural ellipses measurement to obtain results from different methods and compare with field observation. However, results show that AMS ellipsoid shapes are not always correlated to field observation and depend mostly on magnetic mineralogy. This result forces us to be cautious about the interpretation of AMS data for our study of the Tanneron massif structures, but is a very interesting example for the AMS method. The fact that AMS ellipsoid shape is not very reliable to study finite strain shape and depend mostly of the magnetic mineralogy has been reported in very few articles and there is almost no field example as our work (+ where we can compare with other finite strain results obtain with a different method). Moreover, our example shows precise results and even allows us to pinpoint, within single samples, variations of AMS shape between groups dominated by paramagnetic minerals and groups dominated by ferromagnetic minerals. Therefore, we believe that the AMS section of this study is of interest to both the structural geologists and AMS users communities and that it should remain in the manuscript.

Microstructural study: The authors looked at some microstructures to show that a (deformation) temperature gradient exists from east to west. However, the objectives of this microstructural study are not clearly stated.

It makes sense that a migmatitic rock that got "freezed" at depth shows microstructures highlighting a deformation temperature above 550°C with absence of chlorite …and that a similar migmatite affected by an high strain zone displays different episodes including low temperature deformation. Sorry but it is unclear to me where you want to go with this. Do these rocks/structures highlight a different crustal level rather than an E-W cooling gradient?

Also, it would have been good to mention the orientation and name of the studied samples and their locations on a map in order to make the link with the finite strain ellipsoid. As an example, were you able to correlate elongated minerals / directions with

constrictional fabrics (can see some photo on Figure 5 but this should be make it big)? As you did for AMS, I have the feeling that this microstructural study was not conducted in-depth. I am aware that it would have required more work but think that you could have extract more information from them to support your model.

-> As we stated in the introduction of the microstructures section (that we changed for "deformation temperature" section, to be clearer) the objective is to obtain qualitative estimates of deformation temperature, and not to provide an exhaustive description of all type of microstructures ("*Microstructural observations are used to constrain the relative deformation temperature across the area.*"). In fact, without looking at deformation microstructure we cannot be sure whether the strain pattern observed in the whole migmatitic basement (representing a common crustal level of medium to lower crust affected by partial melting) is related to suprasolidus deformation conditions only, or if subsequent subsolidus deformation occurred. So, we cannot know if "migmatitic rock got frozen at depth" without studying their microstructure.

This microstructural study was conducted in depth, we analysed 60 thin sections in the whole area to look at deformation temperature microstructure. Here we synthetised the results by describing the main characteristics for each lithology to see the principal differences and to be able to correlate them with the main finite strain shape and strain fabrics that we identified. We can not describe all thin sections.

- Samples orientations were already provided. We have enlarged them. Similarly, samples names are indicated in the figure captions when the photo corresponds to a sample studied for finite strain analysis, moreover the location of all studied samples is displayed in Fig. 7.

- "were you able to correlate elongated minerals / directions with constrictional fabrics".

-> Yes, this is what we made in the "Finite strain shape" section. We used the "microstructural ellipses measurement" method to obtain the strain ellipsoid shape from field samples fabrics (through microstructures in thin section) and correlated the "elongated minerals direction" (i.e stretching lineation) with a strain style (here constrictional strain). The correlation between the type of tectonite (= constrictional-flattening strain) and the direction of microstructure is shown in the map of fig.7 where ellipses from samples used for this calculation are oriented following the direction of elongated mineral/stretching lineation.

Tectonic model: The finite strain analysis allowed you to define flattened shape fabrics in the Super Cannes area that you interpreted in term of dome roofs. When do these domes emplaced in your model; during stage1?

-> As we discuss in the section 5.4 of the discussion (line 810-816), the formation of potential domes (that we suggest and discuss, without definitively asserting it) mostly in the Super Cannes area occurred during phase 1 at high-temperature synchronously to the partial melting of the basement.

"*The first phase involves (...) This phase represents the sub-horizontal flow of the migmatitic crust at high temperatures. Some parts of the crust with a higher degree of*

anatexis are partially preserved from tectonic forces and experience doming and internal buoyancy-driven flow, as seen in the Super-Cannes hill."

