**Answers to reviewer RC1-2 Christophe Larroque**

We thank for the second review and for raising up further important points that easily could have misunderstood by the reader. We addressed all remarks and questions. We place the comments of RC1 in black and our answers and changes to the manuscript in green letters. We tracked changes in the manuscript.

Dear Editor,

The authors' response to the paper's review satisfactorily addresses many of the points I made (se-2021-9-AC1-supplement and se-2021-9-ATC1). Nevertheless, I think it is necessary to make some further clarifications and corrections in order to clearly precise some points and define the limits of the interpretations proposed in this work. I again recommend major revisions.
I address a new set of comments that are partly issue to discussion with my colleague Dr Marc Régnier (seismologist at Géoazur).
Sincerly,
C. Larroque

Main remarks on the manuscript se-2021-9-ATC1 by Thorwart et al. :
- Clarify the results of space geodesy regarding horizontal motions (these results, of course, have largely evolved since 20 years and the paper should highlight the updated interpretation) : some propositions are below.

We rephrased this section focusing on the centre of the basin and possible sources for compression.

- The analysis of the Ligurian basin is made from the structure proposed by Dannowski et al. 2020 considering that the central part of the basin is constituted by the hyper-thinned continental crust. It is quite legitimate that the authors rely on this hypothesis since it comes from their work and is argued by a published paper. This is not an issue to be re-discussed here but this point is important to be able to conclude, as the authors do, that the seismicity is localized on structures inherited from rifting. However, other interpretations of the central structure of the Basin at this location exist that propose the presence of exhumed mantle without hyper-thinned continental crust and this must be clearly mentioned (e.g. Canva et al., 2021).

Also the refraction seismic study published by Dannowski et al. (2020) leaves the question open, if mantle was exhumed or extremely thin continental crust remained. We added this extension to the sentence in section 2.

- The authors want to prove earthquake clusters in the mantle from location of epicenters and focal depths. The analysis of the seismological data currently presented is not sufficient to convince the reader : (i) the weak P-wave observed on figure 3B, for instance, is not discussed ; (ii) the absolute focal depths are not discussed taking into account the geometry of the network, the velocity model (…) ; (iii) in cluster 1 one event is observed above the moho and the others below: the comparison of the waveforms, above versus below, would allow to argue the focal depths in the mantle in the absence of absolute location.

(i) We added a short discussion on the weak P-wave signal in section 3.1: "*The P-wave is weak in amplitude and followed by a stronger Ps-phase**, which was observed on all OBS stations but not on land stations. S-wave amplitudes are increased by the seafloor itself due to the high impedance contrast. Additionally, the presence of a high or low velocity sedimentary layer with a high***

***impedance contrast in the basin influences the wave field energy. This would only effect signals traveling through this layer, for example Messinian salts, towards the OBS and does not influence signals recorded on land stations. Because of these observations we use mainly amplitudes from land stations to estimate the fault plane solutions.***"

(ii) We added a figure in the appendices (new Fig. A2) showing the theoretical and observed first arrivals and a plot of RMS versus depth for event 2. We are confident that the hypocentres are in the mantle and that we observed also the mantle phase Pn/Sn on OBS A423A and not a crustal phase.

(iii) We added: "*All events of F1 and F2 (cluster C1) are located in the uppermost mantle (between 15 km and 17 km depth, while the events of F3 (cluster C2) are located in the crust. This is supported by observations at station A423A, where Pg/Sg phases could be observed for the events of F3 but not for the events of F1 and F2. Further plots on the accuracy of the focal depths are given in the appendices (Fig. A2).* " Further comments/discussion see next point.

In Fig. A2, the P- and S- phase arrive on OBS A423A later than a head wave travelling along the Moho discontinuity. This can only be explained by a hypocentre below the Moho (case C and D).

- The authors use HyppoDD to determine an uncertainty on the focal depth (some several hundred m, table 1) but this uncertainty is relative inside the cluster. However the authors discuss the absolute focal depths which is not correct because the uncertainty on the absolute focal depth is much larger and in such a context, with the closest station around 20 km from the cluster, may certainly reach sevreal km. The current text may create a major misunderstanding for the reader, this explanation must be reworked to point out that this is not an uncertainty on the absolute focal depths and some precautions must be taken in the further use of the values.

