nature portfolio

Peer Review File

Deep mantle earthquakes linked to CO2 degassing at the Mid-Atlantic Ridge

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

The paper investigates the processes involved at MAR by using a high resolution seismic data set recorded over the time period of approx. 3 weeks. Seismicity is used to deduce the brittle to ductile transition zone and this approach has been used widely at MAR to investigate the thermal structure and underlying geodynamic processes. The hypocenter depth is in these kinds of studies the most important parameter and therefore it must be accurately determined. The authors find deep seated seismicity and together with geochemical analysis they infer that CO2 degassing might be a likely process which could facilitate the generation of brittle faulting at these temperature condition.

The authors used NonLinLoc and an apriori P-wave velocity model determined from wide-angle data to constrain the hypocenter depth. They investigated the influence of the apriori P-wave model on the earthquake location and due to the relatively small perturbations in depth observed with different models they concluded that their results are accurate. Unfortunately, a thorough investigation of the S-wave model (or alternatively vp/vs model) is lacking in the paper, and it is only state "Wadati diagrams yield a Vp/Vs ratio of ~1.73 (Supplementary Fig. 3), which is used to estimate the S wave velocity in the inversion." I assume that S-Fig. 3 shows the S, S-P, and P wave arrival after the initial relocations which I also assume has been done using a vp/vs ratio of 1.73. Therefore, there is no surprise that a vp/vs ratio of about 1.65-1.73 is found as this value was used for the initial location. If I look at the material you might find at MAR, water-saturated sediments, basalt, gabbro, peridotite and serpentine I am rather surprised to see a vp/vs ratio of 1.73 as all of them have actually vp/vs ratios greater than 1.73 (e.g. Grevemeyer and Lange, Geology, 2020). Especially the sediments can have vp/vs ratio up to 5 or even larger.

As shown by Gomberg (BSSA, 1990) an accurate S-pick close to the epicentre is needed to constrain accurately the depth, but this of cause also implies that you need an accurate S-wave model! The only way forward in areas where you do not have both, accurate P- and S-wave velocities, is to carry out a joint inversion for hypocentre locations, station corrections and minimum 1D model as outlined by Kissling et al. (JGR, 1994). In this paper it is also clearly stated that using apriori models for the earthquake location problem should be done with great care as these starting models might lead to bias in the model and therefore earthquake locations.

The authors argue that by iteratively updating the station delay terms in NonLinLoc will lead to more accurate locations. However, this iterative procedure is not addressing the simultaneous inversion problem for velocity model, station correction and hypocentral parameters as it only will lead to the optimum station correction terms for a given velocity model and dependent on the starting model used or in other words dependent on the assumed vp/vs ratio. This problems manifest itself that for the station corrections shown the P-wave residuals tend to more negative corrections and the S-wave corrections to more positive values. As I assume S-wave arrival picks are weight less than P-waves, distance depending damping is also part of NonLinLoc the problem to identify where all these contribution come from is rather complicated.

Additionally, the application of the selection criteria for well contained earthquakes is not applied in the right order even for the current case where only stations corrections are iteratively updated. In any joined inversion only the earthquakes which fulfil all criteria (GAP, number of picks, close station, etc…) should be used as otherwise the poorly determined inversion problem becomes completely ill posed (especially for the trade-off between origin time and earthquake depth). For a proper event depth estimate we need a close by station with an S-pick (Gomberg, BSSA, 1990). Looking at table S1 we see that we are then left with 87 usable events for the inversion which are less than 20% of the data set which has been used by the authors to estimate station corrections.

In summary, I therefore do not agree with the statement that the event locations are robust and that the observed arrival times can only be explained by deep events. It is interesting to note that there is quite some controversy in the community regarding the depth of earthquakes at slow and ultra-slow spreading ridges highlighted by the recent comment and reply in Geology 2020 (Gevemeyer and Lange, 2020; Schlindwein, 2020). This further highlights that the absolute depth problem of earthquakes at MAR is not conclusively solved yet.