Can you correlate these 2 main stages with tectonic events that occurred in different parts of the Variscan belt of western Europe (other than Corsica and Sardinia)? In the paragraph 5.2 you have tried to correlate and discuss part of your results with other studies conducted elsewhere but did not go really further…

It would have been good to integrate and discuss your model in the scope of the Variscan geology affecting western Europe. It would render your model more substantial.

-> Our work focused more on documenting the transtensional strain regime and the deformation paterns and processes recorded in the late stage evolution of the Tanneron Massif basement than the geodynamic evolution of the whole MTM in the context of the Variscan belt. Consequently, we won't expand on the variscan belt evolution in the discussion as suggested by reviewer 2.

A table including sampling sites with structural data should be added ideally with those from previous authors.

-> We answer this point in the comment related to Figure and table, see below. In Table 1 we have added GPS location of samples (rather than sampling sites locality names which are not as precise) along with the structural data that were already included. The finite strain analysis of these rocks is new and to our knowledge there is no pre-existing data in the literature.

Some references are missing.

-> Thank you for pointing this out. Following in text comments from reviewer 1 and 2 we added missing references in some parts, and justified for others that do not need additional references when observations or interpretations are based solely on our own work and ideas.

Summary: In its present state this manuscript is too long and very sprawling. It should be re written in a more concise way, better structured and focus on key information that could help to support the final model. Figures (see below specific remarks) need to be improved and a table added.

-> Figures have been modified following comments from reviewer 1 and 2. We made our best to add or remind some key informations where missing to clarify the manuscript and help its understanding. We modified some parts to be more synthetic and try to reduce some paragraphs where possible. However, regarding the length of the article, we respectfully disagree with reviewer 2. We are aware that the manuscript contains a lot of results and information, but the actual length respects the journal guidelines (suggesting less than 10, 000 words without caption and references). We wanted to submit a complete structural work, going deep into the interpretation, in order to propose a reliable model for this very complex massif. The majority of the results are based on field work that requires to be exhaustive in the descriptions which is in the scope of Tektonika.

in text comments :

Line 68 (first manuscript release): “This paragraph could be shortened and combined with the previous one.”

-> The entire paragraph has been reduced and combined with the previous one.

Line 94 “References are missing!”

-> One reference is given in the previous sentence which announces the idea and one has been added in the mentioned sentence.

Line 110-11 “Not mentioned on Figure 1” “Not mentioned on Figure 1”

-> The Reyran basin is mentioned on Figure 1B, it is clearly visible in the geological map and its green color is indicated in the legend as “Reyran carboniferous basin”. The Plan de la Tour basin is visible in Figure 1A but not mentioned because of its small size on the figure and does not represent an important feature of our study.

Line 134, several spelling corrections

-> Done

Line 137 “Sounds a bit disconnected from the first part of the sentence.”

-> The sentence has been modified.

Line 139 “Difficult to see it in detail on the photo. I would have added a photo zooming on this that could still appear on the same photo.”

-> We changed the figure to add a zoom on this part according to reviewer’s 2 comment.

Line 140 “This sentence does not make sense here in absence of photo.”

-> We thank Reviewer 2 for this relevant comment. We forgot to add the figure reference, we have added it.

Line 141 “Why don’t you add photographs and describe them? Sounds like if you didn’t go in the field to observe facies and only have the possibility to cite the Orsini thesis!”

-> The description comes from our observations in the field and we quote Orsini because he made a comparable description in his thesis. The sentence has been modified accordingly. However, we do not go into further detail and did not add photographs because these are very minor variations which do not influence the results of the article and the discussion.

Line 142 “Between what and what?”

-> The sentence has been modified.

Line 160 “Any photo?”

-> Same remark, see above.

Line 193 “Should appear on a chronological order.”

-> Corrected

Line 211 “Keep the same spelling homogenous or homogeneous.”

-> Corrected

Line 221 “It would be nice to see the sites on a map (e;g. Fig. 1).”

-> ASM sites have been added on figure 7.

Line 242 “Add the South and arrows to show the kinematic”

> The South is already notified, it has been enlarged and kinematic arrows added.

Line 276 “Figures should be numbered in chronological order of appearance.”

-> This sentence has been removed, which fixes the numbering problem.

Line 305 “and east?”

-> Suggestion added

Line 316 “Description should be directed to some figures.”

-> We forgot to add the figure reference for each precise description, we have now improved this in the revised version of the manuscript.

Line 330 “And so?”