That is right, HypoDD determines uncertainties for relative depths. We added two sentences in section 3.1 that the depth estimation was difficult and that we used the SEISAN routine RMSDEP to check the absolute depths uncertainties: "***The determination of the absolute depths was challenging, since the stations show only Pn and Sn phases, except for OBS A423A that was close enough to observe Pg/Sg and Pb/Sb phases.*** *…* ***First****, an initial event location using HYPOCENTER* ***(for event location) and RMSDEP (for uncertainties of absolute depths, Fig. A2)*** *routines within SEISAN (Havskov and Ottemoller, 1999 and references therein)* ***was done. Afterwards****, events …*".

- About the focal mechanisms and the use of FOCMEC : usually amplitude ratio for P-waves are determined on vertical component and amplitude ratio for SV and SH on horizontal component and SH are more likely to be reliable than those involving SV. As explained in the text, the authors determine the amplitude ratio of P- and S-wave on the vertical component only : do they consider that this have no influence on the result ? The use of the S/P ratio also requires the knowledge of attenuation factors (Qp and Qs) to correct the amplitudes, which is not mentionned. The use of the SV/SH ratio avoids this risky correction.

We added to section 3.2: "*The amplitudes were corrected for attenuation effects using Qs=600 and Qp=1300 in the program FOCMEC (Snoke, 2003).*"

Additionally, we determined the focal mechanism for event 2 using amplitude ratio between SV and SH both picked on the horizontal component. The resulting fault plane solution does not differ from our original solution. Therefore we stay with our original solutions.

- The focal mechanisms computed on figure 4 involve onland stations more than 100 km far from the source. What is the velocity model used to determine these focal mechanisms ? On figure 4 we observe rays from these onland stations with emergence angle of ~90° which is not realistic. The authors must discuss these methodological issues show that their consequences do not call into question the results.

We used the velocity model displayed in fig 4b. It is correct that an emergence angle of ~90° for stations more than 100 km is not realistic for crustal events. But for events occurring in the upper most mantle it is. There, the ray paths of mantle phases Pn and Sn are more or less horizontal and only little curved, due to the small vertical velocity gradient in the mantle. Therefore, the emergence angle is close to 90°.

- The discussion about the rheology (4.3) need to be reworked, several inconsistencies make the text difficult to understand.

See comments below.

Some comments on the corrected paper (se-2021-9-ATC1) :

Line 21 (and also in the paragraph 4) : you mention "faults" but in reality it is the nodal planes of the focal mechanisms that are calculated. As you say in the article, potential faults are not identified and from the mechanism, not only can't one determine which of the two nodal planes is the fault plane (in this case, it will depend mostly on the dip of the fault) but also the direction is related to the quality of the mechanism and therefore to some uncertainty. I suggest to take care in the text of the paper.

We keep "*faults*" in the abstract but are careful in the discussion (nodal planes vs faults) as proposed from you later.

Lines 33-34 : « GPS data do not show any significant present-day shortening between Corsica and the northern rim of the Ligurian Sea (Nocquet and Calais, 2004) » and in the discussion you say that there is an horizontal motion measured by spatial geodesy ~0.4 mm/yr (Nocquet, 2012 ; Masson et al., 2019). It is certainly complex to explain the very weak motions observed by space geodesy and the evolution of these measures over the last 20 years in a few sentences. I suggest you simply say that Nocquet and Calais (2004) have shown that most of the Africa/Europe plates convergence is absorbed at the Maghrebian chain. Then, Nocquet (2012) and Masson et al. (2019) showed that a horizontal convergent motion exists between the Corsica-Sardinia block and the mainland Europe with a value of 0.4 mm/year.

Changed to: "*Nocquet and Calais (2004) have shown that the most of the plate convergence between Africa and Europe is accommodated at the Maghrebian chain. A present day horizontal convergent motion of 0.4 mm/year is observe between the Corsica-Sardinia block and mainland Europe (Nocquet, 2012; Masson et al., 2019). Compressive earthquakes …*"

Line 56 : « Serponelli » is better if Serpelloni.