Finally, I want to highlight that the raw seismic data is restricted on the given WEB site as all other data is as well. I also think it is not acceptable that the underlying travel time data (P- and S-waves), earthquake locations, final velocity model, station locations and corrections are not made accessible to the reviewers for an honest review process. Stating that they will be made available after publication is not helpful for a constructive review process and we should work much more towards Open Data policy.

Reviewer #2 (Remarks to the Author):

The authors report an observation of earthquakes below a mid-oceanic ridge at depths where a brittle deformation is not expected in the mantle, based on thermo-mechanical models. Such earthquakes are very interesting because their occurrence reveals some still poorly understood processes in the upper mantle. The authors present a careful study of the earthquake hypocenters and of the accuracy of their determinations clearly demonstrated that the observed “anomalous” depths are not an artifact. Then, they consider several possible hypotheses about the origin of the observed deep earthquake cluster and based on comparison with tectonic and geological settings argue that that the earthquakes in the hot mantle most likely result from the degassing of CO2 in the mantle. This is a very interesting result well supported by observations. The manuscript is well written and I would like to see it published in Nature Communications. I just have a couple of suggestions as detailed below.

Earthquakes occurring in the deep crust or uppermost mantle where a brittle deformation is not expected are often observed beneath active volcanoes. This class of seismicity is known as deep long period (DLP) volcanic earthquakes (e.g., Shapiro et al., 2017; Wech et al., 2020). In my view, the deep seismicity reported in the present manuscript has many similarities with the volcanic DPL earthquakes and some similar hypotheses have been used for their explanation. In particular, the CO2 degassing has been recently suggested to explain the DLP earthquakes (Melnik, 2020). Therefore, I would suggest to the authors to mention this possible analogy in their manuscript.

The model of Melnik et al. (2020) also contain two interesting aspects that might be worth of mentioned. First the CO_2 solubility and the intensity of degassing depend of the amount of H_2O in the melt. Second, and most interesting, the CO_2 degassing can be more than just a “trigger” of earthquakes, as written by the authors in line 194. As shown by Melnik et al., the gas bubble expansion and associated pressure variations can be rapid enough (i.e., occurring over times less than 1 sec) to be itself the main force causing the generation of seismic waves.

The DLP earthquakes differ from “volcano-tectonic” earthquakes associated with the faulting by their spectral content (this is why they are called LP) and by their focal mechanisms containing significant non-double-couple components (e.g., Ikegaya and Yamamoto, 2021). Therefore, if the authors might have some information about the distinct spectral characteristics of the earthquakes from the deep cluster of out their focal mechanisms, this might be interesting to mention in the paper.

References:
Shapiro, N., Droznin, D., Droznina, S. et al. Deep and shallow long-period volcanic seismicity linked by fluid-pressure transfer. Nature Geosci 10, 442–445 (2017). https://doi.org/10.1038/ngeo2952

Aaron G. Wech et al., Deep long-period earthquakes generated by second boiling beneath Mauna Kea volcano. Science 368, 775-779 (2020). DOI:10.1126/science.aba4798

Melnik, O., Lyakhovsky, V., Shapiro, N.M. et al. Deep long period volcanic earthquakes generated by degassing of volatile-rich basaltic magmas. Nat Communications 11, 3918 (2020). https://doi.org/10.1038/s41467-020-17759-4

Takuma Ikegaya, Mare Yamamoto (2021), Spatio-temporal characteristics and focal mechanisms of deep low-frequency earthquakes beneath the Zao volcano, northeastern Japan, Journal of Volcanology and Geothermal Research, 417, 107321, https://doi.org/10.1016/j.jvolgeores.2021.107321.

Responses to the review comments

(The black words show the review comments; the blue words show our responses)

Reviewer: 1

Reviewer #1 (Remarks to the Author):

The paper investigates the processes involved at MAR by using a high resolution seismic data set recorded over the time period of approx. 3 weeks. Seismicity is used to deduce the brittle to ductile transition zone and this approach has been used widely at MAR to investigate the thermal structure and underlying geodynamic processes. The hypocenter depth is in these kinds of studies the most important parameter and therefore it must be accurately determined. The authors find deep seated seismicity and together with geochemical analysis they infer that CO2 degassing might be a likely process which could facilitate the generation of brittle faulting at these temperature condition.

