Barcelona, Spain
November 29th, 2023

Dear Editor,

We would be very grateful if you could consider our manuscript "Thermal structure of the southern Caribbean and NW South America: implications for seismogenesis" for publication in Solid Earth, revised to tackle all the minor issues raised by the reviewers.

We are again very thankful to Dr. Wimpenny and the anonymous reviewer #2 for their useful feedback, which has contributed to significantly improving the manuscript.

The latter, as before, is accompanied by supplementary electronic materials, and all original data and results have been published in a separate public data repository that will be available online if the paper is accepted for publication.

A point-by-point reply to the reviewers follows.

We hope that you will find the submission in good order. Thank you very much for your consideration and best regards,

Ángela M. Gómez, also on behalf of the co-authors.

Reply to comments by Dr. Wimpenny

General Comments:

1. Thermal modelling uncertainties: There is no mention of the uncertainties associated with the temperatures derived from the thermal modelling. Uncertainties will derive from the material parameters (radiogenic heat production, thermal conductivity), as well as the basal boundary condition at 75 km based on converting S-wave velocities to temperatures. The approach the authors take is to select one model that "best-fits" the surface temperature observations from a subset of 25 models in which they have varied the material parameters. However, given that there are vast numbers of variables in these 3-dimensional models, then 25 models as a sensitivity test is unlikely to capture the full range of possible temperature distributions that could match the data. It is also unclear which of the models they discard fit the data slightly worse than the best-fit model, but still fit the data to within its uncertainties, in which case the data cannot be used to infer which model is most accurate.

The arguments the authors present would be significantly improved by including some discussion of the estimated uncertainties in temperatures from their thermal models, and how these uncertainties translate into the uncertainties in the earthquake hypocentral temperature estimates.

Response:

We would like to thank the reviewer for this comment. We agree that 25 models do not capture the full range of sensitivity due to different thermal parameters. However, this is out of the scope of our manuscript, as dedicated efforts are required to properly achieve such uncertainty quantification. This is further explained in the response to the comment "Line 71-72".

Regarding the uncertainties in the hypocentral depth temperatures, we have added the following text in Sec. 4.3:

Given a thermal model, errors in focal depths propagate into uncertainties in the hypocentral temperatures. The values represented in Figs. 6 and 7 are the most likely ones, corresponding to the best estimates of hypocentral locations; uncertainties have been omitted for clarity. For each earthquake, the possible temperature range can be measured directly from the 3D thermal model (Gómez-García et al., 2023), considering the depth range resulting from the best depth estimate plus/minus the formal 90% depth error. Also, an approximate estimate of its temperature uncertainty can be obtained by multiplying the depth error times the local geothermal gradient at the hypocentral location (e.g. Figs. 5 and S6). For deeper crustal earthquakes, both the formal depth errors (Fig. S5) and the local geothermal gradients (Fig. S6) are typically smaller than those for shallower events, implying typically smaller temperature uncertainties too. Note that real hypocentral depth errors may be larger than the formal ones reported in the catalogues (e.g. Wimpenny and Watson, 2020), due to systematic errors earthquake location procedures, such as in the assumed seismic velocity model (e.g. Husen and Hardebeck, 2010). Consequently, eventual improvements in velocity models and earthquake location accuracy will directly reduce the uncertainties in hypocentral temperature estimates.

2. Robustness versus physical meaning of D90: In the original review I suggested that D90 might not be the relevant metric for mapping the controls on the depth of earthquake generation, because it inherently ignores the deepest events and therefore consistently under-estimates the depth to the base of the seismogenic layer. I agree with the authors that estimating D100 is not robust, as one new earthquake can change the D100 estimate. However, there is an important difference between whether a metric is robust, versus if one is physically relevant. The D90 should track changes in the depth to which the *majority* of the seismicity takes place. It does not necessarily track the brittle-ductile transition. I would recommend that the authors go through the manuscript and make sure this distinction is made clear.

Response:
Thank you. We have removed from the text the relation with the brittle-ductile transition to avoid potential misunderstandings.

The robustness of D90 has also been recently highlighted by Ellis et al. (2023) when estimating the maximum depth of seismic rupture on New Zealand's active faults: "We have used D90 rather than D95 as a more robust estimate of H, because D95 (the 95% seismicity cutoff depth) will be more sensitive to location and depth errors for regions with sparse seismicity in which depth uncertainties are about 5–10% of total depth." We now cite this reference in the text.

