Solid Earth Discussions

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Comment on se-2020-209

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Community comment on "Crustal structure of the East African Limpopo margin, a strike-slip rifted corridor along the continental Mozambique Coastal Plain and North Natal Valley" by Mikael Evain et al., Solid Earth Discuss., https://doi.org/10.5194/se-2020-209-CC1, 2021

The study is about the crustal structure at the southern Mozambican margin and its geodynamic evolution during the initial Gondwana break-up. Marine seismic refraction data are investigated to depict the velocity structure along a single profile at the transition from the Mozambique Basin (MB) towards the Northern Natal Valley (NNV). Seismic reflection and gravity data are incorporated for improved interpretation of the sediment sequence and model verification, respectively. The authors suggest the Northern Natal Valley to be floored by thick unrifted continental crust and the presence of thinned continental crust east of the "Limpopo margin" (LM), where ductile shearing caused the flow of lower crust from the Mozambique Coastal Plains (MCP) and NNV towards an inferred "corridor" of anomalous crust. These crustal deformations are proposed to be accompanied by intense magmatism during initial N-S opening of the Africa-Antarctica Corridor from 155 Ma on.

The presented new seismic and gravity data is recorded along a single profile in the scope of the PAMELA project (seven refraction profiles in total). The data and applied methods are suitable to reveal new evidence on the crustal structure and geodynamic evolution of the southern Mozambican margin, which is still under debate.

However, reading the manuscript, gives the impression that the data interpretation is somehow biased, without discussing alternative approaches. My comments to this manuscript aim on highlighting author statements which seem to me not adequately discussed or contradict already existing constraints. Simultaneously, I will provide alternative interpretations, which should be accounted for and unbiasedly discussed in an updated version of the manuscript. Furthermore, I provide hints for an improved structuring of the paper.

Main issues:

Interpretation of the volcanic layers SV1 and SV2: I agree on the suggested different reflection patterns of the acoustic basement at Karoo lavas with the underlying volcano-sedimentary sequence compared to the reflection pattern at volcanic/oceanic crust. Consequently, it is correct, that the identification of profile segments of different seismic signature at the acoustic basement can contribute to the discussion on a segment's crustal nature, as exemplarily the presence of the pre-break-up volcano sedimentary sequence of the Karoo Group is compatible with continental crust. However, regarding the different reflection pattern of the acoustic basement at the MZ3 profile, I partly disagree to the interpretation of the authors. From my point of view, seismic signature of the acoustic basement is of high amplitude and similar reflection pattern for areas northeast of OBS26 (basement high) as well as for the entire area southwest of OBS09 (partially influenced by the younger volcanism at the Almirante Leite Ridge), which contrasts the opinion of the authors. Same high amplitude seismic signature is observed at main parts of the NNV south of OBS08 at profile MZ6 (until profile end) and southeast of OBS14 at profile MZ2 (previous reviews). Reflection characteristics of the acoustic basement in these areas is partly rugged and hummocky and intra-basement reflections are mostly chaotic and seldomly indicate short subparallel reflections. In close vicinity to the study area, such a seismic signature and velocities (SV1 and SV2 of this manuscript) are observed at the upper basement layer of the southern Mozambique Ridge (MozR) (Gohl et al. 2011, Fischer et al. 2016) and at several other oceanic plateaus (e.g. Agulhas Plateau, Shatsky Rise, Manihiki Plateau). In contrast, clear subparallel and dipping reflectors can be observed along profile MZ3 between OBS station 10 to 21 (this manuscript) as well as at the MCP and the northwestern part of the NNV. For these areas of clear parallel reflections in the uppermost layer of the acoustic basement I agree to the presence of the volcano-sedimentary Karoo Group (also drilled by several boreholes in this area). According to the study of Mueller & Jokat (2019) the investigated MZ3 profile runs mostly just southeast to their supposed COB (Scenario 2, Sc2) and crosses the COB at the most southeastern edge of the Inharrime Ridge (OBS 11-21), which they consider to be floored by thinned continental crust with a massive magmatic underplate, allowing the presence of Karoo Group sediments in this segment (OBS10 to 25) and explaining the different seismic signatures of the acoustic basement along the MZ3 profile. Apart from that, picking two separate layers (SV1 and SV2) in this segment seems to me quite ambitious, due to the partly hidden arrivals of SV2 (e.g. Fig. 4, OBS 25 southwest direction) and smooth transition to Pg1 (e.g. Fig. 5, OBS 11 northwest direction). In general, the described different characteristics of the upper acoustic basement and the similarities to other studies in the vicinity of the study area should be described in the manuscript and it needs to be indicated that this alternative interpretation, questions your conclusion of "undoubtedly" "continental nature of the NNV" (line 425p).