The authors used NonLinLoc and an apriori P-wave velocity model determined from wide-angle data to constrain the hypocenter depth. They investigated the influence of the apriori P-wave model on the earthquake location and due to the relatively small perturbations in depth observed with different models they concluded that their results are accurate. Unfortunately, a thorough investigation of the S-wave model (or alternatively vp/vs model) is lacking in the paper, and it is only state “Wadati diagrams yield a Vp/Vs ratio of ~1.73 (Supplementary Fig. 3), which is used to estimate the S wave velocity in the inversion.” I assume that S-Fig. 3 shows the S, S-P, and P wave arrival after the initial relocations which I also assume has been done using a vp/vs ratio of 1.73. Therefore, there is no surprise that a vp/vs ratio of about 1.65-1.73 is found as this value was used for the initial location. If I look at the material you might find at MAR, water-saturated sediments, basalt, gabbro, peridotite and serpentine I am rather surprised to see a vp/vs ratio of 1.73 as all of them have actually vp/vs ratios greater than 1.73 (e.g. Grevemeyer and Lange, Geology, 2020). Especially the sediments can have vp/vs ratio up to 5 or even larger.

We agree with the reviewer that the Vp/Vs ratio in the vicinity of RTI and ridge axis could be high, and therefore, we have included several tests with the Vp/Vs ratio ranging from 1.5 to

2.5 (see Supplementary Fig. 5), but we found that the velocity model with a Vp/Vs ratio of ~1.7 leads to the lowest RMS residual and the largest number of earthquakes located. Thus, the choice of the Vp/Vs ratio (1.7) from the Wadati diagram (Supplementary Fig. 4) is justified.

Secondly, the Vp/Vs ratio estimated from the Wadati diagram does not correlate with the initial values. The Vp/Vs ratio from the modified Wadati diagram (*Chatelain, 1978*) can be computed using the following equation:

$$\frac{t_{Si} - t_{Sj}}{t_{Pi} - t_{Pj}} = \frac{\frac{D_i}{Vs} - \frac{D_j}{Vs}}{\frac{D_i}{Vp} - \frac{D_j}{Vp}} = \frac{Vp}{Vs}$$

where for a station pair (i and j), $t_{(Pi,\quad Pj)}$ and $t_{(Si,\quad Sj)}$ are travel times for P- and S-waves, respectively, Vp and Vs are P- and S-wave velocities, respectively, and (Di, Dj) are hypocentral distances. From this equation, the onset times have been removed during the computation. Therefore, the initial setting of the Vp/Vs ratio in the earthquake location does not affect the results from the Wadati diagram.

Thirdly, we agree that the Vp/Vs ratio for unconsolidated sediments can be very high (*Ferrante et al., 2023; Grevemeyer et al., 2019*), and therefore, we have removed S-wave delays caused by the unconsolidated sediments before the inversion, which reduces the effects of the local high Vp/Vs ratios on the earthquake determinations. The S-wave delays in the sediments are estimated using the method outlined by *Yu et al. (2021)*.

It is possible that the Vp/Vs ratios can be highly heterogeneous in a small region. However, one cannot get a detailed 3-D velocity model (or detailed Vp/Vs ratio) in the whole model space before the earthquake location. Indeed, we just can use an averaged velocity model to locate earthquakes. Of course, this kind of model must be carefully selected as we have done in the manuscript.

In summary, our choice of the Vp/Vs ratio of ~1.7 is reasonable.

As shown by Gomberg (BSSA, 1990) an accurate S-pick close to the epicentre is needed to constrain accurately the depth, but this of cause also implies that you need an accurate S-wave model! The only way forward in areas where you do not have both, accurate P- and S-wave velocities, is to carry out a joint inversion for hypocentre locations, station corrections and minimum 1D model as outlined by Kissling et al. (JGR, 1994). In this paper it is also clearly stated that using apriori models for the earthquake location problem should be done with great care as these starting models might lead to bias in the model and therefore earthquake locations.