-> We write this sentence to advise the readers that quartz dynamic recrystallisation textures have not a linear relation to deformation temperature but are also dependent on other factors. It is important to remind it in the interests of transparency to remember that these textures can be used to obtain qualitative temperature estimation but do not represent an exact solution to obtain precise temperatures.

Line 385 “Lithology?”

-> The sentence has been modified.

Line 420- 432 and other: “(orthogneiss)” - “(paragneiss)”

-> Done

Line 471 ““REF?” ““REF?”

-> The two proposed models to explain the “rheology-dependent finite strain shape variation” come from our own ideas and not models taken from the bibliography. We have modified the sentence to clarify this point.

Line 492 “REF”

-> Added

Line 496 “To re write”

-> Done

Line 513 “Ferré et al., 2014”

-> Added

Line 541 “Gébelin et al., 2006”

-> Added

Line 542 “Unclear to me. Is the structural level different?” “Do you mean the top?”

-> No, in this scenario the structural levels are the same, the difference of finite strain shape between the Croix des Gardes hill (L>S) and Super Cannes (S>L) is explained by the model of a single common diapir. In a diapir structure the center is characterised by constriction while the overflowing edges of the dome are caracterised by flattening. The erosion will have erased the diapir top and exposed a level just below the cap.

Line 542 “A sketch is needed”

-> In order to limit the manuscript and figures we chose not to include a sketch for this structural feature. The dome shape is suggested on Figure 11.

Line 551 “Ok for super Cannes but it is representative?”

-> Yes, we often see this tectonic feature in the field in the Cannes migmatite unit (which is the subject of this paragraph). We've amended the sentence to make it more precise.

Line 554 “Be more precise”

-> Done

Line 567 “REF?”

-> Added

Line 584 “This sentence is not directly connected with the above text.”

-> The sentence has been relocated and changed.

Line 595 “Repetition”

-> The sentence has been changed.

Line 633 “Photos are needed”

-> The basin is not the target of this manuscript, so to avoid further extending the length of the article we don’t think that photos are key for this specific paragraph. A reference to a MSc report dealing with the basin deformation (with photos) has been added.

Line 644 “repetition”

-> This short paragraph presents the final model and briefly summarises all the observations and reasons that led us to propose a two-phase model. This brief summary is necessary to explain why we are proposing this model rather than another.

Line 656 “Foliation is folded in your figure”

-> As we described in the “planar fabric” section, the entire structure is folded, especially at centimetric to metric scale, and mostly in the S2a domain within the Cannes migmatite unit. This is why we draw foliation folds in the figure.

Figures :

Figure 1:

- Plan de la Tour and Reyran should appear on the map to indicate the Late Carboniferous basins.
- Ductile shear zones should clearly appear on the map. As an example, there is one to the east of the Tanneron village that structured the Orthogneiss (see MSc thesis Gébelin, 1999). These shear zones bounding units such as orthogneiss should also be seen on the cross-section. The orthogneisses should extend further down and not appear as a square/rectangle shape! Only gravity would allow you to draw them such a way if it was the case.
- Add a scale / depth on the cross-section.
- Samples locations + their names from this study should be observed on the map in addition to those from previous authors (e.g. Crevola…)

- As mentioned above in a similar comment, the Reyran basin is already mentioned on Figure 1B in the legend as "Reyran carboniferous basin" and the Plan de la Tour is not mentioned because it does not represent an important feature of our study and we don't have enough space on the insert Fig.1A.

- The major ductile SZ of the MTM are presented in Fig.1A (names and thick black lines). Major and minor SZ of the eastern Tanneron don't appear on fig.1B because it is a geological map without foliation trajectories but they are clearly drawn in Fig.3. We have modified the cross section in Fig. 1 to highlight them. However, lots of SZ as the one you mentioned (east of the Tanneron village) are decametric or less and represent smaller scale structures not relevant for this large scale cross section.

- The orthogneiss are drawn as ovoid shape because as we described in the discussion "*At the map scale, orthogneiss in the Reyran zone can be seen as finite strain markers representing prolate cigar-like bodies, shaped by the interaction of their internal stretching lineations direction and plunge with the topography.*" They preserved a N-S stretched shape from the first phase of the transtensional regime dominated by constrictional strain (Fig.11), thus their cigar-like shape appears as ovoid shape sections in a perpendicular cross section (E-W). It is common in metamorphic domains that former intrusive lithologies appear as lens shape in cross section due to the subsequent deformation of their previous dyke or sill shape.