Comparison and discussion of the velocity structure: Unfortunately, the depicted velocity structure is not well discussed and is biased towards continental crust. The authors extracted 1D velocity-depth profiles of the NNV and compared these to different types of continental crust and to other profiles of the PAMELA project, revealing that crustal velocities of the NNV only hardly fit to extended or rifted continental crust (extracted velocities are at outer/faster edge). The presented comparison of the NNV crust to standard oceanic crust (as usually observed far inside oceanic basins) seems to me not very meaningful as this scenario is of course not likely and of course does not reveal similarities. However, unfortunately the authors do not compare the NNV's velocity structure to any of the already existing seismic refraction profiles at the Mozambican margin (Leinweber et al. 2013, Mueller et al. 2016, Mueller & Jokat 2017), the MozR (Gohl et al. 2011) or the conjugate Antarctic margin (Jokat et al 2004)! From my point of view, the velocity structure of the main part of the NNV should additionally be compared to entire volcanic features/oceanic plateaus (like the MozR, located just south of the NNV, or the Iceland-Greenland Ridge or the 90ºE Ridge, …). I've added such a comparison to this comment. The layers SV1 and SV2 at the main NNV are assigned as basement layer, based on the described seismic evidences in the previous bullet point. The comparison shows clear similarities for the velocity structure of the NNV to oceanic volcanic structures, with typical (i) velocity jumps in the upper crust/layer 2 (your SV1, SV2 and Pg1), (ii) monotonous velocity increase in the lower crust/layer 3 and (iii) a distinct high-velocity lower crustal body (HVLCB) (Carlson et al. 1980; Mueller et al. 2016). A comparison to the conjugate margin in the Lazarev Sea in Antarctica (Jokat et al. 2004) reveals very similar upper basement velocities for the Explora Wedge and a general similar velocity structure with a HVLCB, leading Jokat et al. (2004) to suggest the segments composition to be of only volcanic material. Furthermore, the seismic refraction study at the MozR (Gohl et al. 2011) reveals as well a HVLCB of similar thickness and velocity. Apart from the discussion on the crustal nature in the NNV, the authors declare the velocity structure at the basement high at the northeastern end of the profile and at the "corridor" as indicative for thinned continental crust, based on the comparison to standard oceanic crust, vz-profiles of other studies of the PAMELA project and a slightly thicker lower crust. In this regard, I suggest again a comparison to the conjugate Lazarev Sea in Antarctica. Here, profile 96110 (Jokat et al. 2004) reveals for the northern part, in the area of well-expressed magnetic reversal anomalies the presence of oceanic crust, which is as well characterized by an 8 km thick lower crust of same velocity. From my point of view, such a similarity should be mentioned and allows the alternative interpretation of oceanic crust at OBS32 to 28 of profile MZ3 (maybe up to OBS 27 at the basement high).

