tektonika

Review Report

Wedmore et al., The Drivers of Lower Crustal Earthquakes Along Magma-Poor Portions of the East African Rift, TEKTONIKA, 2025.

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1st Round of Revisions

Decision Letter

(3 Oct. 2024)

Dear authors,

We have now received two reviews on your manuscript submitted to Tektonika. You'll find these reviews appended to this letter, as well as the evaluation by the associate editor.

The reviewer 1 has made a very detailed, thorough and rigorous assessment of your manuscript. He concludes that the assumptions and premises on which your interpretation is based do not adequately account for much existing data. Without going into detail and being less critical all the same, the reviewer 2 identifies the same problems. In addition, the organization of the manuscript is not rigorous, mixing description of existing data, results, interpretation and discussion, and the methods are not presented anywhere except briefly and mixed with the results.

Based on these reviews and our own reading and evaluation, we decided to decline your submission at this stage while encouraging you to rework it and consider a resubmission if you think you can address all reviewer's comments. Tektonika will be pleased to consider the resubmission of a seriously revised manuscript. If you decide to resubmit, please include a rebuttal letter answering all reviewer's comments and explaining how you have addressed them.

Thanks for submitting to Tektonika,
Best Regards,

Robin Lacassin, Tektonika Executive Editor
Guillaume Duclaux, Tektonika Associate Editor

Associate editor evaluation:

Dear Dr Wedmore,

Thank you for submitting your manuscript titled "The drivers of lower crustal earthquakes along magma-poor portions of the East African Rift" to Tektonika. We appreciate the opportunity to consider your work for publication in our journal.

Editorial Decision:
After careful consideration of the editorial team and thorough review by two expert referees, we regret to inform you that we cannot accept your manuscript for publication in its current form. While the topic of your research is of primary interest to our journal's readership, the reviewers have identified several major flaws that preclude publication at this time.

Review Summary:
Both reviewers acknowledged the potential significance of your research topic. However, they also raised substantial concerns about your study. Reviewer 1 has provided a very detailed review of your work and we encourage you to consider every single point raised there.

Recommendation:
Given the extent of the concerns raised, we believe that addressing these issues would require substantial additional work and a major revision of the manuscript. Therefore, we have decided to decline your manuscript at this stage. We encourage you to thoroughly address the reviewers' comments before considering a resubmission.

We appreciate your interest in Tektonika and hope that you will consider us for future submissions of your work.

Sincerely,
Guillaume Duclaux, Associate Editor

Comments by Reviewer 1

(Folarin Kolawole)

I thank the editor for inviting me to review this manuscript. The manuscript by Wedmore et al. investigated possible mechanisms for deep (>20-km) earthquakes in the magma-poor Western branch of the East African Rift System (EARS) by presenting sparse regional earthquake data, and integrating 1-D geodynamical modeling, and soil CO2 gas flux in Southern Malawi. The authors suggest that by numerically testing for a thick seismogenic dry crust with many assumptions to accommodate brittle deformation in the lower crust and upper mantle, and by providing soil CO2 with low fluxes in a portion of the region, they can conclude an absence of deep fluids in driving the deep earthquakes in the region.

I have two serious concerns about this work, first being the interpretations of datasets presented to make this argument which are unable to support the strong conclusions being made, and second, the low quality of the organization and writing of the manuscript. In the light of overwhelming observational evidence of various published studies from both seismology and hot spring geochemistry along the EARS's western and southwestern rift zones that have made clear arguments for the roles of fluid (volatiles) -driven lower crustal and upper mantle earthquakes, I cannot in good faith support the publication of this work. The assertion of 'strain localization in space & time' as opposed to the role of fluids in inducing the strain localization is also weird, considering that the strain is already localized on a shear zone in the lower crust for an earthquake event to even nucleate on it. Earthquakes represent a mode of strain release, and only manifests strain localization at the scale of a slip surface, not necessarily at the scale of a fault zone. Also, earthquake nucleation in the lower crust is not just dependent on the ability of the rocks to support brittle deformation, but in the ability of unstable slip to occur during the brittle deformation. The models presented make a case for the ability of the lower crust in the Western rift branch to accommodate embrittlement, but making the leap to triggers of unstable slip (earthquake nucleation) within this depth interval is the real issue here, which is why I emphasize that the data and model provided do not support their conclusions.

I apologize that I cannot encourage a way forward to publication for this work without a serious consideration of the various observational data existing out there, along with a clear lack of explanation of triggers of unstable slip nucleation in a brittle lower crustal domain. I expand on the details of my points below:

The interpretation of datasets presented and the inability of the datasets to support the conclusions in the manuscript:

Seismology dataset (Crustal Vp/Vs)

In line 214, Vp/Vs is described as a proxy for the bulk composition of the crust. This definition of Vp/Vs ratio is incomplete since Vp/Vs ratio is also sensitive to fluids, lithology, and thermal state of the crust, i.e., areas of high geothermal gradient or heat flow would also produce high Vp/Vs ratio in the crust due to thermal softening of the crust (e.g., Hauksson & Unruh, 2007). Similarly, partial melts also create high Vp/ Vs ratios in the lower crust and upper mantle (e.g., Nakajima et al., 2001; Hodgson et al., 2017).

In lines 214-219, the authors describe published data on low and high Vp/Vs values along the rift branch, with high Vp/Vs ratios (>1.8) attributed to the Rungwe volcanic province. This is incorrect because the Tanganyika Rift, which has been shown to have a high Vp/Vs ratio is not in the vicinity of Rungwe (see Hodgson et al., 2017), rather, it is located at >300 km distance from the volcanic province which precludes an association with surface-breaching magmatism. Hodgson et al. (2017) concluded that the observed high Vp-Vs ratio in the crust is likely due to lower crustal melts.

Following Hodgson et al, another study (Lavayssière et al., 2019) show the presence of upper mantle and lower crustal earthquakes beneath this same rift zone and provide explanations that indicate that they are caused by mantle-sourced fluids.

Even recently, two studies from the same region confirm these interpretations from observations of strong lower-crustal seismic anisotropy and earthquake swarms (Ajala et al., 2024; Kolawole & Ajala, 2024).

Also, far south, the popular 2016 Mw6.5 Botswana earthquake, which is a lower-crustal earthquake (24 – 29 km hypocenter) that occurred in cratonic crust, has been shown by many studies to be likely driven by fluids (Gardonio et al., 2018; Fadel et al., 2020; Paulssen et al., 2022). While there is yet to be high-resolution seismology and magnetotelluric campaigns along the other magma-poor basins of the Western Branch, these data-rich locations already provide enough observational data (which are obviously ignored by the authors), to discourage the conclusions made in the current manuscript in review.

I notice that the premise of ‘dry crust’ in the current work relied a lot on modelling results from recent paper by Fagereng et al., which propose a mechanism by which shear zones in dry thick crust of the western branch accommodate strain. However, the paper by Fagereng et al. itself suggested the role of fluid migration along these shear zones which could easily drive their unstable reactivation. The authors should note that petrological modeling of ‘dry lower crust’ primarily refers to absence of water in pore spaces, and decrease in effective normal stress only gets you as far as shear failure is concerned, but says nothing about unstable slip by rheological weakening mechanisms of melts and volatiles.

In addition to these overwhelming seismological data on fluid-driven seismicity in the region, I also refer the authors to the recent works by an Oxford group which are showing the presence of mantle-sourced volatiles degassing in the Kafue Rift, again at another magma-poor rift that is located many hundreds of kilometers southwest of the Rungwe volcanic province (see details in 'Soil CO2 gas flux' below).

Soil CO2 gas flux:

CO2 gas flux is not only always mantle-sourced, in fact, it is well known that microbial processes contribute a significant amount of soil CO2 gas fluxes. In the manuscript, CO2 flux sampling methodology and measurements are briefly introduced in Section 3.2 (lines 342-358), but the authors fail to explain if or how they constrained the source of the CO2 flux. Thus, while the measurement of CO2 is interesting, the CO2 data presented does not contribute anything significant to the discussion on the mechanisms of deep earthquakes in southern Malawi Rift as the source of the CO2 cannot be confirmed. A good example of how this type of analysis should be done is presented in Kämpf et al. (2013) which examined CO2 fluxes in a magma-poor rift setting. In humid, highly vegetated rift basins as the ones in southern Malawi rift, contributions from heterotrophic and autotrophic processes to soil CO2 gas would indeed be significant (Jassal et al., 2005) and the effect need first to be accounted for.

Furthermore, the authors do not include active degassing hot-spring geochemistry in their examination of gas fluxes, specifically Helium isotope studies, which can actually determine the presence of deep mantle-sourced fluids (or micing of mantle and crustal volatiles) by examining the ratio of 3He to 4He (e.g., Barry et al., 2013). In another magma-poor rift of the EARS western branch (Kafue Rift), there has been recent work on mantle Helium degassing by the Oxford group: Karolté et al. (2024) in isotopic indicators of early continental rifting, and Daly et al. (2020) presented at AGU 2020. These studies have an element of CO2 degassing, so the author's claim of no reported evidence of significant CO2 degassing in line 341 is sort of misleading.