- The scale for the cross section is already present at the top.

- Samples location from this study are present in Fig.7. We don't understand the mention "those from previous authors (e.g. Crevola…)". Crevola has not performed finite strain analyses or AMS (as we do here) in his PhD thesis or other articles.

Figure 2:

- Sample/outcrop locations should be added on each photo. I can see some GPS coordinates (ok in a table) but a name would be more useful so to directly correlate the photo with a location on the map (Fig. 1).
- *Zoom on Fig.2B*

-> We disagree about the usefulness of providing locality names. Various versions of the local topographic maps between the 1990s and today present different names or names' spelling for similar localities. GPS coordinates are much more precise and reliable. Coordinates in degrees provided at the 4th decimal as we do in the Figures caption and tables are accurate within a 11m radius, which seem as good as it should be for geological work.

-> We have added a zoom on Fig.2B.

Figure 3:

- Are the foliation trajectories only from the authors' data?
- I think that a mistake has been made in the caption: Sv overprints Sh and not the other way around from what I could read in the manuscript.

- Yes, foliation measurements and trajectories are from our own data.

- The mistake has been corrected.

Figure 4:

- Same comment than Figure 2.
- Remove the arrow next to the N (if the north is to the right, just put a N on the right hand side).
- 4B "Add the South and arrows to show the kinematic"

- As explained for Figure 2, we provide GPS coordinates for position accuracy.

- The arrow indicates that the photo is in plan view (North with the arrow refers to a map view) while double indications such as "NE SW" indicate that the photo is in front view (vertical plane).

- The South was already present, we have enlarged it and added arrow to show the kinematic.

Figure 5:

- Good to see some observations supporting cigare-shape or pancake-shape ellipsoids at the thin section scale. However, this should be clearly stated on the manuscript.

- Again, please add the name of sample locations.

- A sentence has been added to clearly mention the cigar-shape illustration at thin section scale.

- As explained for Figure 2, we provide GPS coordinates for position accuracy.

Figure 6:

-In the case AMS data remain in the manuscript, you need to add the magnetic foliations and magnetic lineations on this map. Also, difficult to differentiate your work from the work done by previous authors (e.g. Crevola…).

- Magnetic foliations and lineations are presented through stereograms in Fig.9 associated with structural measurement of foliation and lineation from each site as measured in the field and show a very good concordance for all sites. After that, our objective is to use AMS data to study finite strain evolution with the AMS ellipsoids and not to obtain foliation and lineation azimuth and plunge as for study in isotropic granite for example. Thus, we believe that adding the magnetic foliation and lineation to this map will not provide any information other than the stereogram in figure 9 and will simply show once again that the direction of the magnetic lineations follow those measured in the field. However, as we had forgotten to indicate the location of the AMS site in the first submission, we have added the locations on Fig.7 which is more relevant on a map with all the finite strain shape results (qualitative field observations + Microstructural Ellipses Measurement + AMS).

- The lineations presented in this figure are almost all from our field data, to differentiate our work from previous authors (very few lineations in the middle of the area), we added a different symbology (arrow with a bottom circle).

Figure 8: A real shame not to be able to locate the samples on a map.

- In fact, samples are already indicated in the map of Fig.7, in the legend “Calculated finite strain ellipsoid samples” and in the map by ellipsoids with a yellow outline. But as both reviewers did not clearly notice it, we changed the text in the legend and the outline to make them more visible.

Figure 10: Simplistic (see my comment above). Same, not sample names…these photos could come from someplace else.

-> (See our complete answer above in the “general comment” section). This figure illustrates the evolution of fabrics with temperature and also between ortho- and paragneisses. We find that its simplicity helps convey our message clearly. The GPS coordinates for each sample photograph is already provided in the Figure caption.

Figure 11: Some additional info should be added:

– Pancake shape ellipsoid

– Geological time period

– orientation- some key sample locations.

– shear zones (we can see the kinematics but not clearly associated to shear-zones). How do you exhume high-grade rocks from phase 1 to phase 2?