3. Use of colons throughout the text: There are a number of places where colons are used after abbreviations (like e.g.:). I'm not sure the colons are necessary, but this can be confirmed by typesetting of the article.

Response:
Thank you. We removed the colons as it seems to be the editorial style.

4. The role of hydration state: The majority of the manuscript focuses on how lithology and temperature might be the main control on why some parts of the deep crust are seismogenic but others are not. A number of studies have recognised that the presence of water within minerals and interstitial water is also likely to be important in controlling whether a given material will be seismogenic at a given temperature [e.g. Mackwell et al., 2004; Jackson et al, 2008]. Can the authors explain why they think hydration state of the crust is not important, or why they have not mentioned hydration, in their discussion for the controls on the depth of earthquakes?

Response:
Thank you for your comment. We briefly mentioned the role of dehydration reactions in section 4.4:

"Alternatively, the occurrence of upper mantle earthquakes is nowadays broadly recognized (e.g. Chen et al., 2013) as also dehydration reactions can trigger seismicity at temperatures above the normal brittle-ductile transition (e.g. Bishop et al., 2023; Rodriguez Piceda et al., 2022)."

However, as our modelling scheme does not take into account water content, we decided to keep this discussion out of the paper's scope.

We have added the new references suggested by the reviewer to the paragraph above.

Line by Line Comments:

Line 14: If the authors want to use crustal seismogenic depth (CSD) then, in my opinion, they need to be explicit that it is similar to the brittle-ductile transition, but if seismicity extends through the crust and into the upper mantle, then the CSD does not correspond to the brittle-ductile transition. Statements like "… the CSD is a proxy for the brittle-ductile transition…" are potentially true, but not always.

Response:
We removed from the text the relation with the brittle-ductile transition to avoid potential misunderstandings.

Line 15: "The CSD largely limits the depth down to which crustal earthquakes may rupture …" – the phrasing of this makes it sound like the CSD controls the rupture depths. I'd suggest re-phrasing to: "The CSD represents the depth to which crustal earthquakes occur, and therefore is an important constraint on the seismic hazard in a region because it will be related to the maximum depth of earthquake ruptures".

Response:
Considering the use given to CSD in the seismic hazard literature, we have rewritten the sentence as "In particular, most earthquakes in the crust nucleate down to the crustal seismogenic depth (CSD), which is a proxy to the maximum depth of crustal earthquake ruptures in seismic hazard assessments."

Line 32-33: "The coherence of the hypocentral temperatures with those expected from laboratory measurements provides additional support to the model." – which model? The thermal model? The model in which lab experiments are extrapolated to lithospheric scales?

Response:
We modified the text as follows:
"The coherence of the calculated hypocentral temperatures with those expected from laboratory measurements provides additional support **to our modelling workflow**."

Line 40-41: I would suggest removing the text after "… i.e., temperature at which …" and then merging the second paragraph with the first.

Response:
Thank you. We changed the text accordingly.

Line 41: Change “assemblies” to “assemblages”

Response:
Thanks for pointing out this typo. We fixed it through the text.

Line 50-53: It would be worth explaining exactly how Ueda et al., (2020) inferred that earthquakes occur in mafic rocks at temperatures of ~720 degrees C [i.e. they used thermobarometry of mineral assemblages in rocks containing psuedotachylytes to infer the temperatures at which the psuedotachylytes formed]. This is relevant because the thermobarometry results have associated uncertainties of ±50 degrees typically, so the range of temperatures might be more like 670-770 degrees C.

Response:
We modified the text as follows:
“For example, Ueda et al. (2020) found that the brittle-to-ductile transition in peridotite occurs at ~720 °C, **based on thermobarometry of equilibrium mineral assemblages in fault-related deformed rocks (pseudotachylytes, cataclasites, and mylonites)**.”

Line 53: “Afonso and Grose (2013) … used a more realistic thermal model than … McKenzie et al., (2005)” – more realistic in what way? Answering this question is important for the reader to be convinced that the models were in fact an improvement, and therefore that the existing bounds on the temperature of seismogesis in the mantle may be incorrect. I think Afonso and Grose included the temperature dependence of density, specific heat and conductivity derived from laboratory experiments (though worth double-checking this)?

