We have made substantial changes of the original version of our paper submitted to Short Communication of EGU Solid Earth publications thanks to comments by 3 reviewers and during a second round of reviews motivated by new comments made by the topical editor. We are here responding to a third round of reviews following comments by the Topical Editor S. Tavani.

Point 1 arises from the curiosity of the first reviewer about your temperatures of 300° obtained in a sedimentary sequence that is commonly considered by geologist working in that area as non metamorphic. I share this curiosity and I suspect that many readers will do the same. I recognize your effort in providing a response to this question but it is not fully clear why you do not want to add petrographic description of the "hotter" samples, as suggested by the reviewer #1. I would really appreciate having in the supplementary material images of thin sections of the samples for which you have obtained temperatures around 300°.

Unfortunately, we have not taken photographs of the carbonate samples while performing the Raman analysis. We understand that this might be curious for the readers. But the reason is that the temperatures are obtained on organic matter that did not originate from metamorphic fluids. This was controlled by looking at the Raman spectra itself. Moreover, the maturation state of the organic matter is not visible on the microscope, and it is not dependent on the presence or not of newly formed minerals (which was not seen as we were analyzing the samples). We thus think that such images will not help the reader.
Moreover, as we described in the previous minor review round, the lack of metamorphism is well-known and for a long time by the geologists working in the region and this has been substantiated by a number of papers that we cited. We have proposed some directions to explain why there is no metamorphism in the revised version (lack of significant metamorphic fluid flow and low pressures) but exploring why there is no metamorphism in the Digne Nappe is not the goal of our paper.

Point 2 As the authors know, Raman derived temperatures in diagenetic environments are problematic, and the application of the qualitative method by Saspiturry et al. (2020) could be considered questionable by many readers. Moreover, the fact that your thermal jump occurs at the transition between temperatures estimated by two different methods (one of which is by Saspiturry et al.), strongly calls the attention. In order to demonstrate the occurrence of the temperature jump, if you do not want to use the two methods in an overlap area, in figure 2 you should at least add the graphs for some raman spectra parameters, such as the RBS (Raman band separation) or the R1 (intensity ratio between D and G).

We agree that the reader must be able to see directly in the article what the Raman spectra look like. We have integrated a new figure (fig. 2 in the new version of the manuscript) where we present representative Raman spectra for samples from this study, with the RSCM temperatures obtained and the R1 and RBS parameters as the Editor suggested. In addition, we provide a supplementary file presenting the position and intensity values of the G and D bands and RBS and R1 parameters of the spectra shown in Fig. 2.

Point 3
Com'on! If you tune properly the parameters a polynomial function of degree N compared to a polynomial function of degree N-1 will always provide a better fit. So, if you add a third rifting event your RMS will surely decrease. The point is that you should make crystal clear to the reader that despite you are adding a second rifting event (i.e. despite you are increasing the

degree of the polynomial function) the temperature shift across Middle-Late Jurassic boundary still cannot be modeled.

Perhaps there is some misunderstanding here. No interpolation was made in new Figure 4 (Fig.3 old). The RMS evaluates the goodness of fit between observed (circles) and calculated maximum temperatures (diamond, square and triangle symbols) extracted from our thermal modelling. The problem is non-linear. Calculate Tmax depend on when and how rift-related heating (tectonics) occurred relative to burial heating (sediment burial). For instance, adding a third rifting event after the second Cretaceous one would not change the result. This is because of the limited post-Cretaceous sediment burial and because raise temperatures above the values reached in the early Cretaceous would require to drastically increase lithospheric thinning after the early Cretaceous which makes no sense in the geological context. Also it must be remembered that the temperature-depth relationships presented in new figure 4 are apparent geothermal gradients, this is the reason why we also present in new Figure 6 the evolution geothermal gradients recorded in the stratigraphic column. So to sum up, yes it is obvious that the shift cannot be reproduced because the temperatures above and below are reached during two different periods (early Cretaceous and Cenozoic) and tectonic settings (rifting and foreland basin). The model we propose has the advantage to reproduce 1) the maximum temperatures of around 300°C that can only be achieved in the early Cretaceous when burial and thinning act together, and reproduce at the same time 2) the low-temperature in the upper samples and the temperature above 200°C in the lower samples. Note that none of the three reviewers have raised any issues concerning the modelling approach.

Point 4
I understand that in this paper you do not want to discuss about the connection between the NE-SW striking Valaisian structures and the WNW-ESE striking "Pyrenean" structures of the SE France basin/Brianconnais. However, your figure 6 suggests the two sets of structures form part of the same framework. Either you tone down such a graphic suggestion (i.e. removing the faults in Vocontian basin and the entire Provence platform from your figure 6) or you discuss the bigger picture and explicate you view on how the Pyrenean and Valesian rifts connects.

Why should we remove the faults in the Vocontian basin and the Provence ? First this reconstruction that implies a connection between the Valais Basin and the Vocontian Basin has been proposed in many other studies (that we referred to) and as you said we don't want to elaborate more on the connection between the Valais Basin and the Pyrenean rift (not shown here). Again, none of the reviewers commented on this reconstruction and we already modified it based on your previous suggestion.

Naïm Célini and co-authors.