Responses to reviewers:

Note that original comments are in black and responses are in blue and in italics.

Responses to reviewer #2: Frédéric Mouthereau

I still have some comments since I have noticed that some of the points Kim Welford said she will address in the revised ms are not in fact.

I would like to apologize for any confusion that arose from the responses during the last round of review as I am new to the Solid Earth procedures. I continued to work on the revision, and the corresponding rebuttal letter, after the online discussion board had been closed. In the final rebuttal letter that was submitted with the revised manuscript, I amended some of my original responses relative to the online discussion but had no means of editing the online responses once the rebuttal letter was finalized.

Kim's answer
"1) I also note that the Labrador Sea is presented to occur between Archean and Proterozoic crusts. But the lithosphere below Labrador has lost its archean root as it has been modified during Paleoproterozoic and the mantle is much younger thinned, and refertilized during the Neo-Proterozoic (Connelly et al., 2000; Tappe et al., 2006). This might be keys to explain why the rifting took this direction.

Yes. This is a compelling point that I will incorporate into the discussion."

But I don't see this point discussed in the revised ms. Please incorporate it clearly.

As updated in the final rebuttal letter from the last round, I commented that this is a compelling point but it was not clear to me how this information could be extrapolated oceanward into this synthesis and used to explain the COTZ characteristics. As such, I decided to leave discussion of causative mechanisms for Labrador Sea rifting out of the manuscript, particularly since incorporation of the two suggested references would necessitate an even broader discussion of onshore versus offshore controlling structures that would need to be expanded to all of the margins under consideration. Instead, I have chosen to reference the Nature Communications paper by Gouiza and Naliboff (2021) as they address the influence of inheritance in the Labrador Sea rifting segmentation offshore.

also

Kim's answer :
"In addition, the drawing of the Variscan front to the North looks fine but the Rheic suture is to the south so Avalonia should also extend to the south of the Variscan front. this problem is inherent to the presentation of both deformed orogenic domains and the crustal affinity domain. You can refer to Mouthereau et al. (2021) for the mapping in this area and perhaps find useful the description of the mantle evolution in that domain.

Yes. I agree that Avalonia lies south of the Variscan front and as you mentioned, it is challenging to show both the orogenic domains and the crustal affinities within one map without making it significantly more complicated. I will refer to your 2021 paper for ideas on how to do this most effectively.

But I don't see reference to this paper in the revised ms.

As stated in the final rebuttal letter from the last round, yes, I agree that Avalonia lies south of the Variscan front and as you mentioned, it is challenging to show both the orogenic domains and the crustal affinities within one map without making it significantly more complicated. Ultimately, having played with a few *variants, I decided to not show the southern boundary of Avalonia on the European margin because it makes the figure too complicated and the offshore extrapolations of the boundaries are simply unknown. I have however now referenced Mouthereau et al. (2021) when mentioning the importance of considering mantle rheology and fertility during rifting.*

Regarding the point line 600 in the conclusions section. I suggest to add "composition" or "petrological" and "continental" because mechanical inheritance is not restricted to rheology but to density (thermally and compositionally related) and isostatic parameters more broadly.

.... of the southern North Atlantic show symmetric lateral extents north of the Newfoundland-Azores Fracture Zone all the way to the Labrador Sea but a stark asymmetry exists further south, possibly due to fundamental differences in rifting mechanisms, rifting velocities, the thermal regime, and/or rheological and petrological variations of the continental lithosphere.

Agree. The text has been edited as suggested.

Responses to reviewer #3: Georgios-Pavlos Farangitakis

Reviewer #3 had no further suggestions and was happy for the revised manuscript to be accepted as is. I thank the reviewer for the time that he dedicated to reviewing the revised manuscript.

Responses to reviewer #4: Anonymous

General comments:
This is a very useful, well written and illustrated manuscript that merits to be published. Indeed, as pointed out by the author, such a detailed synthesis of refraction seismic lines across he COTZ was missing for the southern Northern Atlantic, despite being one of the type localities of magma-poor margins.

I would like to thank the fourth anonymous reviewer for joining in the review process mid-stream, after one full round had already been completed.

Overall, I do not have major comments and I think that the manuscript can be published with minor modifications. Below I have some comments that being addressed, could clarify some of the weak points of the work.