Indeed, we have been very careful in choosing the 1D velocity. We have referenced a P-wave velocity model from an active-source seismic refraction profile (Wang et al., 2021; Supplementary Fig. 2). Then we used the Wadati diagram (Supplementary Fig. 4) to obtain the Vp/Vs ratio to construct an S-wave velocity model, which is reasonable from several Vp/Vs ratio tests (Supplementary Fig. 5). Furthermore, in the revised version, we have constructed a ″minimum″ 1-D velocity model by the VELEST program (*Kissling et al., 1994*) to locate earthquakes and the results show that the RMS residuals of the two models are close, but our selected model resulted in a larger number of located earthquakes than the inverted ″minimum″ 1-D velocity model (Supplementary Fig. 3). Therefore, we have used ″Model 5″ (see Supplementary Fig. 2a) to locate earthquakes, which is the best fitting velocity model.

The authors argue that by iteratively updating the station delay terms in NonLinLoc will lead to more accurate locations. However, this iterative procedure is not addressing the simultaneous inversion problem for velocity model, station correction and hypocentral parameters as it only will lead to the optimum station correction terms for a given velocity model and dependent on the starting model used or in other words dependent on the assumed vp/vs ratio. This problems manifest itself that for the station corrections shown the P-wave residuals tend to more negative corrections and the S-wave corrections to more positive values. As I assume S-wave arrival picks are weight less than P-waves, distance depending damping is also part of NonLinLoc the problem to identify where all these contribution come from is rather complicated.

We agree with the reviewer that one should perform simultaneous inversion for Vp, Vs and hypocentre locations, but given the geometry of the array and earthquakes, the simultaneous inversion for the velocity model would be poorly constrained. However, our approach will allow a robust relative location, which we can interpret in a more global sense. In fact, this procedure can remove the effects of the sediments or other (*Grevemeyer et al., 2019; Parnell-Turner et al., 2017, 2021; Yu et al., 2021; Chen et al., 2023*).

Additionally, the application of the selection criteria for well contained earthquakes is not applied in the right order even for the current case where only stations corrections are iteratively updated. In any joined inversion only the earthquakes which fulfil all criteria (GAP, number of picks, close station, etc…) should be used as otherwise the poorly determined inversion problem becomes completely ill posed (especially for the trade-off between origin time and earthquake depth). For a proper event depth estimate we need a close by station with an S-pick (Gomberg, BSSA, 1990). Looking at table S1 we see that we are then left with 87 usable events for the inversion which are less than 20% of the data set which has been used by the authors to estimate station corrections.

We have followed the guideline from *Gomberg et al. (1990)* and have constructed a sub-dataset of 403 earthquakes with location quality **A** and **B** (Supplementary Table 1) to perform the earthquake location again, excluding the 112 events that were poorly determined. The following figure shows that the earthquake locations using the two different datasets do not change a lot and therefore, our results are robust and reliable.