Changed

Line 84 : Béthoux 1992 is Béthoux et al., 1992

Changed

Line 90 : « seismic rupture » is not the right term, seismic reflexion helps to image fault not seismic rupture.

Changed to "*faults*"

Line 175 : precise « subsurface sedimentary layers » ?

Changed

Line 192 : several times you mention « top of crystalline basement » may be precise what is it : continental crust-mantle-oceanic crust undifferentiated ? Wouldn't it be easier to talk about the sediment layer base ?

Changed to "*base of sediment layers*" at three places.

Lines 209-215 : difficult to understand may be a simple skecth would help the reader to follow your idea.

We added a sketch of the two different nodal planes as figure 4d and linked it in the text.

Lines 210-211 : « Events of family 1 occur at greater depth than events of family 2. ». If I look at the table 1, I read F1 : 15,5 km, 15,1, 14,9 and F2 : 16,3 km, 16,1, 15,2, 16,1…. Then F1 seems to be shallower than F2. Nevertheless I think that it is impossible to discuss such precision on the depth of microearthquakes located with stations more than 20 km away. It seems reasonable to me to delete this statement.

Exchanged 1 and 2. We keep it, depth uncertainties are given in the table. The uncertainties relative to each other should be good. Problematic might be the absolute depths. There was an error in figure 5 in the legend. It is now corrected to magenta star as F1 and yellow star as F2.

Line 211 : nodal planes are observed not faults. Following this observation you could interprete one of the nodal plane as the fault with some discussion.

Changed "*fault plane*" to "*nodal plane*" to discuss if one or two faults were active.

Lines 231-233 : « It was proposed that this shortening is a result from the CCW rotation of the Adriatic microplate rather than from the motion of an independent rigid Corsica-Sardinia block (Nocquet and Calais, 2004). » If you discuss the question of the origin of the compression on the northern Ligurian margin, then it must be presented in its entirety because it is obviously an important point for the question of inversion even if it is not completely resolved at present :

- Nocquet and Calais (2004): at that time measurements were not sufficient to detect the weak horizontal motion between the Corsica-Sardinia block and the mainland. They suggested that the counterclockwise rotation of Adria could be the cause of this compression, although the rotation pole near Turin strongly limits this influence.
- Larroque et al (2009) propose that this compressional zone at the southwestern junction of the Alpine-Ligurian Basin could be related to the stress generated by crustal thickening of the chain to the north with also as a consequence the extensional tectonic regime on the high peaks of the southern Alps.
- Sanchez et al (2010) question this extensional regime in the Alpine domain and propose that the Adria rotation controls the deformation up to the northern margin of Liguria.
- Finally Eva et al (2020) show from seismicity analysis that the counterclockwise rotation of the Adria block has no influence south of 45°N and thus no influence on the compressive regime of the northern Ligurian margin (which is confirmed by the analysis of extensive earthquakes over the Southern Alps [(Thouvenot et al, 2016) and GPS measurements over the Southern Alps (Mathey et al, 2020)].

The current consensus solution is the horizontal northward displacement toward NW of the Corsica-Sardinia block.

We took away the sentence and added Eva et al. (2020) to the discussion. We wanted discuss sources for compression of the basin centre, not at the Ligurian margin, since C1 and C2 are located in the basin centre: "*To summarise previous studies, sources for the regional compressional stresses* ***in the basin centre*** *could be: (1) Africa-Europe convergence, (2) CCW rotation of the Adriatic plate (Larroque et al., 2016), or (3) north-eastward motion of the Tyrrhenian Sea towards stable Europe (Nocquet, 2012). The geodetic network lacks stations in Northern Africa, excluding reliable geodetic constraints on plate motions (Nocquet, 2012). The latest plate motion models (Nocquet, 2012) are based on seismicity and other geophysical and geological information and indicate that the majority (90-100%) of Europe-Africa convergence is accommodated in the Maghrebides. An analysis of two decades of dense GPS data presents a ~0.4 mm/y motion of Corsica representing a NNW-SSE shortening that is compatible with the tectonic and seismicity observations at the Ligurian margin (Masson et al., 2019).* ~~*It was proposed that this shortening is a result from the CCW rotation of the Adriatic microplate rather than from the motion of an independent rigid Corsica-Sardinia block (Nocquet and Calais, 2004).*~~ ***While Eva et al. (2020) show that the CCW rotation of the Adria block has no influence south of 45° N.***"