Origin of the high-velocity lower crustal body (HVLCB): The authors interpret velocities higher than 7.0 km/s as lower continental crust with high magmatic content. Additionally, they write that "undoubtedly" (line 486) lower continental crust flow from the MCP, NNV and LM towards the "corridor" of anomalous crust and cause the slightly thickened lower crust in this area. This is a speculation and can not be ruled out, although wondering why the lower crust in the corridor shows velocity lower than 7 km/s (like at the conjugate margin) and contradictorily the lower flowed crust at the MCP, NNV and LM has velocities higher than 7 km/s? However, from my point of view, lower crustal boudinage might be only of minor or local order during initial break-up at this margin. Discussion on the HVLCB should address the two different expressions of HVLCB in the study area, and requires a discussion in the light of earlier refracted studies in the vicinity of the study area and at the conjugate margin. The profile MZ6 (Moulin et al. 2020) shows for km 250-430 a pronounced velocity increase in the lower crust with 7.4-7.5 km/s. This HVLCB is exactly of same length and velocity as observed at the Central Mozambican margin (Leinweber et al. 2013, Mueller & Jokat 2017). A HVLCB of this type is a common feature at volcanic passive margins and underlies the COB at the Central Mozambican margin, where the HVLCB's origin is interpreted in terms of magmatic underplating. Along profile MZ6 the HVLCB (velocity up to 7.4-7.5 km/s) underlies the transition from the MCP and most northern part of the NNV towards the main part of the NNV, with its different seismic signature of the acoustic basement. This distinct similarity requires the discussion of a possible COB in this segment. A similar velocity structure of the HVLCB is only tentatively interpreted below the volcanic flows/SDRs of the Explora Wedge at the conjugate Antarctic margin (covered by ice) and are as well interpreted to mark the COB. Regarding the second type of HVLCB (velocities of 7.0 to 7.3 km/s and thickness up to 16 km) below the NNV, it's worth discussing that the same HVLCB is observed at the oceanic plateau of the MozR, just south of the study area. Gohl et al. (2011) suggest the HVLCB to originate by the massive addition of mantle-derived magma, as observed at several oceanic plateaus. The highlighted similarities in the general velocity structure (previous bullet point) and observed HVLCBs in the vicinity of the study area and at the conjugate margin, need to be discussed in the manuscript and demonstrate an alternative interpretation of the derived velocity structure and crustal nature at the NNV.

Density modelling: I appreciate that the authors are incorporating gravity modelling for cross-checking their velocity model. However, I do not agree to the declared "well" (line 262) fit. Obviously, there is a misfit of up to 40 mGal at profile km 310-400 and up to 20 mGal at km 120-250. Such a misfit, should motivate for checking the modelled velocities, interfaces and velocity-density conversion. Furthermore, you should discuss possible reasons for the preferred low mantle density, as this significantly differs to other combined seismic/gravity studies at the Mozambican and conjugate Antarctic margin (upper mantle densities of about 3.3 g/cm^3). As an idea for a more significant cross-check of your velocity models, I suggest performing a 3D gravity modelling (separate study and not to be included in this manuscript). You have a great database of seismic data, which is able to provide important constraints. Outcome of the 3D gravity model will be new insights into the crustal setting of the MCP (parts which are not covered by your seismic study), cross-check of your velocity profiles and an improved understanding about the crustal setting in the transition from the MCP to the cratons and orogenic belts in the west and towards the Mozambique Basin in the east.