1) It would be helpful to have a paragraph focusing on the Moho in the COTZ and explain the velocity characteristics of this structure and the properties of related rocks (a figure could be very helpful)

The previously revised manuscript does include a section of text about how the Moho was defined in the synthesis. To address this comment from reviewer #4, I have added two sentences about the velocity characteristics of the Moho in that existing section in this latest revision. Specifically:

For the PmP reflections to be clear during RWAR profiling, the velocity contrast between the lower crust and the upper mantle must be large enough to generate a detectable reflection. Typically, the velocity contrast across the Moho corresponds to 6.9 km/s for the lower crust and to 8 km/s for unaltered mantle, with mantle velocities decreasing with increasing degrees of serpentinization.

2) The term "transitional crust" used for instance on l.139, does not really make sense, since there is no transition between continental rocks, serpentinite and basalts. While velocities and other geophysical parameters may show transitions, rocks don't, and when we talk about crust, we often refer to rocks. The

same applies to “homogeneous” oceanic crust (line 142). From a geological point of view a crust is not “homogeneous”. Thus, would not use such a term

Within the rifted margin community, the term “transitional crust” is very much a standard term used. It refers to crust that may consist of varying amounts of different crustal types (e.g., thinned continental crust interspersed with pockets of serpentinized peridotites or magmatic products). In the existing text, it is explained: “Transitional crust can refer to hyperextended continental crust, exhumed mantle, embryonic oceanic crust, and any combination thereof, making the demarcation of the limits of transition zones challenging”. As such, to conform to the existing literature, I have not removed the use of this term.

In terms of homogeneous oceanic crust, that is fair. I have replaced “homogeneous” with “classic Penrose-type” oceanic crust and provided a reference.

3) Definitions: Here the COTZ is defined and subdivided in three subunits (a), (b) and (c) (see lines 154 to 156). However, it is not clear how the continentward limit of the COTZ (and domain (a)) is defined. Is it the necking zone, or the coupling point? This is important, since you define a width of the COTZ; so where do you start measuring. It is also not clear how you define the limit between domains (a) and (b). Is it the ECC of Nirrengarten et al. 2018?). If so, be more explicit. And finally, the limit between domain (b) and (c) needs to be defined as well as the limit of first oceanic crust. In the present version, definitions are not clear and not applied rigorously in the Figures 3 ot 6. This is clearly a weakness of the present version of the manuscript.

As explained in the text, I define the landward limit of the COTZ as corresponding to “hyperextended continental crust underlain by serpentinized mantle”. This does not always correspond to the necking zone but it can. As you mention, the coupling point is perhaps a better definition so I have now stated that in the text explicitly. Subunit (b) only exists where mantle is interpreted to be exhumed and its landward limit would be the ECC (this has now been added to the text). Finally, the boundary between subunits (b) and (c) occurs where exhumed mantle ends and anomalously thin oceanic crust begins. While this boundary will generally correspond to the LaLOC according to the definition of Sauter et al. (2023) used herein, the oceanward limit of the COTZ may be further oceanward if the oceanic crust has not reached normal thickness, or if the unexhumed serpentinized mantle extends further oceanward than the LaLOC. I rechecked the COTZ extents shown in Figures 3 to 6 and found that all but profile 8 did conform to the standardized COTZ definition introduced here. The COTZ extent for profile 8 has now been updated in Figures 4 and 8 to conform to the new definition.

4) It is not clear how domains of serpentinized mantle underlying thinned crust are defined. What are the criteria used?

Serpentinized mantle underlying thinned crust is defined solely based on the presence of upper mantle velocities lower than 8 km/s. This has been made clearer in the latest revision of the manuscript.

5) It would be useful to show the locations of drill holes penetrating basement and showing the type of rocks they recovered (this is the strongest support for your interpretation)

For the southern North Atlantic, there are very, very few drill holes that penetrate basement in the COTZ. These are effectively limited to the Iberian margin, with only one drill hole on the Newfoundland margin. Consequently, a synthesis of COTZ across the entire southern North Atlantic can only be accomplished using geophysical datasets, such as the velocity models investigated herein. Given their limited contribution to this synthesis, and the fact that the maps are already quite busy, I have not plotted basement drill holes for this study. Where drilling information has influenced the velocity model building, it is already explained in the corresponding published works.