Response:
Thank you. We clarified this sentence and updated it as follows:
“Similarly, Grose and Afonso (2013) studied the evolution of the oceanic lithosphere using more realistic thermal models than those assumed by McKenzie et al. (2005) **(i.e. including the effects of hydrothermal circulation, oceanic crust, and temperature-pressure-dependent thermal properties, as well as mineral physics)**, and found a brittle-ductile transition closer to the 700-800°C isotherms, depending on the estimated mantle temperature.”

Line 64-67: “intracontinental faults, the brittle to ductile transition seems to be … limited by the 300-350 degree C isotherm” – this statement is likely not true. There is plenty of evidence for earthquakes occurring on intracontinental fault zones at depths where the estimated temperatures far exceed 300-350 degrees C [see Jackson et al., 2008; Sloan et al., 2011; Craig et al., 2012; Emmerson et al., 2006].

Response:

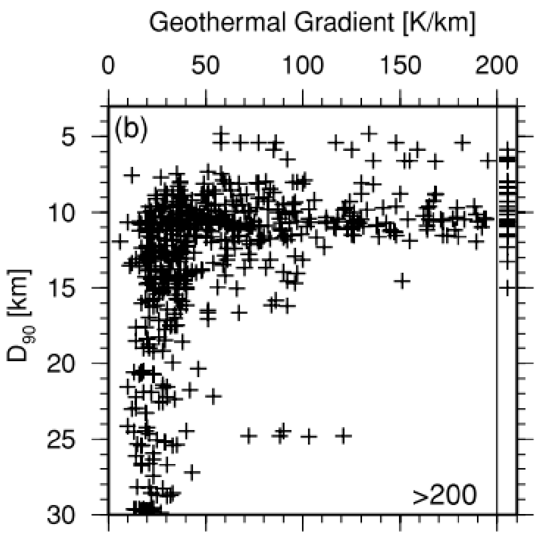
We agree in this point with the reviewer. Therefore, we removed the details about the bounding isotherms, as they were a result for a particular region studied by Zuza and Cao (2020). The new sentence reads as follows:
"The results from these efforts indicate that in intracontinental faults, the brittle-ductile transition seems to be controlled by variations in the geothermal gradient (Zuza and Cao, 2020)."

Line 63: Consider changing "up-scale" to "scale up" and "target" to "determine".

Response:
Thank you. We modified the text accordingly.

Line 67: Can you cite some examples of where the high geothermal gradient correlates with shallowing seismicity to support your argument here?

Response:
One example is the work by Tanaka (2004) -already cited in the manuscript-, who studied the relationship between heat flow, geothermal gradient and D90 depths in Japan. Figure 1 in Tanaka (2004) (see below) shows that as the geothermal gradient increases, the D90 depths become shallow, particularly for geothermal gradients > 100 K/km.



Line 71-72: I agree that the 1-dimensional geotherms are a simplification, but these simplifications are made because it is believed that horizontal diffusion or advection of heat plays a minor role in controlling the temperature field in relatively stable tectonic settings. Similarly, a simple layered geometry is often assumed, because the exact nature of the subsurface lithologies, and their material properties (e.g. radiogenic heat production, thermal properties) are not known precisely. The key point is that the unknowns contribute greater uncertainty to the temperature predictions than does ignoring horizontal diffusion of heat, and therefore the 1-D simplification is justified. Equally, a full parameter sweep can be performed with 1-D models, meaning you can quantify uncertainty more easily.

Response:
We would like to thank the reviewer for the comment, to which we only agree in part. While discussing the limitations of considering (multi) one-dimensional thermal modelling, it is worth to note that these studies not only assume that advection and lateral diffusion of heat can be neglected, but also the effects of 3D heterogeneities in the rock properties as driven by an heterogeneous lithospheric configuration (which can be hardly captured by 1D approximations).
Heat refraction from material contrasts as well as thermal blanketing by sediments are some examples. Those are indeed important processes for building up lateral and vertical variations that are genetically linked to the configuration of the plate, including structural inheritance. These effects can only be captured and described by considering a 3D model that integrates as close to reality as possible (given the data availability) structural heterogeneities between first order geological domains "amalgamated" through time into the present-day lithospheric configuration.