Heat flow measurements used in the model constraints:

The authors extract surface heat flow measurements from up to 75 km of the plate boundary (line 284-285). The rifts of the Western branch of the EARS are ~30 – 60 km wide, so sampling up to 75-km from the rift axis) means that some of the heat flow samples are taken at distances ~45-km away from the rift axis. Since the rift axes is where thermal advection from the mantle is highest, such sampling will be within the rift flanks which are much colder. Thus, the heat flow measurements used in the models is thus misleading as several of the heat flow samples would be lower than what would be present in the rift axes, resulting in a colder model of the lower crust which would of course produce brittle material in the models.

Moreover, it is also not clear why the authors ignored published heat flow data in the Malawi Rift constrained from Curie Point depths (Njinju et al., 2019) which is higher resolution than sparse East Africa bore hole samples in the Global Heat Flow Database. The limitations of the few measurements in the database extend beyond sediment compaction (lines 286-288), as there is also the issue of local radiogenic heat production from sedimentary shale layers.

The low quality of organization of the text:

The manuscript is hard to follow due to low quality of organization of the text. There are no clear and defined sections for methods, results and discussion, instead they are all lumped into Section 3. The combination of the sections, coupled with vague sentences in the writing, makes distinguishing between data and interpretations hard for the reader, especially where both are lumped together in the same paragraph (e.g., lines 341-358, where 342-349 should be method, 349-351 in results, and

351-358 in discussion). The manuscript feels rushed, and there is a lack of attention to detail, such as the Figure 8 introduced but never mentioned in the manuscript, and overall, the main goal of the manuscript appears to be to publish the new soil CO_2 gas flux dataset instead of adding to the body of work examining deep earthquakes in magma-poor regions.

Lack of citations of data source:

There are multiple times thought out the manuscript where the sources of datasets were not cited, such as no citation Moho depth measurements and 31 published estimates of Vp/Vs values (line 201 and 217 respectively), and no explanation provided as to reasoning for a friction coefficient value of 0.6, and a hydrostatic fluid pressure value of 0.4 (line 299).

Figures:

The figures also reflect a lack of attention to detail, and the organizational flow of the figures needs to be reworked, i.e., Figure 6 is introduced in line 176, shortly after Figure 2 is introduced in line 160 and before Figure 3, or, Figure 8 is placed before Figure 7 and not mentioned once thought-out the manuscript.

Some brief comments about each figure:

Figure 1:

Panels A and B need a scale.

Panel C is not helpful or insightful, it would be more useful if it was color-coded by region/tectonic boundary so we could see the spatial pattern along the temporal one.

Include name of tectonic plates and rifts

Fault (blue lines) should also be in panel

B.

A-A', etc… in panel B are hard to see. Should be the same typography as panel A.

In the figure caption, indicate that that the red lines are the plate tectonic boundaries, indicate what the black-dashed lines are.

Inset with map of Africa which the Panels A and B zomm into.

Figure 2:

Panel needs A, B, C labels.

Plot Vp/Vs spatially

Rift names along the profile length would be helpful to situate earthquakes spatially.

Figure 3:

Letters for profiles A,B,C need to be bigger in panel A, similar to Figure 1 panel A.

Rift names along the profile length would be helpful to situate earthquakes spatially.

Figure 6:

Instead of color-coding by latitude, plot them in map view.

Explain why >24 km distance from rift axes was chosen in the caption.

Figure 8:

Instead of changing the color of the circle outline, change the shape but keep the same color to indicate aftershocks of Mag> 5.8

Explain why >24 km distance from rift axes was chosen in the caption.

References:

Ajala, R., Kolawole, F. and Menke, W., 2024. Blind magmatism abets nonvolcanic continental rifting. Communications Earth & Environment, 5(1), p.80.

Daly, M. C., et al., (2020). Helium isotope Evidence for the Mantle Connection of the Southwestern Rift; a Developing Plate Bounary in Africa, AGU 2020 Abstract #T028-05Fadel, I., Paulssen, H., van der Meijde, M., Kwadiba, M., Ntibinyane, O., Nyblade, A. and Durrheim, R., 2020. Crustal and upper mantle shear wave velocity structure of Botswana: The 3 April 2017 central Botswana earthquake linked to the East African Rift System. Geophysical Research Letters, 47(4), p.e2019GL085598.

Gardonio, B., Jolivet, R., Calais, E. and Leclère, H., 2018. The April 2017 Mw6. 5

Botswana earthquake: an intraplate event triggered by deep fluids. Geophysical Research Letters, 45(17), pp.8886-8896.

Hauksson, E. and Unruh, J. (2007). Regional tectonics of the Coso geothermal area along the intracontinental plate boundary in central eastern California: three-dimensional Vp and Vp-Vs models, spatiotemporal seismicity patterns, and seismogenic deformation, J. Geophys. Res., 112, 1–24.

Hodgson, I., Illsley-Kemp, F., Gallacher, R. J., Keir, D., Ebinger, C. J., & Mtelela, K. (2017). Crustal Structure at a Young Continental Rift: A Receiver Function Study From the Tanganyika Rift. Tectonics, 36, 2806–2822. https://doi.org/10.1002/2017TC004477

Jassal, R., Black, A., Novak, M., Morgenstern, K., Nesic, Z. and Gaumont-Guay, D., 2005. Relationship between soil CO2 concentrations and forest-floor CO2 effluxes. Agricultural and Forest Meteorology, 130(3-4), pp.176-192.

Kämpf, H., Bräuer, K., Schumann, J., Hahne, K. and Strauch, G., 2013. CO2 discharge in an active, non-volcanic continental rift area (Czech Republic): characterisation (δ13C, 3He/4He) and quantification of diffuse and vent CO2 emissions. Chemical Geology, 339, pp.71-83.

Karolté, R., et al., (2024) Isotopic indicators of early continental rifting, presented in the Rift and Rifted Margins Seminar on Feb., 5, 2024, https://www.youtube.com/watch?v=sPyWWqJK9jk

Kolawole, F. and Ajala, R., 2024. Propagating rifts: the roles of crustal damage and ascending mantle fluids. Solid Earth, 15(7), pp.747-762.

Lavayssière, A., Drooff, C., Ebinger, C., Gallacher, R., Illsley-Kemp, F., Oliva, S.J. and Keir, D., 2019. Depth extent and kinematics of faulting in the southern Tanganyika rift, Africa. Tectonics, 38(3), pp.842-862.

Nakajima, J., Matsuzawa, T., Hasegawa, A. and Zhao, D., 2001. Three-dimensional structure of Vp, Vs, and Vp/Vs beneath northeastern Japan: Implications for arc magmatism and fluids. Journal of Geophysical Research: Solid Earth, 106(B10), pp. 21843-21857.

Njinju, E. A., Kolawole, F., Atekwana, E. A., Stamps, D. S., Atekwana, E. A., Abdelsalam, M. G., & Mickus, K. L. (2019). Terrestrial heat flow in the Malawi Rifted Zone, East Africa: Implications for tectono-thermal inheritance in continental rift basins. Journal of Volcanology and Geothermal Research, 387, 106656. https://doi.org/10.1016/j.jvolgeores.2019.07.023

Paulssen, H., Micallef, T., Bouwman, D.R., Ruigrok, E., Herman, M.W., Fadel, I., van der Meijde, M., Kwadiba, M., Maritinkole, J. and Ntibinyane, O., 2022. Rifting of the Kalahari Craton through Botswana? New seismic evidence. Journal of Geophysical Research: Solid Earth, 127(4), p.e2021JB023524.

Comments by Reviewer 2

(Ameha Muluneh)

Review report on the manuscript entitled “The drivers of lower crustal earthquakes along magma-poor portions of the East African Rift” by Wedmore and colleagues

Deep crustal seismicity occurs at depths where the temperature is too high for brittle deformation to occur, challenging our current understanding of the mechanism of earthquakes. Wedmore and colleagues use a high-resolution seismic catalogue and simple thermo-rheological modelling to understand the drivers of these deep crustal earthquakes in the East African Rift System (EARS). The authors show that deep crustal earthquakes in the EARS are mainly driven by high strain rates following large magnitude earthquakes. The manuscript is well written. I recommend publication after minor revisions. Below I list comments that need to be addressed before the manuscript is accepted for publication.

I am not sure that some of the areas you have studied are as magma-poor as you suggest. For example, Wedmore et al., 2024 EPSL showed the presence of melt beneath the Edward-George rift, west of the Tanzanian craton; Ajala et al., 2024 also showed that the presence of deep melt in the Rukwa rift supports rifting. These evidences suggest that there is a likely contribution of fluids exsolved from magma to drive lower crustal seismicity in the whole or part of the study area. High pore fluid pressure combined with high strain rate could explain lower crustal seismicity (e.g., Muluneh et al., 2021).

Line 347-348 & 528-529 - It should be noted that the absence of significant CO_2 flux does not necessarily mean that the pore fluid pressure is low or that there is no melt. Low flux can occur in areas of low permeability, so you may not be able to detect it on the surface.

Could you check the effect of the maximum and minimum heat flows on the temperature profile and yield strength envelope, instead of taking the mean heat flow value (line 293)?

You suggest that the driver of lower crustal seismicity does not vary over a small spatial scale, yet you argue that localized high strain rates could be responsible. Could you elaborate on this concept?

Figures

The figures are quite nice.

Please add a location map to Figure 1. Also add a horizontal scale bar to 1A and B.

I hope the above comments will help the authors to clarify some of the issues with the already well-written manuscript.