Line 254 : concentration of earthquakes was already mentionned in the ligurian domain : on the northern margin and in the 2011 epicentral area.

We keep this section as it is to have a short introduction into the new chapter.

Line 255 : the hypothesis that C1 and C2 occurred on the same fault because they area as close as 25 km is … an hypothesis : take precautions and be less assertive.

Changed to: "*It is possible that both clusters may originate from the same fault zone, however, this cannot be clearly determined by our dataset*."

Line 255-265 : the discussion about the difference in orientation is interesting but donc forget that you discuss about nodal planes and the faults are not identified (same topic as line 211). Could the authors propose some hypothesis on which one of the nodal plane is the fault plane ?

Changed first and second "*fault plane*" to "*nodal plane*" according to line 211.

Line 262-263 : « Since the 2011 events are located more to the southeast, closer to the coast, they represent an older phase of rifting compared to C1 and C2 » may be replace by : Since the structures supported the 2011 events are located more to the southeast, i.e. closer to the coast, they represent early rifting stage structures whereas the structures supporting C1 and C2, located more to the center of the basin, were developped during a later rifting stage.

Changed

Line 267 : take precaution about the location in the mantle as the absolute uncertainties on the focal depth is of several km.

For the location of the EQ we used only stations in the Ligurian Basin to avoid 3D effects from topography or thicker crust. Absolute depth still can have high uncertainties, however, we observe a high apparent P- and S-wave velocity that confirms our interpretation that the earthquakes occur at mantle depth.

Line 272 : « formation » : could you precise what is it, I think it is the amount of water in the crust ?

Changed to "crust".

Line 273-276 : I don’t understand the discussion about Handy and Brun (2004) : the seimicity is not an indcator of rock strength but it concentrates in weak zones but the weak zones are indeed zones of low resistance no ? This is confusing for me.

In strong zones the Earth would not break. Weak in the sense possible to break but not so low resistant that tensions are discharged constantly.

Line 277-280 : I agree the remark of Dr Le Breton about the section 4.3 and the new formulation of the authors leaves me confused. Currently, heat flow measurements through active rift such as the East African display high heat flow then a warming of the crust compared to the standart situation. Heat flow in the Ligurain basin (Della Vedova et al., see discussion in Bethoux et al., 2008) is currently high. I don't understand how the crustal thinning could allow a cooling of the temperature in crust ? nor the role of the drop of pressure on the CMB in the strengthenning of the crust ?

We rephrased this section to clearly differentiate, that heat flow values reflect the present day thermal state of the basin not the state during the rifting process. And we added more discussion on results of Béthoux et al. (2008) as proposed by Le Breton.

Line 287 : what is « attenuated crust » ?

Changed to “*weakened crust*”

Line 289 : IN THE Atlantic rather than « …the in the… »

Changed

Line 293 : « 4 km depth » precise : depth below sea level, below the CMB ?

Added “*below the CMB*”.

Line 292-294 : you propose that extension developped without stretching ? It seems to me that the 2 two processus act jointly ? How to produce normal faults without a minimum of stretching ?

Added a “more” to the sentence. Both processes take place coincidently.