- Pancake shape ellipsoids are present to the right of the diagram (representing the Super-Cannes domain which is the only domain with dominant horizontal flattening strain) as small black oval ellipses in the YZ and XY sections (compared to the cigar ellipsoids represented in the other domains by rounded and elongated ellipses in the YZ and XY sections). There is not a large 3D white pancake ellipsoid as the two representing

“constrictional strain” in phase 1 and “plane strain” in phase 2 because flattening is mostly restricted to the Super Cannes domain and do not represent a major strain regime elsewhere in the study area.

- Geological time period is not written because we don’t know the precise timing between phase 1 and phase 2, both likely taking place during the Pennsylvanian.

- Orientation is present and represented by the black “N” letter with an arrow in the left corner of the figure close to the block diagram of the first phase. Samples are not present because they are already visible in other map figures and this figure is a synthetic block diagram to resume our model. Adding sample location will complicate the figure which already contains lots of elements.

- Shear zones are represented by anastomosed networks of S2b foliations in phase 2 (red foliation) with tighter and thicker lines than other S2b domain. In fact, almost all S2b foliations domains are local shear zones (+ the main La Moure SZ). We made changes to enlarge lines and bring them closer to highlight shear zones.

- High grade rocks are exhumed during both phase 1 and 2 by the strong stretching flow which thin the overthicken crust. Moreover, the plunge of stretching lineation is not exactly horizontal but ranges mainly between 0-30° allowing for upward movements.

Table 1: ok but you should include a table with your sample locations and structural measurements + those perhaps from previous authors.

- Structural measurements related to each sample are already included in the table. We have added the GPS coordinates (and location are visible in the map of Figure 7). To our knowledge there is no previous study focusing on finite strain analyses or AMS in the Tanneron massif and all original data are from us.

2nd Round of Revisions

Decision Letter

We have reached a decision regarding your submission to ТЕКТОNIKA, "Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France".

Our decision is to: Resubmit for Review. Reviewer A is effectively in favour of publication of the revised manuscript but Reviewer B raises a number of important issues that have still not been addressed. We feel that these need to be carefully considered before publication in Tektonika as the comments and suggestions are all geared towards making the work more accessible to a broader audience and more informative for putting the work in the larger regional context. Please address each point.

Comments by Reviewer 1

I have check carefully the revised paper “Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France” by J. Gremmel, G. Duclaux, M. Corsini and J. Bascouby . I have found the new version noticeably improved compared to the previous one. The modifications about description of planar fabrics make the text more clear for the readers. I'm still puzzled about the interpretation of the two textures L>S and S-L and the explanation about their finding in two different lithologies. The authors stated the these two textures represent increments of deformation, i.e., two phases during a single event. I am not convinced. However, the authors discuss the two different possible interpretations of this textures occurrence and their choice is now clear. On the whole, the authors provide a picture where the tectonic events can be subdivided in more phases. This approach must be clarify in the methods. Taking into account this approach, i suggest to the authors a check of the text about the use of event, stage and phase in order to make more homogeneous the use of this words.

Finally, I suggest the publication of this paper on Tektonika.

Comments by Reviewer 2

Dear Editor, dear authors

Enclosed please find my review of the manuscript entitled "Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France," from Gremmel et al.

This paper is a new version of the manuscript that, in addition to structural measurements (including AMS) and microstructural observations, aimed to publish quantitative finite strain data to characterize the tectonic regime of the Tanneron Massif at the end of the Carboniferous.

The new version has been well improved over the original version where the text was unprecise, lacked some relevant references and did not flow smoothly making it difficult to understand.

Based on the low number of AMS stations and on the fact that the authors' AMS data did not significantly support the finite strain ellipsoids, I suggested the authors to remove them especially that they render the paper too long. The authors disagree and insist to include them on the new version of the paper as they consider that it is interesting to highlight the fact that the shape of the AMS ellipsoid is not always reliable to study the finite strain shape as it depends on magnetic mineralogy. Ok, fine! Although I think that this problem has already been covered by colleagues, I would suggest the authors to write a specific paper emphasizing the 2 different methods used in this study. However, that's fine with me if they want to keep them in the new version.

Regarding the figures, I can see that the authors do not want to make the efforts to improve some figures with the goal to help the reader. Below are my main comments.

Figure 1: I am still convinced that localities mentioned in the text should be included in the figure and vice-versa. Therefore, I would not refer to the "Plan de la Tour" in the text if you do not want to include it on the map.