On the second point raised by reviewer#1, we agree that simplified 1D modelling is computationally less expensive and as such, offers capabilities to ensemble modelling. Here the reviewer does however disregard recent progress made in the field of model order reduction modelling, which nowadays enables to run multifidelity ensemble simulations (for both global and local sensitivity analysis and uncertainty quantification) without imposing stringent limitation of the model geometry (1D vs 2D vs 3D) as well as the driving physics (whether thermal diffusion or more complex non-linear physics). Some of the co-authors have indeed demonstrated how a family of such surrogate models, based on a reduced basis approximation of the lower order dimension, can indeed be used for complex 3D geology and non-linear physics (e.g. Degen et al., 2021; Degen et al., 2022).

Line 79-80: "Upscaling the seismogenesis from laboratory experiments…" consider changing wording to "… scaling up the predicted conditions of seismogenesis from laboratory experiments to the lithosphere"

Response:
Done.

Line 84: Is the CSD really "influenced… by the local geothermal gradient"? The local geothermal gradient is just a proxy for the absolute temperature at depth, which is most likely the parameter that controls whether faults break in earthquakes or creep aseismically and therefore the CSD.

Response:
To clarify the sentence, we refer now to temperature instead of geothermal gradient: to "As the CSD is influenced by factors that vary in space, such as lithology and **temperature**…"

Line 88: "The subducting segments of the … slab … in the study area are flat … implying that the subucting [sic] velocities might be lower than in the steep segments" – is this true? I would assume that, if the slabs are plate-like and do not deform extensively internally, then the subduction velocity and the advection of heat beneath the overriding plate should only vary along the length of the subduction zone due to the variations in relative plate motions about the plate's rotation pole. What evidence is there that the subduction velocity is slower in the flat slab segments compared to in the steep slab segments in the Andes?

Response:
The reviewer's interpretation would imply that the slab is rigid, which is not expected to be the case. Slower subduction velocities in flat slabs have been noted in the references cited: Currie and Copeland (2022) noted this in a different subduction zone (Farallon plate), and Schellart and Strak (2021) found this in geodynamic simulations (where flat subduction occurs when subduction velocity reaches a minimum).

Line 91: "on much longer timescales" not "in much longer timescales".

Response:
Done.

Line 93-95: I am really struggling to understand what this paragraph means. Maybe consider re-phrasing to: "The novelty of our study is to consider how spatial heterogeneity in the lithology of the lithosphere and mantle temperature influences the temperature distribution and seismicity within the crust"?

Response:
Thank you. We added this suggestion and connected this sentence with the paragraph above it.

Line 96-102: This statement about seismic hazard is important, but the authors need to be more explicit of exactly how their work can update our understanding of seismic hazard in the region. Specifically, it will provide an estimate of the spatial variation in the thickness of the seismogenic crust and its links with surface observables. The thickness of the seismogenic crust sets the seismogenic area of faults, and therefore the possible maximum magnitude of earthquakes these faults can host.

Response:
Thank you for this suggestion. We modified the text as follows:
"In particular, the CSD is a proxy to the maximum depth of seismic ruptures in crustal faults (e.g. Ellis et al., 2023; Zhang et al., 2022), which in turn may limit the rupture areas and the maximum magnitudes of the earthquakes that these faults may host."
The new references cited have been added to the bibliography.

Line 105: "results" not "resulted".

Response:
Done.

Line 108: Spelling error – "steep" not "step".

Response:
Done.

Line 110: Are there any studies to cite that have looked at the timing of volcanism across the region from absolute dating, and which have argued that the presence of a flat slab led to the termination of volcanism, to support this argument?

Response:
Yes, the work by Wagner et al. (2017) focuses on the timing of volcanism and the evolution of the Nazca subduction through time. We misplaced the citation to this paper in the previous version of the manuscript. We corrected the paragraph as follows:
"The present-day flat slab geometry has been established since about 6 Ma, when the Nazca tear developed separating the north (flat) and south (steep) segments. As a result, the volcanic activity has ceased in the continental crust of the overriding plate of the north segment, which spatially corresponds to our study area **(Wagner et al., 2017)**."

Line 113: "remainder" not "remaining".

Response:
Done.

Line 130: "dominated by plateau and magmatic arc terranes" – as in oceanic plateau rocks? Please clarify what is meant by plateau rocks.