References:

Ajala, R., Kolawole, F., W. Menke, 2024. Blind magmatism abets nonvolcanic continental rifting. Communication earth and environments.

Muluneh, A., Keir, D., Corti, G. 2021. Thermo-Rheological Properties of the Ethiopian Lithosphere and Evidence for Transient Fluid Induced Lower Crustal Seismicity Beneath the Ethiopian Rift. Frontiers in Earth Sceicnes, doi: 10.3389/feart.
2021.610165

Wedmore, L., Dan Evans, Jack N. Williams, Juliet Biggs, Åke Fagereng, Peter Mawejj,, Fred Tugume, Thomas Blenkinsop, Daniel E.J. Hobley, 2024. The early onset of magmatic rift faulting in the Edward-George Rift, Uganda. Earth Planet. Sci. Lett. 638 (2024) 118762

2nd Round of Revisions

(resubmitted manuscript)

Authors' Reply to reviews

(13 Jan. 2025)

The authors have resubmitted a new revised manuscript on January 13, 2025. They accompanied this submission with a comprehensive response to the reviews made on the previous manuscript. This response is appended in the following pages.

Note to the editor: we thank you for soliciting two reviews for our initial submission to Tektonika investigating lower crustal seismicity in the southern East Africa System (EARS). We acknowledge that there were deficiencies in this submission, and as outlined below, we have made significant changes to address them with this revised submission.

The reviews that were provided to us are shown below in black text, and our replies on how we addressed them are indicated in blue italicized text. When quoting directly from the manuscript, text is shown in red. We have added numbers to the reviewer's comments to facilitate cross-referencing within this document. Line numbers refer to the tracked changes version of the manuscript.

Reviewer 1:

I thank the editor for inviting me to review this manuscript. The manuscript by Wedmore et al. investigated possible mechanisms for deep (>20-km) earthquakes in the magma- poor Western branch of the East African Rift System (EARS) by presenting sparse regional earthquake data, and integrating 1-D geodynamical modeling, and soil CO2 gas flux in Southern Malawi. The authors suggest that by numerically testing for a thick seismogenic dry crust with many assumptions to accommodate brittle deformation in the lower crust and upper mantle, and by providing soil CO2 with low fluxes in a portion of the region, they can conclude an absence of deep fluids in driving the deep earthquakes in the region.

I have two serious concerns about this work, first being the interpretations of datasets presented to make this argument which are unable to support the strong conclusions being made, and second, the low quality of the organization and writing of the manuscript. In the light of overwhelming observational evidence of various published studies from both seismology and hot spring geochemistry along the EARS's western and southwestern rift zones that have made clear arguments for the roles of fluid (volatiles) -driven lower crustal and upper mantle earthquakes, I cannot in good faith support the publication of this work. The assertion of 'strain localization in space & time' as opposed to the role of fluids in inducing the strain localization is also weird, considering that the strain is already localized on a shear zone in the lower crust for an earthquake event to even nucleate on it. Earthquakes represent a mode of strain release, and only manifests strain localization at the scale of a slip surface, not necessarily at the scale of a fault zone.

As ever with the term 'localization,' it always comes down to the spatial and temporal scale that you're referring to. Hence, we agree that it may seem obvious that strain during an earthquake is localised within faults and finite strain over time is localised in shear zones. However, that has to be placed into the context of the underlying assumption in our 'reference' 1-D lithospheric strength profile (Figure 5), and in other similar studies of crustal strength in the EARS (Nyblade and Langston 1995; Albaric et al 2009; Fagereng 2013), in which time-intergrated strain is equally partitioned across the >50 km wide rift. Our hypothesis is instructive in that when we do explicitly consider across-rift strain localisation within narrow faults and shear zones, it can account for the deep seismicity in the southern EARS (Figure 6). This is also consistent with the hypothesis that rifting initiates in pre-existing heterogenous dry viscous shear zones, which then loaded the surrounding crust to frictional failure in regions where this deformation mechanism is favourable (Fagereng et al., 2024). We have clarified this at Lines 739-742.

We next investigate if frictional failure in the lower crust is driven by deformation localising within smaller-scale structures within the rift, and which in turn, implies that strain rates around these structures are higher than if strain was equally partitioned across the southern EARS (Birhanu et al., 2016; Wedmore et *al., 2021).*

- *Albaric, J., Déverchère, J., Petit, C., Perrot, J., & Le Gall, B. (2009). Crustal rheology and depth distribution of earthquakes: Insights from the central and southern East African Rift System. Tectonophysics, 468(1-4), 28-41.*
- *Fagereng, Å., Diener, J. F. A., Tulley, C. J., & Manda, B. (2024). Metamorphic inheritance, lower- crustal earthquakes, and continental rifting. Geochemistry, Geophysics, Geosystems, 25(3), e2023GC011305.*
- *Nyblade, A. A., & Langston, C. A. (1995). East African earthquakes below 20 km depth and their implications for crustal structure. Geophysical Journal International, 121(1), 49-62.*

Also, earthquake nucleation in the lower crust is not just dependent on the ability of the rocks to support brittle deformation, but in the ability of unstable slip to occur during the brittle deformation. The models presented make a case for the ability of the lower crust in the Western rift branch to accommodate embrittlement, but making the leap to triggers of unstable slip (earthquake nucleation) within this depth interval is the real issue here, which is why I emphasize that the data and model provided do not support their conclusions.

We thank the reviewer for highlighting this point. Of note here is that deformation experiments on representative rocks from the southern EARS (Hellebrekers et al 2019) indicate that unstable velocity weakening behaviour in felsic lithologies at lower crustal pressure and temperatures may only be possible at high strain rates. Hence, further reinforcing our hypothesis that strain localisation can explain the embrittlement of the lower crust in southern Africa, and that this brittle deformation is accommodated via unstable seismic slip. This now noted at Lines 1104-1107.

We also note that high strain rates may actually be a prerequisite for earthquakes to nucleate within a predominantly felsic lower crust, as the rocks tend to be velocity- strengthening but become more unstable with higher strain rate (Hellebrekers et al., 2019).

- *Hellebrekers, N., Niemeijer, A. R., Fagereng, Å., Manda, B., & Mvula, R. L. (2019). Lower crustal earthquakes in the East African Rift System: Insights from frictional properties of rock samples from the Malawi rift. Tectonophysics, 767, 228167.*

I apologize that I cannot encourage a way forward to publication for this work without a serious consideration of the various observational data existing out there, along with a clear lack of explanation of triggers of unstable slip nucleation in a brittle lower crustal domain. I expand on the details of my points below:

The interpretation of datasets presented and the inability of the datasets to support the conclusions in the manuscript:

As discussed further below we have made major changes to the manuscript to address the reviewer's concerns, most obviously by removing the CO2 analyses. We have also conducted further analysis to evaluate the sensitivity of the 1-D strength profiles to the geothermal gradient and to viscous creep flow laws for a hydrous lower crust (Figure 6), and addressed their comments about the manuscript's structure. Ultimately, this leads us to propose that although locally important, we do not consider fluids to be the principal mechanism for triggering lower crustal seismicity within the southern EARS. We recognise that the reviewer might not agree with out interpretation. Nevertheless, we would hope that in the spirit of healthy scientific discourse, that would not preclude this study from being added to this debate.

1) *Seismology dataset (Crustal Vp/Vs)*

1a) In line 214, Vp/Vs is described as a proxy for the bulk composition of the crust. This definition of Vp/Vs ratio is incomplete since Vp/Vs ratio is also sensitive to fluids, lithology, and thermal state of the crust, i.e., areas of high geothermal gradient or heat flow would also produce high Vp/Vs ratio in the crust due to thermal softening of the crust (e.g., Hauksson & Unruh, 2007). Similarly, partial melts also create high Vp/Vs ratios in the lower crust and upper mantle (e.g., Nakajima et al., 2001; Hodgson et al., 2017).

We agree and apologise that we did not clarify in our description of Vp/Vs. We now specify that 'Vp/Vs is a proxy for the bulk <u>rheology</u> of the crust' in the revised submission (Lines 469-470) to highlight this point

1b) In lines 214-219, the authors describe published data on low and high Vp/Vs values along the rift branch, with high Vp/Vs ratios (>1.8) attributed to the Rungwe volcanic province. This is incorrect because the Tanganyika Rift, which has been shown to have a high Vp/Vs ratio is not in the vicinity of Rungwe (see Hodgson et al., 2017), rather, it is located at >300 km distance from the volcanic province which precludes an association with surface-breaching magmatism. Hodgson et al. (2017) concluded that the observed high Vp-Vs ratio in the crust

is likely due to lower crustal melts. Following Hodgson et al, another study (Lavayssière et al., 2019) show the presence of upper mantle and lower crustal earthquakes beneath this same rift zone and provide explanations that indicate that they are caused by mantle-sourced fluids. Even recently, two studies from the
same region confirm these interpretations from observations of strong lower-crustal seismic anisotropy and earthquake swarms (Ajala et al., 2024; Kolawole & Ajala, 2024).

In the revised manuscript, we have corrected this point to indicate that the high Vp/Vs ratios indicated by Hodgson et al 2017 are associated with the southern Tanganyika Rift and not the Rungwe Volcanic Province (Lines 473476). Though note high Vp/Vs ratios below the Rungwe Volcanic Province are documented by Borrego et al (2018).