Figure 2: I find it regrettable not to be able to locate the photos on a map. Ok, the authors mentioned the GPS coordinates, but the reader cannot read the text while at the same time trying to find the locality of the photo on Google Earth or whatever. Therefore, I insist by recommending the authors to find a way so the reader can directly correlate the photo with a location on a map. As directly stated in the answers, the authors might think this useless, probably because they know the field very well but for someone who has no clue of the region, I am convinced that it is very useful.

Figure 3: It would have been interesting to add the data of the previous authors; at least the map from Crevola, and compare with yours …

Figures 4 and 5: I have the same comment as the one written above for Figure 2. It is clearly important to be able to locate the photographs on a map. I guess that each photograph can be correlated with an outcrop so it should not be too difficult…

Figure 8: Always the same problem, it is impossible to locate the samples on a map.

Figure 11: I am very sorry to be annoying but I think that this figure would benefit from additional information such as at least 1. some bullet points highlighting the core message, 2. Geological time period (there are some ages acquired by some authors, including the authors themselves and we cal always mention “ca. …”, 3. A sketch on a corner that would replace the mechanisms encompassed by this region at larger scale. As it is, it is not very understandable.

Also, one of my main concerns was the non-integration and discussion of the results in the scope of the Variscan geology of western Europe at least. It is highly disappointing knowing that some authors have documented transtension or transpression in surrounding terrains!

What is the purpose to conduct such a study if the authors do not try to integrate their works at larger scale and refuse to discuss/compare their final results with previous works conducted in surrounding areas?

My comments aim at improving this work that I consider important for the better understanding of oblique tectonic regimes affecting rocks in the middle and lower crust. I am very sorry if they make the authors frustrated but I think that they would make this publication more substantial (especially the last one).

All the best,

A. Gébelin

Authors' Reply to Reviewer 1

I have check carefully the revised paper "Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France" by J. Gremmel, G. Duclaux, M. Corsini and J. Bascouby . I have found the new version noticeably improved compared to the previous one. The modifications about description of planar fabrics make the text more clear for the readers. I'm still puzzled about the interpretation of the two textures L>S and S-L and the explanation about their finding in two different lithologies. The authors stated the these two textures represent increments of deformation, i.e., two phases during a single event. I am not convinced. However, the authors discuss the two different possible interpretations of this textures occurrence and their choice is now clear. On the whole, the authors provide a picture where the tectonic events can be subdivided in more phases. This approach must be clarify in the methods. Taking into account this approach, i suggest to the authors a check of the text about the use of event, stage and phase in order to make more homogeneous the use of this words.

Finally, I suggest the publication of this paper on Tektonika.

Recommendation: Accept Submission

-> We thank reviewer 1 for his suggestion and we have now carefully checked the text to homogenise the use of terms "event - stage - phase". In order to avoid any misunderstanding we now define in the methodology section what we mean with these terms : "Our model defines the tectonic evolution by two successive deformation phases during a single tectonic event by opposition to two separated different tectonic events. Hence the "phases" represent deformation increments in our study area which are not to be confused with "events" or "stages" defined by major changes in external (boundary) conditions. These former merely relate to local changes in strain accommodation as structures evolve (Fossen et al., 2018)."

Authors' Reply to Reviewer 2

Enclosed please find my review of the manuscript entitled "Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France," from Gremmel et al.

This paper is a new version of the manuscript that, in addition to structural measurements (including AMS) and microstructural observations, aimed to publish quantitative finite strain data to characterize the tectonic regime of the Tanneron Massif at the end of the Carboniferous.

The new version has been well improved over the original version where the text was unprecise, lacked some relevant references and did not flow smoothly making it difficult to understand.

Based on the low number of AMS stations and on the fact that the authors' AMS data did not significantly support the finite strain ellipsoids, I suggested the authors to remove them especially that they render the paper too long. The authors disagree and insist to include them on the new version of the paper as they consider that it is interesting to highlight the fact that the shape of the AMS ellipsoid is not always reliable to study the finite strain shape as it depends on magnetic mineralogy. Ok, fine! Although I think that this problem has already been covered by colleagues, I would suggest the authors to write a specific paper emphasizing the 2 different methods used in this study. However, that's fine with me if they want to keep them in the new version.

-> As we discussed in the first round of review, we considered removing this section following reviewer B initial comment, but we believe the AMS data in this study provide a valuable example of high grade terrain where AMS is not suitable to decipher the finite strain shape, especially for L-type tectonites.