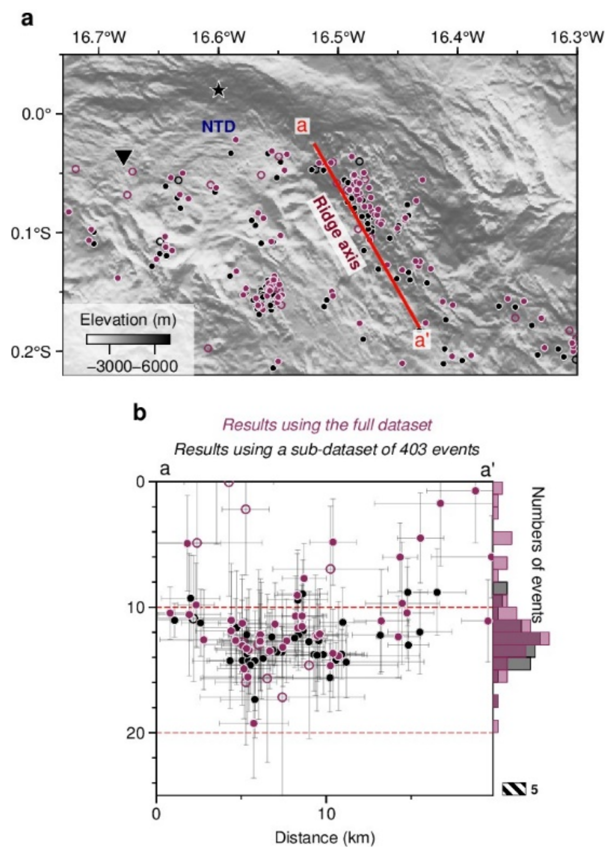

Figure Earthquake location test. (**a**) Bathymetric map and located events. Solid and open dots indicate earthquakes with depth uncertainty of ⩽5 km and 5-10 km, respectively. The colour of the circle indicates the results using the full dataset and a sub-dataset, respectively. (**b**) The focal depth distribution of earthquakes along the profile (aa′) in (**a**).

In summary, I therefore do not agree with the statement that the event locations are robust and that the observed arrival times can only be explained by deep events. It is interesting to note that there is quite some controversy in the community regarding the depth of earthquakes at slow and ultra-slow spreading ridges highlighted by the recent comment and reply in Geology 2020 (Gevemeyer and Lange, 2020; Schlindwein, 2020). This further highlights that the absolute depth problem of earthquakes at MAR is not conclusively solved yet.

We respectfully disagree with the reviewer's judgement; We have performed a thorough analysis and have obtained robust, relative, hypocentral locations, given the geometry of the experiment. We fully appreciate the reviewer's concerns about previous conflicting results and have also participated in this debate (*Yu and Singh, 2023, Nat. Commun.*). *Gevemeyer et al. (2019, Geology)* re-analyzed the deep events by *Schlindwein & Schmid (2016, Nature)* and found that these deep earthquakes were caused by large S-wave delays, caused by the unconsolidated sediment covering the seafloor. We have proposed a novel method to quantify this delay using OBS data (*Yu et al., 2021; Ferrante et al., 2023*) and have removed such delays prior to the inversion. A very recent paper by *Jie et al. (2022, Nat. Commun.)* argued that microearthquakes at ultraslow-spreading mid-ocean ridges do not extend to deeper than 15 km.

Finally, I want to highlight that the raw seismic data is restricted on the given WEB site as all other data is as well. I also think it is not acceptable that the underlying travel time data (P- and S-waves), earthquake locations, final velocity model, station locations and corrections are not made accessible to the reviewers for an honest review process. Stating that they will be made available after publication is not helpful for a constructive review process and we should work much more towards Open Data policy.

We appreciate the reviewer's efforts to Open Data policy, and we agree with it. In the revision, we have provided the raw data for the review process, including P- and S-wave arrivals, earthquake locations, 1-D velocity model, station locations, and station corrections.

References

Chatelain, J. L. Etude fine de la sismicité en zone de collision continentale au moyen d'un réseau de stations portables: la région Hindu-Kush Pamir. (Université scientifique et médicale de Grenoble, 1978).

Chen, J., Crawford, W. C. & Cannat, M. Microseismicity and lithosphere thickness at a nearly-amagmatic oceanic detachment fault system. Nat. Commun. 14, 430 (2023).

Ferrante, G. M. et al. Seismically-derived porosity of deep-sea sediments over the last 74 Ma in the equatorial Atlantic Ocean: Implications for paleo-climate. Earth Planet. Sci. Lett. 612, 118163 (2023).

Kissling, E., Ellsworth, W. L., Eberhart-Phillips, D. & Kradolfer, U. Initial reference models in local earthquake tomography. J. Geophys. Res. Solid Earth 99, 19635–19646 (1994).