Response:
Yes, we meant **oceanic** plateau, and have modified the text accordingly.

Line 140-144: A general question about this section that it might be worth trying to address in the text: why is it relevant what geologically-inferred sutures and fault systems run through the study area? I can see why the geological terranes are important, but less so the specific faults.

Response:
We had already mentioned (former lines 119-120) that "As a consequence, large-scale sutures (faults) act as major boundaries between these terranes (Kennan and Pindell, 2009)". To further clarify our motivation we have added to that sentence "so they have to be addressed, as they may potentially limit domains with different thermal and/or seismogenic behavior."

Line 184-186: "The best model was selected as the one that independently best reproduced the temperatures measured in the boreholes". It seems important to consider all the models that match the borehole temperature data to within the data's uncertainties (±10 degrees?), as opposed to just the one model that best-fits the data, especially if the differences in data fits are small. The reason I say this is because models with the same near-surface temperatures and mantle temperatures could have very different temperatures in the mid and lower-crust, so just considering a single model might not be reflective of the range of possible temperatures at the depths of earthquakes.

Response:
We agree with the main reasoning, that is, models having different properties can fit a single observable (considering the range of uncertainty in the observable measurements). However, we would like to add that the range in the thermal parameters, as discussed in the manuscript, have been chosen based on the insights from the 3D data-integrative geological modelling (i.e. regional geology, gravity, seismic profiles, etc) and it has been chosen also to be consistent with our knowledge of the tectonic evolution of the study area. This is reflected in the fact that we ended up with a finite range of variations for each parameter, or in other words, we performed a local sensitivity analysis instead of a global one.

It is also important to note that a more adequate uncertainty characterization of the thermal modelling approach requires an independent effort out of the scope of the current research. As we have been mentioning in previous responses, order reduction modelling is one of the most recent and promising approaches to achieve such systematic sensitivity analyses.

To the main comment by reviewer#1, the range of variations in the thermal properties of the crustal layers could hardly explain having a systematically different thermal configuration, while at the same time fitting the measured temperature (see supplementary Figure S1). This is the main reason for our choice to discuss in detail only the best fitting model, while still discussing the mismatch of all the other members of our analysis as SI materials.

In order to better clarify the approach, we have noted in the text that:
"The model fitting approach followed, for simplicity, a local optimization in which the initial average values of some thermal properties were tuned only if necessary, in order to reproduce with minimum misfit the independent measurements of temperatures in boreholes (as discussed in Sect 4.1)."

Line 238: I still think that "Depth of Crustal Earthquakes" is clearer than "Crustal Seismogenic Depth", but I'll leave it up to the authors to decide what they want to use.

Response:
There is no agreement in the literature on how this depth should be called. For example, Zhang et al. (2022) refer to the "lower seismogenic depth" in the title but start their introduction as "We investigate crustal seismogenic depths of the western United States [...]".

Since, to be precise, we are mapping D90, we have now mentioned already in the abstract that CSD is "mapped as D90, the 90% percentile of hypocentral depths".

Line 246-247: You can remove the citations for Wimpenny et al., 2018 and Wimpenny 2022, as their results are included in the gWFM catalogue that you have already cited.

Response:
The only data of the gWFM catalogue within the limits of our study are those provided by Wimpenny et al. (2018) and Wimpenny (2022), so it seems better to keep mentioning these original sources too.

Line 264: A better citation than Wimpenny (2022) here would be something like McCaffrey and Abers (1988) or Nabalek (1984), as these were really the papers that demonstrated how waveform-modelling methods provided more accurate estimates of earthquake centroid depths.

Response:
Thank you for this recommendation. We replaced the citation to Wimpenny (2022) with McCaffrey and Abers (1988) and Nabalek (1984).

Line 312: Change to "… first converted to Mw using the relations…"

Response:
Changed.

Line 313: I would recommend just leaving this sentence as "using the relations detailed in Text S6." – all the citations are difficult to follow and can be found in the Supplement.

Response:
Thank you, we modified the text accordingly. We also removed the references to Di Giacomo et al. (2015) and Salazar et al. (2013), which are no longer cited in the main text and can be found in the supplement.

Line 409-410: I would recommend removing the point about whether the crust and lithospheric mantle are coupled or not – I don't see how the lack of seismicity can tell us that.