Regarding the figures, I can see that the authors do not want to make the efforts to improve some figures with the goal to help the reader. Below are my main comments.

-> We are very sorry that reviewer B felt this way as we did not intend to oppose her suggestions. The original proposition of reviewer B was to name localities with each photograph and we still find this not quite suitable for localisation. The new idea of pinpointing photographs localities on a map as proposed during this round of review works great and we have edited Figure 1 accordingly to highlight each photograph from Fig 2, 4 and 5 location. All the 3D models available from Sketchfab also have an attached location map so the reader can directly locate the individual outcrops described in the manuscript.

Figure 1: I am still convinced that localities mentioned in the text should be included in the figure and vice-versa. Therefore, I would not refer to the "Plan de la Tour" in the text if you do not want to include it on the map.

-> Following reviewer B recommendation we have now added the "Plan de la tour" Carboniferous basin name in figure 1.

Figure 2: I find it regrettable not to be able to locate the photos on a map. Ok, the authors

mentioned the GPS coordinates, but the reader cannot read the text while at the same time trying to find the locality of the photo on Google Earth or whatever. Therefore, I insist by recommending the authors to find a way so the reader can directly correlate the photo with a location on a map. As directly stated in the answers, the authors might think this useless, probably because they know the field very well but for someone who has no clue of the region, I am convinced that it is very useful.

-> We now provide each photograph from Figure 2, 4 and 5 in the map of figure 1. We agree with reviewer B that this is useful to the reader.

Figure 3: It would have been interesting to add the data of the previous authors; at least the map from Crevola, and compare with yours …

-> Unfortunately data from Crevola are not published in any peer-reviewed journal and this author has not made precise foliation trajectories maps at the scale that we present here. In any case, we already discuss the structural interpretation of Crevola in the discussion section, and we think that adding a figure to show these data would only make the paper more cumbersome while it is already very rich in data.

Figures 4 and 5: I have the same comment as the one written above for Figure 2. It is clearly important to be able to locate the photographs on a map. I guess that each photograph can be correlated with an outcrop so it should not be too difficult…

-> We answered this point above, all locations are now displayed in Fig1.

Figure 8: Always the same problem, it is impossible to locate the samples on a map.

-> Thanks for pointing out this wasn't quite clear. As we indicated in the first review round, samples' location for figure 8 are presented in figure 7 (same thing for samples used in figure 9 for AMS data). We have now added a note in the caption to better indicate this.