Gomberg, J. S., Shedlock, K. M. & Roecker, S. W. The effect of S -wave arrival times on the accuracy of hypocenter estimation. Bull. Seismol. Soc. Am. 80, 1605–1628 (1990).

Grevemeyer, I. et al. Constraining the maximum depth of brittle deformation at slow- and ultraslow-spreading ridges using microseismicity. Geology 47, 1069–1073 (2019).

Parnell-Turner, R. et al. Seismicity trends and detachment fault structure at 13°N, Mid-Atlantic Ridge. Geology 49, 320–324 (2021).

Parnell-Turner, R. et al. Oceanic detachment faults generate compression in extension. Geology 45, 923–926 (2017).

Wang, Z., Singh, S. C., Prigent, C., Gregory, E. P. M. & Marjanović, M. Deep hydration and lithospheric thinning at oceanic transform plate boundaries. Nat. Geosci. 15, 741–746 (2022).

Yu, Z. et al. Semibrittle seismic deformation in high-temperature mantle mylonite shear zone along the Romanche transform fault. Sci. Adv. 7, eabf3388 (2021).

Yu, Z. & Singh, S. C. Increase of P-wave velocity due to melt in the mantle at the Gakkel Ridge. Nat. Commun. 969 (2023).

Reviewer #2 (Remarks to the Author):

The authors report an observation of earthquakes below a mid-oceanic ridge at depths where a brittle deformation is not expected in the mantle, based on thermo-mechanical models. Such earthquakes are very interesting because their occurrence reveals some still poorly understood processes in the upper mantle. The authors present a careful study of the earthquake hypocenters and of the accuracy of their determinations clearly demonstrated that the observed "anomalous" depths are not an artifact. Then, they consider several possible hypotheses about the origin of the observed deep earthquake cluster and based on comparison with tectonic and geological settings argue that that the earthquakes in the hot mantle most likely result from the degassing of CO2 in the mantle. This is a very interesting result well supported by observations. The manuscript is well written and I would like to see it published in Nature Communications. I just have a couple of suggestions as detailed below.
Thank you very much for your positive comments.

Earthquakes occurring in the deep crust or uppermost mantle where a brittle deformation is not expected are often observed beneath active volcanoes. This class of seismicity is known as deep long period (DLP) volcanic earthquakes (e.g., Shapiro et al., 2017; Wech et al., 2020). In my view, the deep seismicity reported in the present manuscript has many similarities with the volcanic DPL earthquakes and some similar hypotheses have been used for their explanation. In particular, the CO2 degassing has been recently suggested to explain the DLP earthquakes (Melnik, 2020). Therefore, I would suggest to the authors to mention this possible analogy in their manuscript.
The model of Melnik et al. (2020) also contain two interesting aspects that might be worth of mentioned. First the CO2 solubility and the intensity of degassing depend of the amount of H2O in the melt. Second, and most interesting, the CO2 degassing can be more than just a "trigger" of earthquakes, as written by the authors in line 194. As shown by Melnik et al., the gas bubble expansion and associated pressure variations can be rapid enough (i.e., occurring over times less than 1 sec) to be itself the main force causing the generation of seismic waves.
We thank the reviewer for providing useful articles. We have included them in the revised manuscript and have some more discussion of the possibility of deep long-period earthquakes. See lines 233-240.

The DLP earthquakes differ from “volcano-tectonic” earthquakes associated with the faulting by their spectral content (this is why they are called LP) and by their focal mechanisms containing significant non-double-couple components (e.g., Ikegaya and Yamamoto, 2021). Therefore, if the authors might have some information about the distinct spectral characteristics of the earthquakes from the deep cluster of out their focal mechanisms, this might be interesting to mention in the paper.

Good point. We have added the spectral analysis of the deep earthquakes beneath the ridge and found that some deep earthquakes lack high-frequency energy (>5 Hz). We suggested that they may be deep long-period events, see Supplementary Fig. 12. However, not all the events are DLP earthquakes and more work about the source process needs to be done in the future.