Lines 309-314 : Definitely there is something to rethink in the presentation of these interpretation about the rheology. This paragraph is clearly contradictory with the previous. You propose here that the crust is hot whereas you propose previously that the crust is cold (line 277 : « cooling of rocks within the crust »). Furthermore the présentation of a high heat flow in the central Ligurian basin is rather too simple and misleading. First you must mentionned that the work of Hansen and Nielsen is not dedicated to the Ligurian basin but is a modelling of sedimentary basins in general in order to study the relationships between lithospheric structures and (permanent relative weakness zones) and thermal structure and they propose : « the maximum Moho temperature, and therefore also the weakest upper mantle, is encountered beneath the flanks of the basin » and not « …localised crustal radiogenic heat production allow for a temperature maximum at the CMB beneath the basin centre. » as proposed by the authors. In the following they explain why the sediments thermal blanketing could be responsible for the weakest crust in the center of the basin. The modeling proposed by béthoux et al (2008) also deserves to be presented in a little more detail if you want to argue your interpretations : what about temperature pattern, the heat flow (observed and rougly corrected by Della Vedova, Pasquale…) and calculated in the Ligurian basin ?

Added “Thermal modelling for sedimentary basins” to Hansen and Nielsen (2002). We rephrased this section, included the timing (rifting time and present day) and extended the discussion taking comments from Le Breton into account, especially on the results and draw conclusion to our results.

Line 341-343 : in the legende of A2 precise « first motion polarities for events 2, 6, 8 and 9 from left to right ».
Added

Canva et al. Structural inversion of the North Ligurian margin: results from the SEFASILS experiment, EGU General Assembly 2021, online, 19–30 Apr 2021, EGU21-9759, https://doi.org/10.5194/egusphere-egu21-9759, 2021.

Larroque, C., Delouis, B., Godel, B., Nocquet, J.M., 2009. Active deformation at the southwestern Alps–Ligurian basin junction (France–Italy boundary) : evidence for recent change from compression to extension in the Argentera massif. Tectonophysics, 467 (1–4), 22–34, http://dx.doi.org/10.1016/j.tecto.2008.12.013.

Both works not added, we keep the discussion on the central basin and do not extent it to the Ligurian margin.

Eva, E., Malusà, M.G., Solarino, S., 2020. Seismotectonics at the Transition Between Opposite-Dipping Slabs (Western Alpine Region). Tectonics, 39, https://doi.org/10.1029/2020TC006086.

Added

Thouvenot, F., Jenatton, L., Scafidi, D., Turino, C., Potin, B., Ferretti, G., 2016. Encore Ubaye : Earthquake Swarms, Foreshocks, and Aftershocks in the Southern French Alps. Bull. Seism. Soc. Am., 106, 2244–2257, https://doi.org/10.1785/0120150249.

Not added, we keep the discussion on the central basin and do not extent it to the Ligurian margin.

Mathey, M., Walpersdorf, A., Sue, C., Baize, S., Deprez, A., 2020. Seismogenic potential of the High Durance Fault constrained by 20 yr of GNSS measurements in the Western European Alps. Geophys. J. Int., 222, 2136-2146, https://doi.org/10.1093/gji/ggaa292.

Not added, we keep the discussion on the central basin and do not extent it to the Ligurian margin and the Alps.

**Answers to reviewer RC2-2 Eline Le Breton**

We thank for the second review and for raising up further important points. We think that especially section 4.3 is now improved and clearer to the reader. We addressed all remarks and questions and are very grateful for the recommended changes to the manuscript. We place the comments of RC2 in black and our answers and changes to the manuscript in green letters. We tracked changes for both reviewer comments (RC1 and RC2) in the manuscript.

**Review of "Basin inversion: reactivated rift structures in the central Ligurian Sea revealed by OBS" by Thorwart et al.**

First I would like to thank the authors for taking into account and replying to my comments during the first round of review. The uncertainties on the data and method section are now presented and discussed in more details. The manuscript is thus significantly improved. I have however some remaining points regarding the presentation/formulation especially of the amount of rotation of Corsica-Sardinia and in the discussion of the fault planes (4.1) and of the rheology/temperature/seismicity (4.3; see comments listed below) that would require some additional minor revision of the text.

l. 19: Oligo-Miocene rift basin

Added

l. 21: Slightly different striking directions of presumed rift-related faults in the basin centre

Added

l. 23-24: I find confusing to compare present-day S-wave velocities and Vp/Vs ratio to strengthening of the lithosphere during a rifting event that happened more than 16 Myrs ago. I would rephrase slightly here, following comments below on Section 4.3.

