Re-review of the paper: "*Thermal structure of the southern Caribbean and NW South America: implications for seismogenesis*" by Gómez-García et al.,

Reviewer: Dr Sam Wimpenny (University of Bristol, UK)

Thank you to the authors for engaging constructively with the first round of reviews and for their responses.

The edits the authors have made have clarified how the uncertainties associated with the earthquake depth estimates might translate into uncertainties in the estimated temperatures at the earthquake hypocentral locations. These new additions do not significantly change the relationships between earthquake depths and temperatures originally presented.

There are several line-by-line comments that need to be addressed. The text can be quite difficult to follow sometimes, with incomplete reasoning (as pointed out by Reviewer 2 in the original reviews), meaning I had to re-read sections multiple times to understand the arguments being presented. I have tried to highlight these in the review.

The only technical comment I have is to include some logic for what the authors think the possible range is for the temperature estimates at each earthquake hypocentral depth.

I recommend the manuscript could be accepted following these minor revisions.

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.

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.

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.

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?

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.

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".

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?

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

Line 41: Change "assemblies" to "assemblages"

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.

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)?

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].

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

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

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.

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"

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.

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?

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

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"?

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.

Line 105: "results" not "resulted".

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

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?

Line 113: "remainder" not "remaining".

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

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.

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.

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.

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.

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.

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

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.

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.

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

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

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.

Line 510: Maybe change the subtitle to: “Temperature at 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?

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?

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.

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).

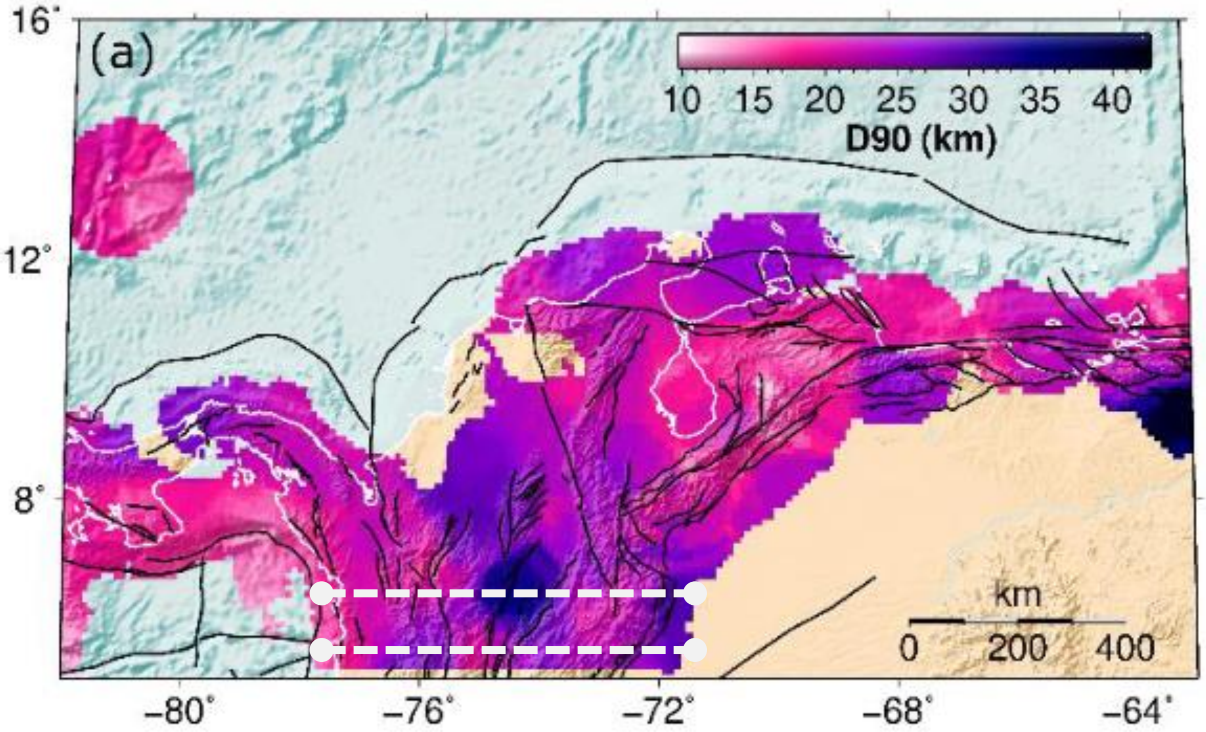

Figure R1: Figure 8a from the manuscript with two E-W profiles shown as white dashed lines.

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.

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

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).