References:

Shapiro, N., Droznin, D., Droznina, S. et al. Deep and shallow long-period volcanic seismicity linked by fluid-pressure transfer. Nature Geosci 10, 442–445 (2017). https://doi.org/10.1038/ngeo2952

Aaron G. Wech et al., Deep long-period earthquakes generated by second boiling beneath Mauna Kea volcano. Science 368, 775-779 (2020). DOI:10.1126/science.aba4798

Melnik, O., Lyakhovsky, V., Shapiro, N.M. et al. Deep long period volcanic earthquakes generated by degassing of volatile-rich basaltic magmas. Nat Communications 11, 3918 (2020). https://doi.org/10.1038/s41467-020-17759-4

Takuma Ikegaya, Mare Yamamoto (2021), Spatio-temporal characteristics and focal mechanisms of deep low-frequency earthquakes beneath the Zao volcano, northeastern Japan, Journal of Volcanology and Geothermal Research, 417, 107321, https://doi.org/10.1016/j.jvolgeores.2021.107321.

Thanks again for providing these useful articles.

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Although the authors have put a large amount of work into assessing the robustness of their event depth estimates I am still not convinced by them for the following reasons:

1) In the rebuttal letter the authors state that by using the "modified" Wadati approach the origin time is cancelled out and that we can directly observe/measure the vp/vs ratio. This is only correct if we have a constant vp/vs ratio in the whole model as otherwise the ray path of P and S are different and then of cause the distance terms in the equation provided are cancelling out. I think we do all agree that vp/vs at MAR will not be constant with depth.

2) In the panels provided in S4 (Wadati plots) we can clearly see that there are problems with quite a lot of the relative travel time measurements in the standard but also in the modified form as most of the outliers have very large positive deviations. Have they been discarded for the location or left in with the hope that the automatic re-weighting of NonLinLoc gets rid of them? What is concerning me even more that both plots show intercepts of -0.39s and +0.5s which according to the theory/formula you use should not be. This clearly shows that the assumption of a homogeneous vp/vs ratio does not work. 0.5s can easily give a depth change of 4-5 km which is actually the value which is of interest for this paper.

3) The authors use a lot of layers for their 1D model. Looking at the event distribution provided in the rebuttal letter as well most events are part of one depth cluster and only a handful are shallower. To say the least this is a very unfavorable data set to estimate simultaneously event depth and velocities in the overburden. VELEST needs hypocenters at all depth to robustly estimate the minimum 1D model. By using so many layers as you do the problem becomes completely non-linear and depends strongly on the starting model.

Unfortunately, I cannot offer a solution of what to do and how to estimate robustly the event depth. I think all epicentre positions are robust and if you can find a way to rely more on the epicentral positions this could be a very interesting paper. However, as event depth is so important, I think the current data set will not be sufficient to locate the events with travel times only to the accuracy needed to support your conclusions.

Reviewer #2 (Remarks to the Author):

The authors carefully addressed the issues raised in the reviews. In my opinion the paper can be accepted in its present form.

Reviewer #3 (Remarks to the Author):

Review of Yu et al. "Deep mantle earthquakes linked to CO_2 degassing at the Mid-Atlantic Ridge" submitted to Nature Communications

This study uses seismic data collected via ocean-bottom seismometers to suggest the presence of deep earthquakes (10-20 km) along a section of the Mid-Atlantic Ridge between the Romanche and Chain transform faults. The authors attribute the source of the deep earthquakes to CO_2 degassing from melt beneath the ridge. The manuscript is well written. My expertise is within the field of geochemistry, and I therefore defer to the other reviewers for their judgments on the geophysical methods and interpretations presented in this study. In this review, I will focus on the geochemical interpretations laid forth by the authors.

To assess the hypothesis that CO_2 degassing is the cause of the deep earthquakes, the authors download data from PetDB (https://search.earthchem.org/) for the region, and use incompatible trace element abundances (Ba, Rb, Nb) to estimate the CO_2 concentration of the primary melt. The ratio of CO_2/Ba and CO_2/Rb has been shown to be approximately constant in undegassed MORB magmas (e.g., Saal et al., 2002; Hauri et al. 2017; Le Voyer et al., 2019), and using these ratios to estimate the primary melt CO_2 content is a valid approach.