- *Borrego, D., Nyblade, A. A., Accardo, N. J., Gaherty, J. B., Ebinger, C. J., Shillington, D. J., ... & Tepp,*
  G. (2018). Crustal structure surrounding the northern Malawi rift and beneath the Rungwe Volcanic Province, East Africa. Geophysical Journal International, 215(2), 1410-1426.

1c) Also, far south, the popular 2016 Mw6.5 Botswana earthquake, which is a lower- crustal earthquake (24 – 29 km hypocenter) that occurred in cratonic crust, has been shown by many studies to be likely driven by fluids (Gardonio et al., 2018; Fadel et al., 2020; Paulssen et al., 2022). While there is yet to be high-resolution seismology and
magnetotelluric campaigns along the other magma-poor basins of the Western Branch, these data-rich locations already provide enough observational data (which are obviously ignored by the authors), to discourage the conclusions made in the current manuscript in review.

In the revised manuscript, we now highlight the examples provided by the reviewer where lower crustal earthquakes are inferred to be associated with fluids (Lines 1069- 1071). We acknowledge too that there is a need to collect more high-resolution
geophysics data in southern Africa. Nevertheless, we contend that there is sufficient evidence for us to propose examples of lower crustal earthquakes in the EARS which are not fluid driven. This is highlighted by the 1989 Mw 6.1 Salima Earthquake in southern
Malawi where geophysical (Sun et al 2021) and geochemical (Davos-Elizondo et al 2021; Fagereng et al 2024) data do not support the presence of suprahydrostatic fluid pressure in the lower crust. We highlight this at Lines 1073-1078:

There are, however, also regions where fluid-driven mechanisms for lower crustal seismicity are less obvious. For example, the 32 km deep M_W 6.1 Salima Earthquake in southern Malawi nucleated in a region where: (1) metamorphic reactions have dehydrated the lower crust (Fagereng et al., 2024; Manda et al., 2019), (2) possible incursion of meteoric fluids from above can be excluded as hot springs in this region indicate that they percolate to depths <5 km (Dávalos-Elizondo et al., 2021), and (3)
there is no evidence from seismic tomography for upper mantle upwelling or partial melting (Sun et al., 2021).

- *Davalos-Elizondo, E., Atekwana, E. A., Atekwana, E. A., Tsokonombwe, G., & Laó-Dávila, D. A. (2021). Medium to low enthalpy geothermal reservoirs estimated from geothermometry and mixing models of hot springs along the Malawi Rift Zone. Geothermics, 89, 101963.*
- *Fagereng, Å., Diener, J. F. A., Tulley, C. J., & Manda, B. (2024). Metamorphic inheritance, lower- crustal earthquakes, and continental rifting. Geochemistry, Geophysics, Geosystems, 25(3), e2023GC011305.*
- *Sun, M., Gao, S. S., Liu, K. H., Mickus, K., Fu, X., & Yu, Y. (2021). Receiver function investigation of crustal structure in the Malawi and Luangwa rift zones and adjacent areas. Gondwana Research, 89, 168-176.*

1d) I notice that the premise of 'dry crust' in the current work relied a lot on modelling results from recent paper by Fagereng et al., which propose a mechanism by which shear zones in dry thick crust of the western branch accommodate strain. However, the paper by Fagereng et al. itself suggested the role of fluid migration along these shear zones which could easily drive their unstable reactivation. The authors should note that petrological modeling of 'dry lower crust' primarily refers to absence of water in pore spaces, and decrease in effective normal stress only gets you as far as shear failure is concerned, but says nothing about unstable slip by rheological weakening mechanisms of melts and volatiles.

The reviewer is correct to point out the role of fluids in the model by Fagereng et al (2024), however, an important facet of this model is that fluids are only available to drive lower crustal deformation after rifting has started. Hence, there must still be mechanisms that are capable of initiating rifting and lower crustal seismicity in the absence of fluids. We do agree that the rheological implications of adding fluids to the lower crust are not limited to reducing the effective normal stress; however, as noted at Lines 730-733, these mechanisms may ultimately favour viscous creep over brittle deformation:

A hydrous lower crust would also promote retrograde growth of fine-grained phyllosilicate minerals, which at lower crustal conditions are more susceptible to viscous or aseismic creep mechanisms (Craig and Jackson 2001, Fageren et a., 2024, Wintsch et al 1995).A hydrous crust would also have a lower melting point (Fagereng et al., 2024), potentially allowing partial melting and associated weakening of the crust (Rosenberg and Handy, 2005).

1e) In addition to these overwhelming seismological data on fluid-driven seismicity in the region, I also refer the authors to the recent works by an Oxford group which are showing the presence of mantle-sourced volatiles degassing in the Kafue Rift, again at another magma-poor rift that is located many hundreds of kilometers southwest of the Rungwe volcanic province (see details in 'Soil CO2 gas flux' below).

We thank the reviewer for alerting us to this example from the Kafue Rift, which we are happy to include once it is published. More broadly, we now highlight here that when volatiles have been associated with lower crustal earthquakes, these typically manifest as low magnitude (M<4) earthquake swarms (e.g., Lindenfeld et al 2012, Kolawole and Ajala 2024, see also Lines 1069-1071).

The migration of crustal and mantle fluids can also trigger lower crustal seismicity in southern branches of the EARS, typically in the form of M <4 earthquake swarms (Kolawole and Ajala, 2024; Lavayssière et al., 2019; Lindenfeld et al., 2012).

Hence, it is difficult to causally link the release of volatiles with moderate-large magnitude (i.e., Mw>4) lower crustal seismicity in the southern EARS, particularly given that our 1-D lithospheric strength profiles highlight that an increase in pore fluid pressure is not an effective mechanism to embrittle the lower crust (Figure 6). We acknowledge that the role of fluids in the 2017 Botswana Earthquake, as proposed by Paulssen et al (2022), may be an exception to this. However, that interpretation is not unequivocal. Indeed, given that this earthquake may have reactivated a pre-existing structure (Kolawole et al 2017), its 20-25 km focal depth could also reflect locally elevated strain rates.

- *Kolawole, F., & Ajala, R. (2024). Propagating rifts: the roles of crustal damage and ascending mantle fluids. Solid Earth, 15(7), 747-762.*
- *Kolawole, F., Atekwana, E. A., Malloy, S., Stamps, D. S., Grandin, R., Abdelsalam, M. G., ... & Shemang, E. M. (2017). Aeromagnetic, gravity, and Differential Interferometric Synthetic Aperture Radar analyses reveal the causative fault of the 3 April 2017 Mw 6.5 Moiyabana, Botswana,*

earthquake. Geophysical Research Letters, 44(17), 8837-8846.

- *Lindenfeld, M., Rümpker, G., Link, K., Koehn, D., & Batte, A. (2012). Fluid-triggered earthquake swarms in the Rwenzori region, East African Rift—Evidence for rift initiation. Tectonophysics, 566, 95-104.*
- *Paulssen, H., Micallef, T., Bouwman, D. R., Ruigrok, E., Herman, M. W., Fadel, I., ... & Ntibinyane,*
  O. (2022). Rifting of the Kalahari Craton through Botswana? New seismic evidence. Journal of Geophysical Research: Solid Earth, 127(4), e2021JB023524.

2.) Soil CO2 gas flux:

CO2 gas flux is not only always mantle-sourced, in fact, it is well known that microbial processes contribute a significant amount of soil CO2 gas fluxes. In the manuscript, CO2 flux sampling methodology and measurements are briefly introduced in Section
3.2 (lines 342-358), but the authors fail to explain if or how they constrained the source of the CO2 flux. Thus, while the measurement of CO2 is interesting, the CO2 data presented does not contribute anything significant to the discussion on the
mechanisms of deep earthquakes in southern Malawi Rift as the source of the CO2 cannot be confirmed. A good example of how this type of analysis should be done is presented in Kämpf et al. (2013) which examined CO2 fluxes in a magma-poor rift setting. In humid, highly vegetated rift basins as the ones in southern Malawi rift, contributions from heterotrophic and autotrophic processes to soil CO2 gas would indeed be significant (Jassal et al., 2005) and the effect need first to be accounted for.