We assume that the present-day characteristics of the lithosphere reflect the last stage/the end of the rifting and might help to understand the processes leading to that late stage rheology although it ended 16 Ma. The same we assume for the rifting-related faults that developed until 16 Ma ago and might be re-activated today.

l. 25: which is no longer active and located in a plate interior.

Added the timing: "*… during **the Oligocene-Miocene** rifting related extension and thinning of continental crust.*"

l. 74: Careful here, there was a slight misunderstanding on the amount of rotation. Speranza et al. (2002) and Gattacceca et al. (2007), based on paleomagnetic data from Sardinia, suggest a rotation of ~23° to 45°, respectively, between ~21-16 Ma. In Le Breton et al. (2017), I estimated the total amount of divergence between southern France and Corsica-Sardinia since rifting started at about 35 Ma (so between 35-16 Ma, total amount of CCW rotation relative to Europe). This suggests a total rotation of at least 53° between 35-16 Ma (not only 21-16 Ma). So please remove "or" and rephrase for example as:

"a counter-clockwise (CCW) rotation relative to Europe (…) of ~23° (Speranza et al. 2002) to 45° (Gattacceca et al. 2007) between ~21-16 Ma, and of ~53° between 35-16 Ma (Le Breton et al. 2017), with …".

Added the years as proposed.

Figure 2: Either you remove the 53° from the Figure and keep the caption as it is, describing the CCW 23 degrees of rotation in Miocene times from Speranza et al. (2002). Or you should modify the caption accordingly to previous comment (53 degrees are for Oligo-Miocene times).

Added accordingly to the comment above.

l. 206-207, Table 1 and Figure 5: Reading Table 1, C1/F1 are slightly shallower (~15 km) than C1/F2 (~16 km) not the other way around as mentioned in the text and colored on Figure 5. This should be double-checked.

Corrected in the text. Legend in figure 5 was wrong and is now corrected to magenta star for F1 and yellow star for F2.

l. 207: Please precise the main strike of the two planes (NE-SW to ENE-WSW) and that you cannot identify which of the two was the actual fault (activated/ruptured) plane as it didn't rupture the surface.

We added: "*We observe two possible ~~fault~~ **nodal** planes **with a main strike in NE-SW to ENE-WSW** (Fig. 4c, Tab. 2). **However, we cannot identify which of the two was activated.***"

l. 207-209: "For the second fault plane …. For the first fault plane…" These sentences are not clear and should be rephrased. Do the authors mean rather the second / first family of events (and not fault plane)? An event location cannot coincide with a direction of plane…

We changed it to "*nodal plane*" as recommended by C. Larroque.

l. 211-212: "therefore we use the term 'rupture area' in the further discussion", this part of the sentence could be removed as you don't really discuss the rupture area after that.

We removed "*in the further discussion*".

l. 232-233: "If we project C1 and C2 on line A-B that follows the push direction (based on the rake of Table 2 I suppose?) of the thrust events, they map in a slightly tilted vertical plane dipping north-westwards (Fig. 5d)."

Added "*(based on the rake, Tab. 2)*"

I'm not sure to get what you want to say here: do you suggest then that this could indicate potentially the orientation of the ruptured fault plane? Would then the second nodal plane (Table 2), which has a higher dip (around 60°) and dip towards the NW (Table 2 and Figure 4) as along this A-B profile, be then the ruptured fault plane? Interestingly, this dip angle of 60° is typical for normal faults and you suggest in the following sentence that the earthquakes occurred along a pre-existing normal fault that was reactivated into a thrust. However, it could be quite steep for normal faults reaching down to lithospheric mantle.

We added a little sketch in figure 4d showing the fault plane solutions in a side view. Indeed, we would expect that the faults would have a smaller dip. On the other hand, these structures might have generated at the late phase of opening.

l. 251: C1 (ENE-WSW to NE-SW)… 2011 events (NE-SW to NNE-SSW)

Added

l. 257-258: I would either remove "that was estimated with ~23° to ~53° in total amount of rotation between 35-16 Ma" to avoid confusion as mentioned above in previous comment, or rephrase accordingly (23° is between ~21-16 Ma, 53° is for the entire rifting/spreading period ~35-16 Ma). You mentioned already the amount before so it might not be necessary to write it here again.