Proposed geodynamic evolution and pre-drift constellation: The proposed "loose fit" break-up model (Thompson 2017, Thompson et al. 2019) might be compatible with the authors proposed crustal composition and nature at the MCP and NNV and presents a scenario which is worth discussing. However, based on the incomplete discussion of the seismic data without highlighting alternative possibilities, it seems to me that the data interpretation is biased to support the in advance published break-up model of Thompson (2017) and Thompson et al. (2019) (same research group). I would highly appreciate if prior to geodynamic modelling, at first an unbiased study of the seismic refraction data is performed and subsequently(!) a break-up model is developed. Besides declaring the own results as "undoubtedly", it would be fair to mention, that the authors postulated break-up scenario does not account for (i) identified alignments of cross-continent-wide tectonic features observed in magnetic data (Mueller & Jokat 2019), (ii) drift-related fractures traced in gravity and magnetic data until closest to the conjugate margins depicting an early NW-SE rifting/drifting (prior 155 Ma), (iii) magnetic spreading anomalies dated to magnetic field reversal isochrones of up to M38 (ca. 164 Ma) in the Mozambique Basin, (iv) significant difference in the length of the MCP-MozR rift segment to the conjugate rift segment in the Lazarev Sea, (v) alternative plausible interpretation of the crustal composition and nature of the NNV to thickened volcanic/oceanic crust, (vi) continuous magnetic lineations at the NNV, which are parallel to magnetic spreading anomalies in the Mozambique Basin (of proposed same age), (vii) … As previously outlined, the authors break-up model is worth discussing, but the seismic data investigation allows an alternative interpretation, which does not rule out a tight Gondwana fit. Exemplarily, the break-up model of Mueller & Jokat (2019) suggests a tight Gondwana fit, allowing thinned continental crust with a massive magmatic underplate at the southern MCP and at the northern part of the NNV and thick volcanic/oceanic crust at the NNV and MozR, by simultaneously accounting for the exemplarily mentioned additional constraints. Finally, I doubt, that the studies of Domingues et al. (2016) and Hanyu et al. (2017) should be graded as a "strong support" (line 52p) for a "loose fit" scenario and break-up model of the authors. Domingues et al. (2016) compare ambient noise Rayleigh wave group velocity curves in the MCP to "typical oceanic crust" and standard continental crust. Not surprising that a comparison to normal oceanic crust of 6-8 km thickness does not reveal a similarity. None of the research studies, which prefer a tight Gondwana fit, suggest the presence of normal oceanic crust at the MCP. In the following, Domingues et al. (2016) note in general "much slower velocities at short periods than typical continental crust-type theoretical curves" and "path coverage in the MCP region is poor for long periods and at these periods the measurements present higher uncertainties", leading them to suggest the presence of a 20-30 km thick "transitional crust from continental to oceanic" at the MCP. In contrast, the study of Hanyu et al. (2017) deals with gravity and magnetic data at the Natal Valley and MozR and assume the presence of highly stretched continental crust. Suggested crustal thickness at the MCP and NNV is 11-14 km, which is 2 to 3 times thinner than the crustal thickness suggested by the PAMELA project and clearly contradicts the results of the entire refraction study, which actually should be discussed in the manuscript. Furthermore, the trends of the magnetic anomaly lineations are partly adjusted and disproved by the incorporation of additional shipborne magnetic data in the NNV and the compilation of a consistent magnetic anomaly database and map for this area (Mueller & Jokat, 2019). Therefore, from my point of view, these studies should not be misused as a "strong support" for a loose fit break-up model, as these studies declare as well the ambiguous character of their interpretation.

Some more detailed comments:

- To support their interpretation, the authors cite at least 5 unpublished studies of the same research group. Since a reader and reviewer cannot check the content of these studies it is good scientific practice not to reference them for supporting a far-reaching interpretation.
- Through the text the authors give the impression that the presented data and study cover the MCP, although information is only gathered at is rims. This should be clearly highlighted.
- The paper needs a major reorganization. Within the "Methods" chapter 3 occurs a mix of methods results and interpretation. Chapter 4 is a mixture of results and discussion. A clear distinction is required for publishing a scientific study and would increase the readability of the manuscript.
- Identified phases in seismic refraction data cannot be directly labelled as continental at their first mention, as their investigation and interpretation is the main objective of this research study, the crustal nature of the research area is still under debate, other research studies of the same group are still under review and their results are so far not accepted by the scientific community.

Conclusion:

The manuscript provides several interesting and partly new indications for the sediment and crustal setting at the South Mozambique continental margin, which are worth to be discussed. However, the manuscript is lacking an open discussion of alternative interpretations and approaches. In my opinion, the study would highly profit from skipping any statements and discussion about the geodynamic evolution and break-up model (or only a short outlook at the end of the manuscript). The authors should focus on a thoroughly discussion of the crustal structure by including alternative approaches. Such a study will easily fill the entire manuscript and will be highly appreciated by the scientific community. Afterwards the obtained results can be incorporated in a break-up model. Finally, I recommend to integrate the investigation of profiles MZ4 and MZ5 to this manuscript, instead of publishing each profile separately. The current submission/publication strategy makes it impossible for reviewers and the readership to judge about the scientific results and to obtain a clear understanding on the research results, due to the limited access to other journals and manuscripts which are still under review.

I recommend to reject the manuscript, as the required changes to the manuscript structure, phase identification/picking of basement signals and unbiased interpretation and discussion are too extensive as to be accounted for in a major revision of normal timeframe. Furthermore, the rejection allows the incorporation of additional refraction profiles to this manuscript.

Kind regards,

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Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-209/se-2020-209-CC1-supplement.pdf