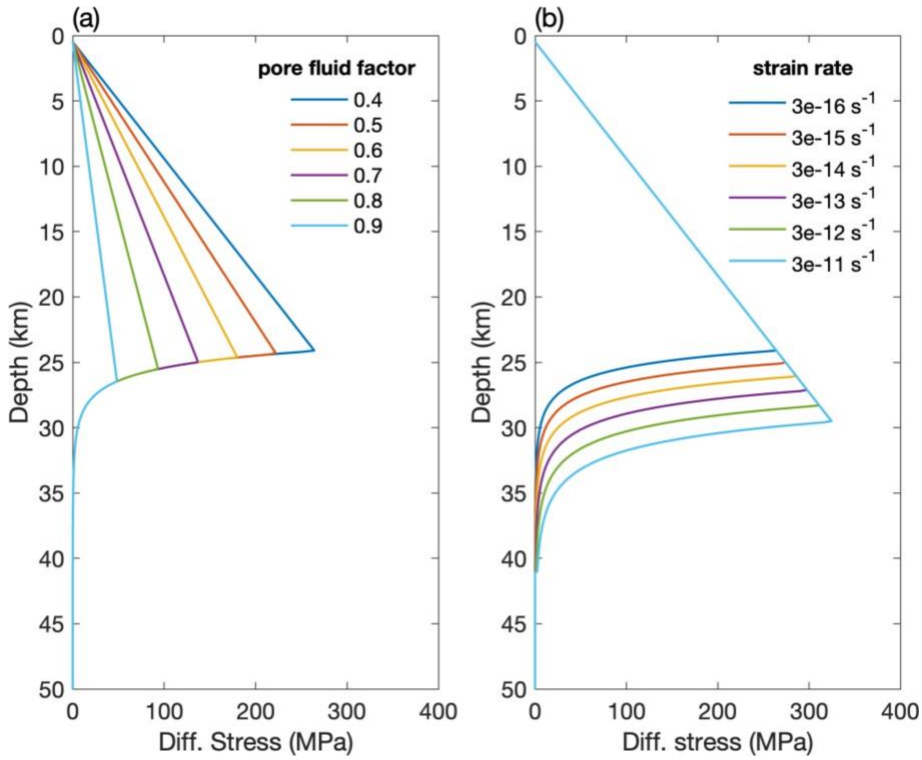
Furthermore, the authors do not include active degassing hot-spring geochemistry in their examination of gas fluxes, specifically Helium isotope studies, which can actually determine the presence of deep mantle-sourced fluids (or micing of mantle and crustal volatiles) by examining the ratio of 3He to 4He (e.g., Barry et al., 2013). In another magma-poor rift of the EARS western branch (Kafue Rift), there has been recent work on mantle Helium degassing by the Oxford group: Karolté et al. (2024) in isotopic indicators of early continental rifting, and Daly et al. (2020) presented at AGU 2020. These studies have an element of CO2 degassing, so the author's claim of no reported evidence of significant CO2 degassing in line 341 is sort of misleading.

We thank the reviewer for their comments and suggestions on our CO2 measurements. Given that we cannot entirely address their concerns on separating the contributions of different CO2 sources in our transects with the *existing data, we believe the most appropriate cause of action is to remove this analysis from the resubmitted manuscript. In doing so, we note that the main findings of this study were not contingent on the CO2 measurements, and so they are not affected by removing this content.*

3.) Heat flow measurements used in the model constraints:

3a) The authors extract surface heat flow measurements from up to 75 km of the plate boundary (line 284-285). The rifts of the Western branch of the EARS are ~30 – 60 km wide, so sampling up to 75-km from the rift axis) means that some of the heat flow samples are taken at distances ~45-km away from the rift axis. Since the rift axes is where thermal advection from the mantle is highest, such sampling will be within the rift flanks which are much colder. Thus, the heat flow measurements used in the models is thus misleading as several of the heat flow samples would be lower than what would be present in the rift axes, resulting in a colder model of the lower crust which would of course produce brittle material in the models.

To explore this uncertainty, in Section 4.5 we now describe strength profiles developed using a range of geothermal gradient in southern Africa. In the case of a relatively low geothermal gradient –realised here by implementing a surface heat-flow 50 mWm^{-2} (vs. 68 mWm^{-2} in our reference profile)- lower crustal seismicity could extend throughout the crust, and even into the lithospheric mantle (Figure 6f). However, of course the point raised by the reviewer is that the geothermal gradient is implemented using an underestimated surface heat flow. In this respect, below we show the influence on the lithospheric strength profiles of using a geothermal gradient with a heat flow estimate of 80 mWm^{-2}.



As expected, applying this higher surface heat flow estimate reduces the depth to the frictional viscous transition relative to profiles using a surface heat flow of 68 mWm^{-2} (compare a&b above to Figures 6a and c in the revised manuscript). With regards to our study's hypothesis that 35-40 km deep seismicity in southern Africa is caused by locally elevated strain rates, these profiles indicate that even the most elevated strain rate estimate can only account for seismicity down to 30 km. However, this challenge with explaining lower crustal seismicity in southern Africa would also apply to other mechanisms discussed in this study (e.g., fluids, mafic lower crust).

We note too that surface heat flow maps in Malawi indicate that in this region, the crust has not been sufficiently extended to allow mantle upwelling to influence the geothermal gradient, although it may be influenced by Karoo rifting (Njinju et al 2019, see also next comment). In other words, surface heat flow in southern Africa is not necessarily lower on the rift shoulders than the rift axes.

3b) Moreover, it is also not clear why the authors ignored published heat flow data in the Malawi Rift constrained from Curie Point depths (Njinju et al., 2019) which is higher resolution than sparse East Africa bore hole samples in the

Global Heat Flow Database. The limitations of the few measurements in the database extend beyond sediment compaction (lines 286-288), as there is also the issue of local radiogenic heat production from sedimentary shale layers.

We thank the reviewer for highlighting the surface heatflow measurements obtained from Curie point depths in Njinju et al (2019). Interestingly, this study indicates surface heat flow measurements in Malawi Rift (60-75 mW/m^2, Figure 7 in Njinju et al 2019) that are compatible with the measurements from the Global Heat Flow Database that were considered when previously developing our 1D lithospheric strength profiles (average heat flow measurement 68 mW/m^2). We therefore do not see the need to revise our estimate for mean surface heatflow in the southern EARS this revised submission, but we do refer to the estimates from Njinju et al (2019) (Lines 637-638).

4.) The low quality of organization of the text:

The manuscript is hard to follow due to low quality of organization of the text. There are no clear and defined sections for methods, results and discussion, instead they are all lumped into Section 3. The combination of the sections, coupled with vague sentences in the writing, makes distinguishing between data and interpretations hard for the reader, especially where both are lumped together in the same paragraph (e.g., lines 341-358, where 342-349 should be method, 349-351 in results, and 351-358 in discussion). The manuscript feels rushed, and there is a lack of attention to detail, such as the Figure 8 introduced but never mentioned in the manuscript, and overall, the main goal of the manuscript appears to be to publish the new soil CO2 gas flux dataset instead of adding to the body of work examining deep earthquakes in magma-poor regions.

We acknowledge there were deficiencies in the structure of the initial submission. In the revised manuscript, we have made major modifications to structure the manuscript in a logical way, and in which we: first analyse the catalogue in Holmgren et al (2023) (Section 3), present our 1D lithospheric strength profiles (Section 4), and now have a distinct Discussion (Section 5), and Conclusions (Section 6). Within Section 4, we have also added a new table (Table 3) to facilitate comparisons between the different lithospheric strength profiles that we developed. Errors with cross referencing our figures have now been corrected.

5.) Lack of citations of data source:

There are multiple times thought out the manuscript where the sources of datasets were not cited, such as no citation Moho depth measurements and 31 published estimates of Vp/Vs values (line 201 and 217 respectively), and no explanation provided as to reasoning for a friction coefficient value of 0.6, and a hydrostatic fluid pressure value of 0.4 (line 299).

A list of the references from which we compiled the Moho depth estimates is now included at Lines 419-421. In addition, we include a table listing the Moho depth and V_P/V_S estimates with the supplementary information. Justification for a friction coefficient of 0.6 and hydrostatic fluid pressure is now added at Lines 566-570 (although ultimately, the lithospheric strength profiles are relatively insensitive to which fluid pressure we select).

Figures:

The figures also reflect a lack of attention to detail, and the organizational flow of the figures needs to be reworked, i.e., Figure 6 is introduced in line 176, shortly after Figure 2 is introduced in line 160 and before Figure 3, or, Figure 8 is placed before Figure 7 and not mentioned once thought-out the manuscript.

Yes, apologies for this. We have checked the figure cross-references, and where necessary made the relevant corrections

Some brief comments about each figure:

Figure 1:

Panels A and B need a scale. → *Added in panel (a)*

Panel C is not helpful or insightful, it would be more useful if it was color-coded by region/tectonic boundary so we could see the spatial pattern along the temporal one.

Our preference to retain Figure 1c as is, but add annotations to this plot to highlight notable earthquake sequences and/or temporary seismic arrays that influenced the
inclusion of events in the H23 relocated catalog. Note, events are already colour coded by depth.

Include name of tectonic plates and rifts. → *Added in panel (a)*

Fault (blue lines) should also be in panel B. → *Faults -now red lines- added to panel (b)*

A-A', etc… in panel B are hard to see. Should be the same typography as panel A. →
Labels (now in panel (a)) made clearer and consistent typography.

In the figure caption, indicate that that the red lines are the plate tectonic boundaries, indicate what the black-dashed lines are.

Added in Figure 1 legend (note white dashed lines indicate rift branches that have been proposed, but for which there is no evidence from geodesy).

Inset with map of Africa which the Panels A and B zoom into. → *Inset map added*

Figure 2:

Panel needs A, B, C labels. → *Labels added*

Plot Vp/Vs spatially → *Now plotted in Figure 1b*

Rift names along the profile length would be helpful to situate earthquakes spatially.
→ *Rift names added to each panel*

Figure 3: *[now Figure 4]*

Letters for profiles A,B,C need to be bigger in panel A, similar to Figure 1 panel A.
Labels made larger in panel (a).

Rift names along the profile length would be helpful to situate earthquakes spatially. *Rift names added (note, this figure is indicting surface heat flow measurements locations, not earthquakes).*

Figure 6:
Instead of color-coding by latitude, plot them in map view.