Response:
The reviewer is right in that as it is written the sentence leads to some confusion and misunderstanding in the reader. What we wanted to highlight is that a cold lithosphere is also mechanically compliant, a typical feature of long-lived geological features as cratons.

However, considering also the comment from reviewer#2, we decided to remove the interpretation about crust-mantle coupling.

Line 469: Should be "… towards lower hypocentral temperatures in the Falcon Basin"?

Response:
Thank you, we modified the text accordingly.

Line 493: Should this say "regional average seismogenic depth" as opposed to just "regional seismogenic depth"?

Response:
Yes, we added it to the text.

Line 495-500: The authors argue that the Murindo earthquake had a hypocentre near the base of the seismogenic layer and that the earthquake ruptured to the surface. Can the authors cite the evidence that this earthquake did in fact rupture to the surface? From what I can tell from the literature, there were earthquake environmental effects, but no recorded primary surface ruptures.

Response:
Thanks for this point. We have now softened the wording, indicating that "Its geological effects **suggest** a surface rupture exceeding 100 km in length" (former line 499) and "the mainshock **most likely** ruptured…" (former line 502).

Mosquera-Machado et al. (2009) reviewed that "No evidence of surface faulting has been reported in the literature." but "surface faulting probably occurred". "There was great uncertainty about the exact location and rupture length of the causative faults for the earthquake sequence, because the tropical fluvial setting and difficult access in the epicentral area do [sic] not allow [a] reasonably accurate identification of earthquake fault scarps."

Nevertheless, when listing the ground effects at the Murindó site they indeed described "Two east–west oval sectors on each side of the [Murindó] fault, uplift in the west were sand and ground water were ejected, and subsidence to the east". This evidences coseismic ground deformation at both sides of the fault, so most likely the rupture reached the ground or almost did.

Line 510: Maybe change the subtitle to: "Temperature at the base of the seismogenic crust"?

Response:
Since we describe the results of depths too, we prefer not to remove this word from the subtitle. We have slightly modified it to "Depths and temperatures of the base of the seismogenic crust".

Line 515: The blue polygon in Figure 8b is very difficult to see, at least on my screen. Maybe make it clearer by making the line thicker, or putting a white background behind it?

Response:

We have made the blue lines thicker, both in Figure 8 and in Supplementary Figure S8.

Line 515: It is not entirely clear what "sheared continental affinity" actually means. Why is the inference that it has been sheared relevant? Is it not just that the terrane is formed from continentally-derived rocks that's relevant?

Response:

We complemented the description of the "sheared continental margin" terrane in the study area as follows:

"The collision of the C-LIP with the continental margin of South America defined not only a broad sheared margin **(with remnants of continental slivers and ophiolitic sutures, Kennan and Pindell, 2009)**, but also extended fragments of mafic and ultramafic rocks associated to mantle-plume processes, and emplaced oceanic crust and remnants of island arcs (see Boschman et al., 2014; Kennan and Pindell, 2009; Montes et al., 2019)."

This said, for the interpretation of the results it is important to note the presence of high-density rocks within this sheared continental terrane.

Figure 8: There are a lot of references in the text to fault zones that are shown on Figure 3, but not on Figure 8. Because the D90 information is on Figure 8, then it is difficult to follow exactly where the authors are referring to in the text without having both Figure 3 and Figure 8 next to one another. I would recommend either adding the relevant fault zone names, or adding some annotations. They could also remove the fault zones that are not relevant from the line map to focus the readers eye on the relevant information. These are just some suggestions.

Response:
We agree that the display is not ideal, but adding names or annotations to Fig. 8 would clutter it excessively, because of its reduced size. We expect that the interested reader will be able to familiarize herself or himself with the names annotated in earlier figures (1 and 3).

Line 512-515: Here the authors say that there is "… a transition from shallow D90 depths and cold temperature associated to [sic] oceanic terranes and island arc affinity in Western South America, towards deeper and hotter values in terranes with a more sheared continental affinity in the east." – The authors need to clarify exactly which part of their study region "west" and "east" are referring to here. The trends they point out are subtle and only hold in certain parts of the map area. For example, if you look at a west-east traverse for two profiles extracted from Figure 8a (see Figure R1 below), for the northern most profile the D90 gets deeper beneath the Middle Magdalena Basin compared to the Western Cordillera. However, for the profile further south the D90 is deeper beneath the Western Cordillera than it is beneath the Middle Magdalena Basin (MMB).

