Please find in this document the answers to the topical editor comments highlighted in red. Modifications in the manuscript are highlighted in blue.

Dear Authors,
after reading the revised manuscript and your responses to the reviewers' comments, I have concluded that an additional round of revision is required.
Below the four points you need to address:

1) You haven't provided any response for a major point raised during the first round of revision. I'm reporting it below and I strongly encourage you to address this issue in the manuscript, not only in the response letter.
You found very high Temperatures, up to 340°, in the Early-Middle Jurassic beds of the most of the sections. These temperatures are commonly considered to be in the range of metamorphism. Low grade metamorphism (green schist facies), but metamorphism. In the Discussion below you propose an interpretation in which such thermal perturbation is related to two rifting events: this means that such perturbation lasted for tens of million of years. I would expect that sedimentary rocks affected by temperatures above 300°C for such a long time interval in an extensional setting (in which the fluid circulation through the crust is highly favoured) would be transformed in metamorphic rocks. At least they should bear evidence of recrystallization and neo-blastesis. And I guess that many of your samples are shales or rocks with a shaly component, that is the most reactive to metamorphic reactions. You should face this point by providing a petrographic description of your samples. In the case you can not find any evidence of recrystallization and neo-blastesis you should propose a mechanism that hampered these processes at such high temperatures.

First of all, remember that among the 63 samples analysed only two RSCM temperatures yielded temperatures between 300°C and 350°C. As indicated in the revised manuscript most studies in the region argue for the absence in the field of metamorphic fluids. In addition, without sufficient fluid pressures no typical greenschist metamorphism are expected (here pressures were close to 0.9-1.2 kbar below lower greenschist metamorphism at time of peak temperature). Actually, our temperatures are consistent with the illite crystallinity index (Aprahamian, 1988) measured at the base of the Digne Nappe that indicates very low-grade metamorphism in the anchizone. Other minerals like chlorite (Artru, 1967; Levert, 199) have been described but they indicate the rearrangement of clay minerals during diagnesis, not greenschist metamorphism.

I am not an analyst or a Raman expert and I do not want to doubt about the RSCM method; but I know that there has been (and probably there is) debate around it. Some authors reported kinetic effects and the occurrence of metastable poorly crystallized graphitic carbon that, in their opinion, would affect the reliability of the RSCM geothermometer (see for example Foustoukos 2012, American Mineralogist). All the more reason you should describe the rocks you analysed, showing the differences between the ones in the upper part of the sections which not experienced high temperatures and the ones in the lower part which were affected by temperatures up to 340°C. Alternatively, if you could not find any evidence, you should discuss how it was possible, in order to exclude that such temperatures are "fake" temperatures due to analytical artefacts

The kinetic effect is reduced in our samples because the temperatures conditions lasted long enough (several millions of years) so that all our samples reached equlibrium. But most importantly the origin of the CM, which can be distinguished based on the Raman spectra, is very different from other studies studying the effect of organic matter crystallisation associated with fluids. Here we have selected spectra indicating that the organic matter was originally deposited in the sediments, and not originated from metamorphic fluids which could be occasionally present but not observed here. The experiments by Foustoukos (2012) were made at HP-HT conditions which are not met in our studied examples. Our results are consistent with the results obtained from previous studies on illite crystallinity and clay mineralogy. We have reformulated this part of the discussion to strengthen our demonstration.

2) In your work you report the occurrence of a temperature shift of about 100° across the Middle/Upper Jurassic boundary. This shift occurs between samples processed with different calibrations (Lahfid et al. (2010) for temperatures ranging between 200 and 340°C, and Saspiturry et al. (2020) for lower temperatures between 100 and 180°C). Can you rule out that this shift is due to a methodological issue? Having an overlap area (let's say 150-250°) in which samples are processed with both calibrations would greatly help to convince the reader.

This shift cannot be due to a methodological issue because prior to applying one calibration or another, a qualitative analysis of Raman spectra is systematically performed in order to apprehend the range of temperature in which the sample is located. Indeed, the Raman spectra of different temperature ranges strongly vary (see Saspiturry et al., 2020; Lahfid et al., 2010 for examples of Raman spectra evolution with temperature). Thus when we identify the probable temperature range of a sample we apply the relevant calibration. On the contrary it is not relevant to apply two different calibrations on a sample from this "overlap area" because the different calibrations imply the use of different parameters and this would imply strong variations on the final temperature value obtained.

3) Again concerning the shift. If confirmed, such a remarkable temperature shift intuitively requires a thermal event at the end of the Middle Jurassic, rather than during the Cretaceous. Comparison between models and data, indeed, shows that the two-rifting scenario fails to reproduce the abrupt temperature change for 4 of the 6 analyzed sections.

It is noteworthy that ALL the scenarios fail to reproduce the abrupt temperature shift along the sections. This shift is really difficult to obtain by modelling. However, the only scenario that is able to reproduce the highest temperatures at the base of the sections is the Two Rifts-model. Indeed, in the One Rift-model, with the same amount of lithospheric stretching, the temperatures at the base of the sections are not reproduced. Thus, the One Rift-model cannot account for these temperatures.

We agree that the results intuitively point a Middle Jurassic thermal event. However, it would be difficult during the Middle Jurassic to obtain such high temperatures because most of the samples of that age were near the surface of the seafloor where the temperature was close to 0°C. A substantial column of sediments is needed above the Middle Jurassic to have a blanketing effect and to allow the emplacement of a geothermal gradient along the whole section that will allow the Middle Jurassic rocks to record such high temperatures. This condition is possible during the Early Cretaceous and not during the Middle Jurassic.

We also want to remind that the modelling part has been used to find the scenario which best reproduces the temperatures. We believe that the modelling results are not supposed to reproduce exactly the RSCM data. Rather they should allow us to choose between several scenarios which one allows the closest reproduction of the RSCM temperatures in order to decipher which one is the probable scenario. Looking at the comparison between the RSCM temperatures and the temperatures generated in the models, the Two Rifts-model represent unequivocally the best hypothesis.

4) The Cretaceous tectonic framework of the SE France basin is slightly different from what you are reporting in figure 6. The Vocontian basin, the durance Isthmus, and the South Provence basin form E-W elongated horst and graben structures and, fully in agreement, mid Cretaceous faults in the area point to N-S extension. Crustal scale mid Cretaceous faults in the Brianconnais domain also point to N-S extension, rather than NW-SE oriented extension as you infer.

We agree that the Vocontian domain is EW elongated as well as the Provence high. Our reconstruction does not disagree with this. However, it is true that the Briançonnais should be rotated so to appear more EW directed during the Cretaceous as noticed in previous publications e.g. Angrand and Mouthereau (2021); Lemoine et al. (1986), Dumont al. (2012), or Beltrando et al. (2012). We modified figure 6 to keep the northern border of the Briançonnais aligned with Provence in a more EW direction.

The above points should be solved or at least you should add a section in the discussion, in which you list the limitations/discrepancies of your model

In summary, we think that the only limitations are related to the use of 1D thermal approach for solving a problem which is 3D. This is clearly stated in the paper. However, we reiterate that the RSCM results are not biased. The limitations raised by other studies (kinetics, fluids, deformation) on the CM geothermometry simply do not apply here.