Explain why >24 km distance from rift axes was chosen in the caption.

We removed this figure (showing the across rift distribution of seismicity) during the manuscript revisions.

Figure 8*: [now Figure 3]*

Instead of changing the color of the circle outline, change the shape but keep the same color to indicate aftershocks of Mag> 5.8

We now show aftershocks with a circle shape, and background events with a diamond

Explain why >24 km distance from rift axes was chosen in the caption. → *Added*

References: [from reviewer]

- Ajala, R., Kolawole, F. and Menke, W., 2024. Blind magmatism abets nonvolcanic continental rifting. Communications Earth & Environment, 5(1), p.80.
- Daly, M. C., et al., (2020). Helium isotope Evidence for the Mantle Connection of the Southwestern Rift; a Developing Plate Bounary in Africa, AGU 2020 Abstract #T028-05
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- Kolawole, F. and Ajala, R., 2024. Propagating rifts: the roles of crustal damage and ascending mantle fluids. Solid Earth, 15(7), pp.747-762.
- Lavayssière, A., Drooff, C., Ebinger, C., Gallacher, R., Illsley-Kemp, F., Oliva, S.J. and Keir, D., 2019. Depth extent and kinematics of faulting in the southern Tanganyika rift, Africa. Tectonics, 38(3), pp.842-862.
- Nakajima, J., Matsuzawa, T., Hasegawa, A. and Zhao, D., 2001. Three-dimensional structure of Vp, Vs, and Vp/Vs beneath northeastern Japan: Implications for arc magmatism and fluids. Journal of Geophysical Research: Solid Earth, 106(B10), pp.21843-21857.
- Njinju, E. A., Kolawole, F., Atekwana, E. A., Stamps, D. S., Atekwana, E. A., Abdelsalam, M. G., & Mickus, K. L. (2019). Terrestrial heat flow in the Malawi Rifted Zone, East Africa: Implications for tectono-thermal inheritance in continental rift basins. Journal of Volcanology and Geothermal Research, 387, 106656. https://doi.org/10.1016/j.jvolgeores.2019.07.023
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Reviewer 2:

Review report on the manuscript entitled "The drivers of lower crustal earthquakes along magma-poor portions of the East African Rift" by Wedmore and colleagues

Deep crustal seismicity occurs at depths where the temperature is too high for brittle deformation to occur, challenging our current understanding of the mechanism of earthquakes. Wedmore and colleagues use a high-resolution seismic catalogue and simple thermo-rheological modelling to understand the drivers of these deep crustalearthquakes in the East African Rift System (EARS). The authors show that deep crustal earthquakes in the EARS are mainly driven by high strain rates following large magnitude earthquakes. The manuscript is well written. I recommend publication after minor revisions. Below I list comments that need to be addressed before the manuscript is accepted for publication.

1.) I am not sure that some of the areas you have studied are as magma-poor as you suggest. For example, Wedmore et al., 2024 EPSL showed the presence of melt beneath the Edward-George rift, west of the Tanzanian craton; Ajala et al., 2024 also showed that the presence of deep melt in the Rukwa rift supports rifting. These evidences suggest that there is a likely contribution of fluids exsolved from magma to drive lower crustal seismicity in the whole or part of the study area. High pore fluid pressure combined with high strain rate could explain lower crustal seismicity (e.g., Muluneh et al., 2021).

Excellent point - in the revised manuscript we now explicitly only consider events in the H23 catalogue south of 4.5°S. This means we only consider events within one of the Western Branch's four volcanic provinces (Ebinger 1989; Lines 233-235). In addition, we highlight that the seismogenic crust may be locally thinned under these volcanic provinces (Lines 394-402):

*One exception to the ubiquitous lower crustal earthquakes along the southern EARS
may be present at the Rungwe Volcanic Province (RVP). Here there are few earthquakes with focal depths >25 km (Figure 2a), and although there are insufficient events in the H23 relocated catalogue to derive a RVP-specific d95 estimate, a thinner seismogenic crust is consistent with previous studies for this region (Ebinger et al., 2019). A locally reduced seismogenic crust thickness is also observed beneath the Western Branch's other volcanic provinces; 15-20 km at the South Kivu and Virunga volcanic provinces
(Delvaux et al., 2017; Wood et al., 2017) and 15-25 km beneath the Toro-Ankole volcanic province (Lindenfeld et al., 2012a; Wedmore et al., 2024).*

We add here that compared to the Eastern Branch, the spatial extent of the Western Branch's volcanic province are minimal, and so this does not influence our overall finding that lower crustal earthquakes are (near) ubiquitous along the Western Branch.

As indicated in our reply to Comment #1e by Reviewer #1, we agree that fluids associated with deep melts are associated with lower crustal seismicity. However, this seismicity typically manifests as swarms of low magnitude (M <4) events (Lindenfield et al 2012; Ajala and Kolawole 2024). Hence, this mechanism cannot also explain some of the more moderate magnitude *seismicity that occurs in regions with no detectable deep melts.*

As documented in Sibson (2003), it is difficult to maintain near lithostatic pore fluid pressures in normal fault stress states, as they may trigger tensional fracturing that then increases permeability and dissipate the pressurized fluids (Lines 1083-1087).

We highlight too that these 1-D strength profiles do not consider the possibility that near-lithostatic fluid pressures are difficult to maintain in a normal fault stress state (Sibson, 2000; Sibson and Rowland, 2003), or alternatively, that elevated pore fluid pressures are 'ineffective' at reducing effective stresses in the lower crust (Hirth and Beeler, 2015).

Hence, regardless of the question of whether fluids are present in the lower crust, we do not consider that near lithostatic pore fluid pressure are an effective mechanism for the embrittlement of southern Africa's lower crust.

- *Ebinger, C. J. (1989). Tectonic development of the western branch of the East African rift system. Geological Society of America Bulletin, 101(7), 885-903.*
- *Sibson, R. H. (2003). Brittle-failure controls on maximum sustainable overpressure in different tectonic regimes. AAPG bulletin, 87(6), 901-908.*

2.) Line 347-348 & 528-529 - It should be noted that the absence of significant CO2 flux does not necessarily mean that the pore fluid pressure is low or that there is no melt.
Low flux can occur in areas of low permeability, so you may not be able to detect it on the surface.

This has been addressed separately by removing the CO2 flux results in the revised manuscript (see our reply to Comment #2 by Reviewer #1)

3.) Could you check the effect of the maximum and minimum heat flows on the temperature profile and yield strength envelope, instead of taking the mean heat flow value (line 293)?

This point is now addressed through the addition of Section 4.5, which explicitly analyses strength profiles for end-member heat flow cases (see also our reply to Comment #3 by Reviewer #1 who raised a similar issue).

4.) You suggest that the driver of lower crustal seismicity does not vary over a small spatial scale, yet you argue that localized high strain rates could be responsible. Could you elaborate on this concept?

In the revised manuscript, we now highlight that the localisation we are referring to is on structures across the rift, whereas the ubiquitous lower crustal seismicity is an along-rift observation (Lines 739-742):

We next investigate if frictional failure in the lower crust is driven by deformation localising within smaller-scale structures within the rift, and which in turn, implies that strain rates around these structures are higher than if strain was equally partitioned across the southern EARS (Birhanu et al., 2016; Wedmore et al., 2021).

In other words, at any point along the Western Branch, we envisage that lateral heterogenies present that can localise deformation, experience elevated strain rates, and hence drive lower crustal seismicity.

Figures

The figures are quite nice.

Please add a location map to Figure 1. Also add a horizontal scale bar to 1A and B.

Both added

I hope the above comments will help the authors to clarify some of the issues with the already well-written manuscript.

References: [from reviewer]

- Ajala, R., Kolawole, F., W. Menke, 2024. Blind magmatism abets nonvolcanic continental rifting. Communication earth and environments.
- Muluneh, A., Keir, D., Corti, G. 2021. Thermo-Rheological Properties of the Ethiopian
  Lithosphere and Evidence for Transient Fluid Induced Lower Crustal Seismicity Beneath the Ethiopian Rift. Frontiers in Earth Sceicnes, doi: 10.3389/feart.2021.610165
- Wedmore, L., Dan Evans, Jack N. Williams, Juliet Biggs, Åke Fagereng, Peter Mawejj,, Fred Tugume, Thomas Blenkinsop, Daniel E.J. Hobley, 2024. The early onset of magmatic rift faulting in the Edward-George Rift, Uganda. Earth Planet. Sci. Lett. 638 (2024)
  118762

Decision Letter

(10 March 2025)

Dear authors,

We have reached a decision regarding your submission to тектоniка, entitled "The drivers of lower crustal earthquakes along magma-poor portions of the East African Rift". Following the initial submission, your revised manuscript was handled by Associate Editor Guillaume Duclaux and has received two new reviews. We thank you for submitting to Tektonika and we are grateful to the reviewers for their expert input. Our decision is: Revisions Required (please see below for details).

We encourage you to address all of the reviewers' comments thoroughly, documenting all changes made. We look forward to receive a revised version of your manuscript soon together with a rebuttal letter and a manuscript with all the changes marked.

Kind regards,
Robin Lacassin, Executive Editor
Guillaume Duclaux, Associate editor

Associate Editor's Letter:

We have now reached a decision regarding your resubmission to тектоniка, titled "The drivers of lower crustal earthquakes along magma-poor portions of the East African Rift". Our decision is to request moderate revisions to the manuscript. This decision was reached based on a careful assessment of the revised manuscript by the editorial team and constructive feedback from two independent reviewers.