6) Serpentinization is not only dependent on the embrittlement, but also depends on the temperature (should be ≤ 300 to 350°C. This may be added in line 428 and you may add a reference

Agree. Edit and reference added as suggested (Bonatti et al., 1984).

7) You discuss importance of rates (lines 458) during rifting (how are they determined? It's difficult for rifting, there are no time lines such as magnetic lineations!! You do not discuss importance of inherited depleted mantle. This may be useful. See work of Chenin et al.

Agree. It is difficult to be quantitative in this respect. Rifting velocity inferences are currently unconstrained, other than from insights from numerical modelling and deformable plate reconstructions. And yes, inherited mantle fertility is important for understanding rifting processes. This has already been mentioned in the revised text but additional references have now been added (particularly to the work by Chenin and others). Unfortunately, the mantle fertility question cannot be addressed with the present synthesis because the RWAR velocity models only provide limited information about the uppermost mantle.

8) Figures:
• Moho: would use dashed or dotted lines for places where MOHO is not defined and assumed to be a hydration front due to serpentinization. In the present version you use black lines for many other interfaces, and it is not clear how and where you put Moho and with what criteria

This is a compelling point, although difficult to illustrate in the velocity model representations. As mentioned in the text, the Moho is defined by a thicker line where supported by wide-angle reflections in the published works. Where this is not so, the black lines are often just corresponding to model boundaries and not hydration fronts. The Moho interpretations come directly from the published works included in the synthesis and are not reassigned herein so I prefer not to alter the models as they currently appear in the literature.

Fig. 3 is the domain of serpentinized mantle in section 3 realistic? Can serpentinization reach so deep below top basement

The serpentinized mantle in section 3 is extracted directly from the published works and I do not have access to the supporting dataset with which to assess its plausibility. Without extra information, I cannot alter the published model.

Fig. 4 how could you define serpentinite underneath normal oceanic crust in section 8.

As mentioned above, unexhumed serpentinized mantle is defined solely based on the presence of upper mantle velocities lower than 8 km/s. The serpentinite interpretation for the HVLC in section 8 is extracted directly from Thinon et al., 2003.

Fig. 5 not clear why similar velocities result in different interpretations and if Moho in section 14 is really determined

Velocities are unfortunately not unique to specific crustal types, although velocity gradients and the regional context can help minimize uncertainties in their interpretation. As for the Moho in section 14, the PmP reflected arrivals are simultaneously inverted with the refracted arrivals by Merino et al., 2021, so yes, their Moho is geophysically determined.

Fig. 6 the result that there is no mantle exhumation on the Moroccan margin are an interpretation, reflection seismic data show locally evidence for mantle exhumation in the northern Moroccan margin; so would not be too dogmatic on this point.

I agree on this point. The verbiage when discussing the Moroccan margin is specifically made non-definitive for this reason. The text in the revised manuscript stated "There is no evidence of serpentinized mantle or of exhumed mantle in the basement along the Moroccan margin based on models generated to date". I have now specified in the newly revised text that I am referring to RWAR models generated to date. As an aside, I have recently performed some gravity inversions (not yet published) which seem to suggest greater complexity than is captured in the published velocity models. Unfortunately, there are no RWAR velocity models indicating otherwise as of yet.

Fig. 8 why not aligning along LaLOC. At the present version the COTZ goes oceanwards to the LaLOC (see lines Nova Scotia), is this not questioning the location of the COTZ or the LaLOC on these lines? Why do you don't put LaLOC in section 7 (you show a blue crust)?

I chose to align the sections according to the oceanward limit of their COTZ extent, and not LaLOC because LaLOC is based on an interpretation of the basement as boxy oceanic crust (regardless of its thickness, as suggested by Sauter et al. (2023)). As mentioned above, the COTZ can extend further oceanward of LaLOC. For instance, for profile 19, LaLOC is landward of the extent of unexhumed serpentinized mantle. As such, the COTZ is defined according to the unexhumed serpentinized mantle rather than the LaLOC.

For section 7, the authors never report finding evidence of typical oceanic crust so effectively the location of LaLOC there is undefined and I show the colour as a gradient from green to blue to show that uncertainty.