We keep the numbers for better comparison, but changed the phrasing according to the comments above.

Section 4.3: In my opinion, it is important to clarify in this section processes that happened during rifting (35-16 Myrs ago) and the present-day state (thermal and rheology) that is important to understand present-day seismicity.

Most of the discussion - till line 301 - focuses on what happened during rifting. Indeed, it is important to mention it, to explain possible pre-existing weaknesses (rift-related normal faults) in the lithosphere and within the upper mantle, due to former rifting processes. These zone of weaknesses may be reactivated today due to regional compressional stress.

I would rephrase the sentence l. 272-273 to "Stretching of the lithosphere brings crustal rocks to lower pressure and temperature, and thus the lower part of the crust into brittle domain (Perez-Gussinyé and Reston, 2001)". Otherwise, it is confusing because during rifting, the thinning of the lithosphere rather increases, not cools, the overall temperature and high temperatures are usually associated with weakening of the lithosphere, not strengthening. But since rifting stopped, it must have cooled down since then (minus the effect of thermal blanketing from the thick sediment package that reduce lithospheric heat loss as mentioned).

We agree and changed the sentence to the suggested.

This brings me to my last point, the last paragraph (l. 301-305) on heat flow and temperature is confusing/contradictory. Heat flow reflects the present-day thermal state, and this is crucial to understand depth of seismicity. Therefore, this part of the discussion should be improved.

The first sentence indicates that present-day heat flow is high in the basin centre, which may indeed contradict a cool CMB. The following sentence starts with "However" so we expect something explaining why the temperature may still be "cold" in the basin centre, but it finishes by "… allow for a temperature maximum at the CMB beneath the basin centre". So what is meant exactly here? Please clarify this paragraph.

We rephrased this section to clearly differentiate, that heat flow values reflect the present day thermal state of the basin not the state during the rifting process.

Bethoux et al. (2008) is quoted at the end but is not discussed. But actually, this paper provide very good argument to explain the depth of seismicity. Indeed, their thermal modelling (their Figure 5) in the Ligurian Basin, although located more to the north and assuming oceanic crust in the basin centre, indicates depth of the isotherm 320°C, interpreted as seismogenic zone, ranging from 5 to 20 km across the basin. They mention the "occurrence of deeper (up to 20 km deep) earthquakes near the center of the basin, more likely favored by contrast in rheology", rheology contrast most likely due to the above mentioned pre-existing rift-related faults reaching the lithospheric mantle. Similarly, the recent work of Spooner et al. 2019 (Thermal field in the Alps and

its relation to seismicity; there's also Spooner et al. 2020 currently in revision in the same special issue of Solid Earth) discuss the link between thermal field and seismicity distribution in the Alpine area. They show a clear link between location of seismicity and depths of important isotherms within the continental crust/lithosphere (interpreted as mineral phase changes). Most seismic events occur between the 275°C and 450°C isotherms, which fits with the proposed seismogenic depth range in the modelling of Bethoux et al. (2008).

We added discussion on Béthoux et al. (2008) and draw the circle back to our results and their interpretation as proposed. We added: "*These effects are also seen in the Alps and their forelands, where temperatures in the centre of the Molasse Basin are 20° C warmer than at the edges (Spooner et al., 2019). Further northeast of our study area, Béthoux et al. (2008) performed 2D thermomechanical modelling to understand the location of seismic activity. They show that the seismogenic zone in the centre of the Ligurian Basin reaches down to ~20 km depth. Béthoux et al. (2008) assume oceanic crust in the basin centre and relate the location of earthquakes to contrasts in rheology and the presence of a continent-ocean transfer zone. The events of our observed C1 cluster range in the modelled seismogenic zone for the northern Ligurian basin; the contrast in rheology might be provided by pre-existing rift-related faults reaching the lithospheric mantle*."