This revised version has been significantly reworked following the first round of reviews, and all parties have concluded that this work is an important contribution to a timely topic. However, some additional edits are still required.

To enhance the manuscript's accessibility and impact, please carefully address the comments from Reviewer 2. Their review is pasted below). I note the following points:

- Some rewording is required to ensure that the scientific claims resulting from this study are not overstated.
- There appears to be an issue with the continuity of both temperature

and gradient at the Moho interface (somewhat linked to Equations 2 and 3). It seems that in Equation 3,T(Zmoho)was calculated for a Moho depth of 40 km (as in Fagereng et al., 2013) rather than 41 km. Please carefully investigate the reason behind the visible temperature discontinuity in the 1D model, as it has direct, albeit relatively minor, rheological implications for the model's interpretation.

Comments by Reviewer 1

(Ameha Muluneh)

Reviewer 1 has simply stated: "The authors have addressed all my comments and suggestions on the earlier version. I have no objections to the manuscript being accepted in its current form. I thank you for the interesting read."

Comments by Reviewer 2

(Rasheed Ajala)

Thanks to the editor for the invitation to review the current work.

The study proposes strain localization in space due to crustal heterogeneity and in space and time due to postseismic deformation on narrow shear zones (~10 m) as a more general mechanism for lower crustal seismicity in the magma-poor segments of the East African Rift System (EARS). Their analysis is based on a recently published regional seismicity catalog for the southern EARS and 1D thermo-rheological modeling.

The manuscript requires more careful language before publication. The rigor of the analysis, i.e., the use of a simple 1D thermo-rheological model and a sparse, low-resolution earthquake catalog with significant uncertainties, does not match the tone of the paper.

While using simple models to propose new mechanisms is okay, it should not be used to make strong arguments against previous hypotheses that use higher-resolution data sets and methods. I cannot support the conclusions in sections 4.2, 4.4, and 4.5 based on simple variations of a 1D model (variations of the “FVT” depth). For these mechanisms – fluids, composition, and geothermal gradient – much is better left unsaid. Rather than claiming that the proposed mechanism has a more regional influence, I strongly suggest simply presenting it as another potential factor that may lead to lower crustal seismicity with or without the influence of the other mechanisms.

In addition to rewording, I highlight other issues regarding the content below. The previous reviewers also mentioned some of these comments.

More on Careful Language

(1) Lines 20-21, 288-291, 550-556, 623-626: The logic here is false and unwarranted. No law states that having different local mechanisms

generate lower crustal seismicity regionally is impossible.

(2) Lines 157-160: The authors need to recall that H23 is a low-resolution earthquake catalog, and just because most of the events in H23 are within 75 km of the rift axes does not necessarily imply that previous geodetic observations of broader deformation zones are incorrect. It is well-known that the seismic moment is usually a fraction of the geodetic moment. Thus, a lot of the deformation is accommodated aseismically or isn't resolved in H23.

Clarity

(3) Lines 145-149: Can the authors briefly state why they selected 75 km? Also, the manuscript does not immediately show how exactly 75 km is measured. Is the distance on either side of the profiles making it a 150 km wide across-rift zone?

(4) Lines 477-478: This statement is false. Heat flow observations are almost as low as 20 and exceed 100 in Figure 4. Lines 478-482: 50 is not an endmember value in Figure 4b. What makes the other geotherms realistic if the stress profile generated using a heat flow value of 50 is unrealistic? Or is it the observation of 50 that is unrealistic? These questions examine the simplistic nature of the 1D models and support my argument for careful language.

Methodology

(5) There appears to be a discontinuity in the geotherm at the Moho, where the temperature slightly decreases, which propagates into the stress profiles (Figure 5). This is an artifact or error, so carefully check the equations or implementation.

Additional References

(6) Reviewer1 and Reviewer2 suggested citing Ajala et al. (2024), which I found relevant in many places throughout the manuscript (Lines 34-37, 52-54, 58-61, 270-274, 454-456), but was not cited.

(7) Buck (2004) should also be cited. It is relevant in Lines 34-37 and 282-286.

Minor comments

(8) Line 142: “magma poor sections” -> “nonvolcanic”
(9) Line 145: “earthquake” -> “earthquakes”
(10) Line 310: Remove the period following the Williams et al. citations.
(11) Section 4.6 should read localization in Space and Time since you

assume a maximum shear zone width of 100 m here (Line 507).

I hope you and the authors find this review helpful.

References
Ajala, R., Kolawole, F., & Menke, W. (2024). Blind magmatism abets nonvolcanic rifting. Nature Communications Earth & Environment, 1-8.
Buck, W. R. (2004). Consequences of asthenospheric variability on continental rifting. In G. D. Karner, B. Taylor, N. W. Driscoll, & D. L. Kohlstedt (Eds.), Rheology and deformation of the lithosphere at continental margins (pp. 1-30).

Authors' Reply to Reviewers

(21 May 2025)

The authors have provided their answer as a comprehensive PDF. It is appended in the following pages.

Note to the editors: we thank you for soliciting the reviews on the manuscript describing the causes of lower crustal seismicity in the southern EARS. Given that Reviewer #1 did not request any changes to this submission, this document only includes comments provided by the Associate Editor and Reviewer #2. These are provided in black non-italicised text, and our replies to them are outlined in italicised blue text. When quoting from the manuscript, we use red text, and line numbers refer to the tracked changes version of the manuscript.

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Associate Editor comments

I note the following points:

• Some rewording is required to ensure that the scientific claims resulting from this study are not overstated.

We have recontextualised the abstract and conclusions so that our proposed mechanism for lower crustal seismicity in the southern EARS (i.e. strain localization in space in time) is now presented in terms of being one of a number of mechanisms for lower crustal seismicity in this region. This is discussed further in our reply to the major comment by Reviewer #2.

• There appears to be an issue with the continuity of both temperature and gradient at the Moho interface (somewhat linked to Equations 2 and 3). It seems that in Equation 3,T(Zmoho)was calculated for a Moho depth of 40 km (as in Fagereng et al., 2013) rather than 41 km. Please carefully investigate the reason behind the visible temperature discontinuity in the 1D model, as it has direct, albeit relatively minor, rheological implications for the model's interpretation.

We have found the source for this slight error, and this has been corrected (see Figure 5 in the revised manuscript). As suggested by the Associate Editor, the influence of this correction on our overall results is negligible.

Reviewer #2 comments

Thanks to the editor for the invitation to review the current work. The study proposes strain localization in space due to crustal heterogeneity and in space and time due to postseismic deformation on narrow shear zones (~10 m) as a more general mechanism for lower crustal seismicity in the magma-poor segments of the East African Rift System (EARS). Their analysis is based on a recently published regional seismicity catalog for the southern EARS and 1D thermo-rheological modeling.

The manuscript requires more careful language before publication. The rigor of the analysis, i.e., the use of a simple 1D thermo-rheological model and a sparse, low-resolution earthquake catalog with significant uncertainties, does not match the tone of the paper. While using simple models to propose new mechanisms is okay, it should not be used to make strong arguments against previous hypotheses that use higher-resolution data sets and methods. I cannot support the conclusions in sections 4.2, 4.4, and 4.5 based on simple variations of a 1D model (variations of the "FVT" depth). For these mechanisms – fluids, composition, and geothermal gradient – much is better left unsaid. Rather than claiming that the proposed mechanism has a more regional influence, I strongly suggest simply presenting it as another potential factor that may lead to lower crustal seismicity with or without the influence of the other mechanisms.

We acknowledge the reviewer's comment and have reworded the manuscript so that instead of proposing that strain localisation is the "predominant regional mechanism for lower crustal seismicity in the southern EARS", it is presented instead as an "additional overlooked mechanism for lower crustal seismicity in the southern EARS."

For example, in the abstract at Lines 20-27:

We then explore the mechanisms that can account for this deep seismicity by combining 1D lithospheric strength profiles with available regional measurements of Moho thickness, crustal VP/VS ratios, and heat flow. As suggested by previous studies, we find that a mafic lower crustal composition, lower geothermal gradient, and/or high pore fluid pressure can locally explain the observed deep seismicity. However, there are sections of the southern EAR's where the lower crust is felsic, dry, and warm, and in these cases, we propose that the embrittlement of the lower crust is best explained by strain localisation in space and time.

And in the conclusions at Lines 782-785:

A mafic lower crust, low geothermal gradient, and the presence of fluids may locally contribute to this observation. However, in some rift segments, the lower crust is neither mafic, wet, nor particularly cold. In these regions, and possibly elsewhere along the southern EARS, we propose that spatial-temporal elevations in strain rate are a key factor driving lower crustal earthquakes.

As outlined in the discussion (Section 4), we maintain that there are challenges with assuming that fluids, mafic composition, and/or a lower geothermal gradient are ebective mechanisms for generating widespread lower crustal seismicity in the EARS. Importantly, these interpretations do not rely on solely on the 1D lithospheric strength profiles; for example, V_P/V_S ratios in the southern EARS also support the notion that fluids cannot account for lower crustal seismicity in the southern EARS. We have therefore not revised Section 4, and ultimately, we will leave it to readers to judge for themselves which of these mechanisms is more consistent with observations of seismicity and crustal structure in the southern EARS.