Response:
This trend should now be clearer to spot in the figures, thanks to the improvement of the visibility of the terrane contours in figures 8 and S8. The location mentioned by the reviewer seems actually the only, small, exception to the general trend.

To further clarify the text, we have rewritten slightly former lines 513-515, specifying that "Our results suggest a trend from shallower D90 depths and colder temperatures in the Greater Panama terrane (oceanic, with island arc affinity) towards deeper and hotter values in the sheared continental margin (Fig 1b and blue polygons in Fig. 8b)."

Line 520-522: Can I suggest re-phrasing these sentences to: "We interpret the observed variability in D90 between the Central and Eastern Cordilleras, and the Middle Magdalena Basin, to suggest there is significant rheological contrasts between these areas. These major terranes are likely separated by crustal-scale faults." At the moment, I find it hard to understand what these sentences are trying to say.

Response:
Thank you. The new paragraph says:

"We interpret that the observed variability in D90 between the Central and Eastern Cordilleras and the Middle Magdalena Basin (MMB, Fig. 1b) evidences significant rheological contrasts between these areas. These major terranes are likely separated by crustal-scale faults (Kennan and Pindell, 2009)."

Line 547: Spelling error? Should say "CSD" not "SCD".

Response:
Yes, it was a typo. Thank you. We fixed it.

Line 628: Why can a hot upper mantle explain why there are earthquakes at high temperatures? The hot upper mantle just explains why the temperature is high, not necessarily why there is seismicity in the rocks at these high temperatures. For example, Iceland has a hot upper mantle, has a predominantly mafic crust because it has formed from MOR volcanism, but there is very little seismicity deeper than 10-15 km (if any).

Response:
Thanks for pointing this out. Our reasoning was that, having a mafic lower crust, seismicity could occur at higher temperatures as they have a deeper brittle-ductile transition. In this case, the high temperatures are supported by a relatively hot upper mantle. Of course, "hot" here does not mean as hot as Iceland (which is a pretty unique case).

Nevertheless, we decided to remove the "hot upper mantle" sentence from the paragraph, aiming to avoid misunderstandings.

Reply to comment by Reviewer #2

'The crustal earthquakes occur at locations with a mean geothermal gradient of 19.4±1.23 °C/km-1, preferentially clustering in specific zones, e.g. in the North Andes block and the Panama microplate. Seismicity is almost absent in cold lithospheric areas such as the Guyana craton and the Caribbean Large Igneous Plateau. Such correlation indirectly suggests that in these places, the crust and lithospheric mantle may be strongly coupled, and therefore, the differential stress is not high enough to deform the crust in a brittle regime.'

First, better say 19.4±1.2 instead of 19.4±1.23. Second, following the Byerlee law, the brittle strength does not depend on temperature and therefore the same stress is needed to produce brittle failure and earthquakes. Instead I find a more logic explanation that the cold undeformed areas (e.g. the Guyana craton) may have less inhereted structural weakeness (faults) and are therefore more difficult to localize strain. Also compositional differences can have an effect. For example cratons are usually to be compositionally strengthened (via loss of volatiles).
Overall I would suggest 'relaxing' their interpretation, or adding alternative explanation.

Response:
Thank you very much for your comment. We have modified the paragraph following your recommendation:

"The crustal earthquakes occur at locations with a mean geothermal gradient of 19.4±1.2 °C/km-1, preferentially clustering in specific zones, e.g. in the North Andes block and the Panama microplate. Seismicity is almost absent in cold lithospheric areas such as the Guyana craton and the Caribbean Large Igneous Plateau. This again, is an indication that a 1D geotherm approximation will not be robust enough to model the thermal configuration of the heterogeneous study area."

References not cited in the manuscript

Degen, D., Cacace, M. & Wellmann, F. 3D multi-physics uncertainty quantification using physics-based machine learning. Sci Rep 12, 17491 (2022). https://doi.org/10.1038/s41598-022-21739-7

Degen, D., Veroy, K., Scheck-Wenderoth, M. et al. Crustal-scale thermal models: revisiting the influence of deep boundary conditions. Environ Earth Sci 81, 88 (2022). https://doi.org/10.1007/s12665-022-10202-5