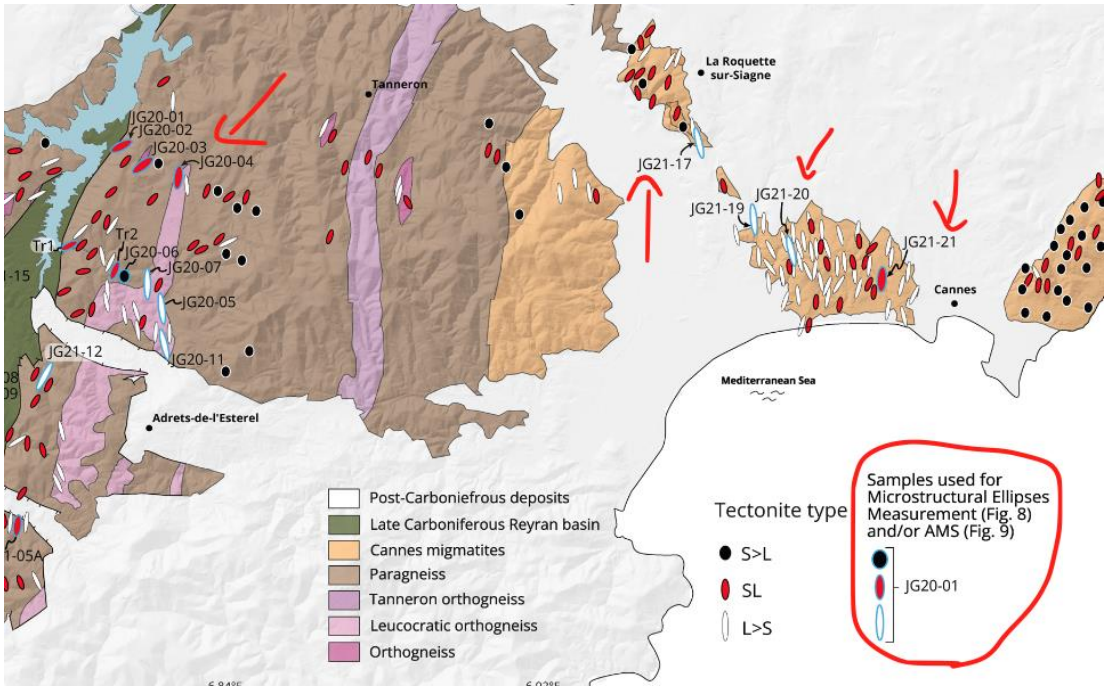

Figure 11: I am very sorry to be annoying but I think that this figure would benefit from additional information such as at least 1. some bullet points highlighting the core message, 2. Geological time period (there are some ages acquired by some authors, including the authors themselves and we cal always mention “ca. …”, 3. A sketch on a corner that would replace the mechanisms encompassed by this region at larger scale. As it is, it is not very understandable.

-> The main aim of Figure 11 is to synthesise the structural evolution restricted to the studied area in the Tanneron massif and to show how the different structures are spatially arranged. The second aim of this figure is to illustrate the late-stage strain evolution of a thickened orogenic crust in a transtensional regime Hence we do not feel comfortable extrapolating our rather local interpretation to the evolution of the whole MTM massif, or to the whole Variscan belt. Because the strain evolution of this transtensional regime is polyphased and complex, the figure can appears busy with several elements showing fabric types evolution in detail. Yet, we have tried to take into account of the reviewer’s comments as follows:
1. We have added bullet points text to the figure to highlight the main characteristics of each phase,
2.We have added a rough geological timeframe in the figure caption reminding that this regime was active between ~325 - 295 Ma. Indeed, with respect to the data and interpretations presented in this contribution we cannot precise the timing any further within the late-stage oblique thinning of the orogen. Ages from the literature belong to the entire event period (~325 - 295 Ma) and we do not present new geochronological data here. They will be the subject of another contribution (in preparation as new geochronological data were acquired and presented in Gremmel’s PhD thesis (see below).
3. The grey arrows on each side of the block diagrams indicate rough estimates of the regional plate motion that forced deformation at the scale of the Tanneron Massif. We cannot add a sketch showing the mechanisms acting at larger scale because there is still some debate about the actual position of the isolated MECS micro-plate in the context of the European Varsican belt.

Also, one of my main concerns was the non-integration and discussion of the results in the scope of the Variscan geology of western Europe at least. It is highly disappointing knowing that some authors have documented transtension or transpression in surrounding terrains!
What is the purpose to conduct such a study if the authors do not try to integrate their works at larger scale and refuse to discuss/compare their final results with previous works conducted in surrounding areas?

-> The topic of this contribution is to document and study the detailed tectonic evolution of structures, type of strain and strain partitioning of a transtensional regime in the middle crust during late stage orogeny. Therefore we focus specifically on structural processes and use the Tanneron massif as a study object to answer a more global question. The aim of the article is not to study the geodynamic of the Variscan belt. In order to compare accurately the late-stage deformation event of the Tanneron massif with those from surrounding areas we need P-T conditions and geochronological data to better constrain this event. We have conducted this type of analyses as you already know but they will be presented in another contribution focusing on these specific aspects contrary to the results that we present here restricted to the structural

data. Indeed, reviewer B has been involved in the Gremmel's PhD thesis review and defense and we suppose this might be the reason she is asking for these data here. We understand the reviewer desire to integrate our findings within the global Variscan context, but the results presented in this contribution do not quite allow that.

This being said, we understand the remark of reviewer B and we added an entire paragraph (lines 801-832), in addition to the references already present in the text, to discuss the integration of our result with previous studies documenting oblique regime during the late-stage event of the Variscan belt.

My comments aim at improving this work that I consider important for the better understanding of oblique tectonic regimes affecting rocks in the middle and lower crust. I am very sorry if they make the authors frustrated but I think that they would make this publication more substantial (especially the last one).

Acceptance Letter

We have reached a decision regarding your submission to тєкτопіка, "Constrictional flow and strain partitioning during oblique deformation: insights from the Variscan Tanneron massif, SE France".

Our decision is to: Accept Submission