In addition to rewording, I highlight other issues regarding the content below. The previous reviewers also mentioned some of these comments.

More on Careful Language

(1) Lines 20-21, 288-291, 550-556, 623-626: The logic here is false and unwarranted. No law states that having different local mechanisms generate lower crustal seismicity regionally is impossible.

As outlined in our previous reply, we have removed the sentences that refer to a need for *a single regional mechanism for lower crustal seismicity in the southern EARS; see also the deleted sentences indicated on the tracked change version of the manuscript at Lines 23, 348, 692, and 783.*

(2) Lines 157-160: The authors need to recall that H23 is a low-resolution earthquake catalog, and just because most of the events in H23 are within 75 km of the rift axes does not necessarily imply that previous geodetic observations of broader deformation zones are incorrect. It is well-known that the seismic moment is usually a fraction of the geodetic moment. Thus, a lot of the deformation is accommodated aseismically or isn't resolved in H23.

We first wish to clarify here that although the H23 catalogue is certainly incomplete, it is not necessarily "low resolution" as it incorporates and relocates events recorded by dense temporary seismic networks in southeastern Africa (for a full list see Table 1 in H23).

With regards to the observation that most seismicity is located within 75 km of the rift axis, this is broadly consistent with the 50-150 km rift width indicated by active fault maps in the southern EARS (e.g., Macgregor et al 2015, Williams et al 2022, Grant et al 2024). Indeed, these fault maps are arguably a better metric to compare against the instrumental seismicity than the geodetic measurements, as they provide an indicator of the area over which permanent deformation from multiple earthquake cycles has been accommodated. To this end, we have removed the comparison to the GNSS observations in the revised manuscript.

The reviewer also raises the important point that instrumental catalogues in the southern EARS have sampled only a smaller fraction of this region's available moment rate. Whether the remaining moment rate is released aseismically, or if it represents locked faults and the relatively short period sampled by instrumental catalogs is discussed extensively in Ebinger et al 2019 and Williams et al 2023. We now outline this at Lines 169-174:

In addition, a combination of East Africa's short instrumental period (<100 years) and locked low-slip rate faults means that the moment release indicated by earthquake catalogs represents only a small proportion of the geodetic moment rate (Ebinger et al., 2019; Williams et al., 2023); nevertheless, that is not to say that East Africa's sporadic and incomplete record of seismicity cannot be used to investigate the rheology of its crust.

Clarity

(3) Lines 145-149: Can the authors briefly state why they selected 75 km? Also, the manuscript does not immediately show how exactly 75 km is measured. Is the distance on either side of the profiles making it a 150 km wide across-rift zone?

At lines 184-193, we now outline that the 75 km metric used to filter between events that we consider are in or outside the southern EARS is based on the observation that active fault maps indicate that this section of the rift is generally 50-150 km wide (see also above comment and Figure 1):

In addition, we associate events with each southern EARS rift branch by searching for those that are located within 75 km of the rift axis as shown in Figure 1; this is broadly consistent with the 50-150 km rift widths indicated by southern EARS active fault maps (Grant et al., 2024; Muirhead et al., 2019; Wedmore et al., 2022; Williams et al., 2022c).

As a sensitivity test, we also report the number of "outside rift" events if a 35 km criterion was used to filter rift events (Lines 201-204).

In total, we find that just 37/224 (17%) of the events in the H23 relocated catalogue are located
>75 km from a rift axis (Table 1); this increases to 88/224 (39%) of events if we use a stricter criterion of 35 km to filter rift events. This further supports the notion that the majority of events in the H23 catalogue are events within the southern EARS.

(4) Lines 477-478: This statement is false. Heat flow observations are almost as low as 20 and exceed 100 in Figure 4.

We apologize as the heat flow measurements <=25 mWm^{-2} should have been excluded from this analysis, as in addition to the corrections for sediment compaction (outlined in the initial submission), these outlying heat flow measurements likely reflect measurements made in disturbed sediments (Ebinger et al 1987). This is now outlined at Lines 421-424 in the manuscript, and we have updated Figure 4 accordingly.

We exclude early measurements from Lake Malawi and Lake Tanganyika that indicated heat flows <25 mWm-2 (Degens et al., 1971; Von Herzen and Vacquier, 1967) as these have since been corrected for sediment compaction ebects or reflect measurements made in disturbed sediments (Ebinger, 1989; Ebinger et al., 1987).

Note, we now also apply a heat flow of 65 mWm^{-2} (previously 68 mWm^{-2}) into the 1D lithospheric strength profiles. This represents the mean heat flow from all southern EARS *measurements, as opposed to just measurements from the Western Branch.*

Lines 478-482: 50 is not an endmember value in Figure 4b. What makes the other geotherms realistic if the stress profile generated using a heat flow value of 50 is unrealistic? Or is it the observation of 50 that is unrealistic? These questions examine the simplistic nature of the 1D models and support my argument for careful language.

In response to this, we now use a minimum heat flow estimate of 45 mWm^{-2} (Line 597), which in conjunction with the removal of heat flow values <25 mWm^{-2} (see previous comment), provides a more reasonable lower bound estimate (only 7/47 of rift measurements have a heat flow <45 mWm^{-2}, see also Figure 4).

In this instance, we suggest that a strength profile in Figure 6f that is implemented with a 45 (or 50 mWm^{-2}) heat flow measurement is unrealistic, not the heat flow measurement itself (Line 604). As suggested by the reviewer, in some end member cases, these profiles do not provide a realistic representation of lithospheric strength. However, we should not lose sight of the fact that in most cases, these profiles are remarkably successful in predicting the maximum depth of seismicity (e.g., Brace and Kohlstedt 1980, Sibson 1982, Burov 2011, Williams et al 2025); nor are our interpretations based on these strength profiles alone.

- *Brace, W. F., & Kohlstedt, D. L. (1980). Limits on lithospheric stress imposed by laboratory experiments. Journal of Geophysical Research: Solid Earth, 85(B11), 6248-6252.*
- *Burov, E. B. (2011). Rheology and strength of the lithosphere. Marine and petroleum Geology, 28(8), 1402- 1443.*
- *Sibson, R. H. (1982). Fault zone models, heat flow, and the depth distribution of earthquakes in the continental crust of the United States. Bulletin of the Seismological Society of America, 72(1), 151-163.*
- *Williams, J. N., Eberhart-Phillips, D., Bourguignon, S., Stirling, M. W., & Oliver, W. (2025). Deep and clustered microseismicity at the edge of southern New Zealand's transpressive plate boundary Journal of Geophysical Research: Solid Earth, 130(5), e2024JB030371.*

Methodology

(5) There appears to be a discontinuity in the geotherm at the Moho, where the temperature slightly decreases, which propagates into the stress profiles (Figure 5). This is an artifact or error, so carefully check the equations or implementation.

Thanks for raising this, we have found the cause for this discrepancy and corrected Figure 5. Along with revising our mean heat flow estimate from 68 to 65 mWm^{-2} (see our reply to comment for Lines 477-478), this has mean slightly revising our estimates to the frictional-viscous transition in Table 3; however, these revisions do not alter the main findings of this manuscript.

Additional References

(6) Reviewer1 and Reviewer2 suggested citating Ajala et al. (2024), which I found relevant in many places throughout the manuscript (Lines 34-37, 52-54, 58-61, 270-274, 454-456), but was not cited.

We thank the reviewer for this suggestion, and have added this reference to various points of the manuscript (Lines 70, 328, 331 and 582)). Note, although an interesting study into the presence of lower crustal melts beneath the southern Tanganyika rift, its applicability to elsewhere along the southern EARS is limited, as V_P/V_S values <1.8 on other rift sections are inconsistent with the presence of lower crustal melts (Figure 4)

(7) Buck (2004) should also be cited. It is relevant in Lines 34-37 and 282-286.

Added at Lines 51 and 341

Minor comments

(8) Line 142: “magma poor sections” -> “nonvolcanic”

Replaced (Line 181)

(9) Line 145: “earthquake” -> “earthquakes”

Corrected (Line 184)

(10) Line 310: Remove the period following the Williams et al. citations.

Removed (Line 373)

(11) Section 4.6 should read localization in Space and Time since you assume a maximum shear zone width of 100 m here (Line 507).

Good point - we have retitled this section ‘Transient localisations of strain’ (Line 620)

I hope you and the authors find this review
helpful. Cheers.

References

Ajala, R., Kolawole, F., & Menke, W. (2024). Blind magmatism abets nonvolcanic rifting. Nature Communications Earth & Environment, 1-8.

Buck, W. R. (2004). Consequences of asthenospheric variability on continental rifting. In G.
D. Karner, B. Taylor, N. W. Driscoll, & D. L. Kohlstedt (Eds.), Rheology and deformation of the lithosphere at continental margins (pp. 1-30).

Acceptance Letter

(4 June 2025)

Dear authors

Thanks for your revised manuscripts that take into account the comments by the reviewers and the associate editor. Based on our evaluation we have decided to accept your manuscript today.

You will be contacted by our copy editing team in the coming weeks.

Thanks for submitting to Tektonika !

Robin Lacassin, Tektonika Executive Editor
Guillaume Duclaux, Tektonika Associate Editor