However, this exercise was conducted incorrectly in this study (discussed below) and, moreover, it was not necessary for them to perform these calculations. Extensive research and discussion about the geochemistry, including CO_2 content, of basalts and their mantle sources from this area has been published by Le Voyer et al. (2015, 2019) and Schilling et al. (1994, 1995). The authors do not cite Le Voyer et al. (2015) or Schilling et al. (1994, 1995) in the text and report that there is no CO_2 data for the samples pulled from their PetDB search (Legend in Figure 5), but CO_2 data (and extensive discussion on volatiles) exists for 11 out of 17 of the samples provided in Supplementary Table 6 (and plotted in Figure 5). I am assuming that the authors were unaware of these studies, although they do cite Le Voyer et al. (2015) and Schilling et al., (1995) in Supplementary Table 6, so I'm not sure how incorporation of these studies or the existing CO_2 data got missed. I applaud the authors on providing the sample names from their PetDB search, but I must insist that they incorporate the studies associated with the geochemical data into the main text of the manuscript. This oversight is critical to address, as it has implications to the authors' interpretation of the source of the deep earthquakes. Therefore, I recommend major revisions, but not rejection. I support this work being published, but the authors must first reconcile with the previous geochemical studies conducted in this area before it can be deemed acceptable for publication.

Schilling et al., (1994, 1995) presents geochemical data (major elements, trace elements, radiogenic isotopes), as well as estimates of mantle potential temperature, crustal thickness, depth of melting, and melt fraction for the Mid-Atlantic Ridge from ~5oN to ~6oS, including the ridge between the Romanche and Chain transform faults. Le Voyer et al., (2015) add volatile measurements (H_2O, CO_2, S, F, Cl) to this same suite of samples (5oN to 3oS). In fact, much of Le Voyer et al. (2015) is focused on the ridge segment separating the Romanche and Chain transform faults. They report that the samples from the northern end of this ridge segment have elevated trace element patterns and elevated volatile contents relative to their neighbor samples along the ridge. Le Voyer et al. concludes that these samples are produced through low degrees of partial melting of an enriched source material, which is consistent with the conclusions of Schilling et al. (1995). I strongly recommend that the authors review Schilling et al.

(1995) and Le Voyer et al. (2015) because the data and discussion provided in these two studies are directly linked to the interpretations that the authors wish to make about CO_2 degassing being the source of deep earthquakes in this region.

Le Voyer et al. (2019) calculate the primary melt CO_2 content of global mid-ocean ridge segments (segments as defined by Gale et al., 2013) via CO_2/Ba and CO_2/Rb systematics in undegassed MORB. They include an estimation for the primary melt CO_2 content of segment MARR168 (Mid-Atlantic ridge between 0.202oN and 0.229oS), which is comparable to Yu et al.'s "North" in Figure 5, and MARR 169 (Mid-Atlantic ridge between 0.223oS to 0.643oS), which is comparable to Yu et al.'s "South" in Figure 5. Le Voyer et al. (2019) calculate the primary melt to have a concentration of 5248 ppm CO_2 for segment MARR168 and 2060 ppm CO_2 for segment MARR169 (see Supplementary Table 4 in Le Voyer et al., 2019). In Yu et al., the authors calculate a primary melt CO_2 concentration of >7000 ppm from the same samples and using the same CO_2/Ba and CO_2/Rb systematics. The discrepancy can be explained by Yu et al. not correcting for fractional crystallization to a composition in equilibrium with Fo90 olivine before extrapolating the Ba and Rb concentrations to a CO_2 content. Correcting the geochemical data to be in equilibrium with Fo90 olivine is necessary because, with progressive crystallization, Ba and Rb concentrations will increase in the melt and extrapolation of these concentrations to a CO_2 content will therefore not represent the CO_2 content of the primary, unfractionated melt. The authors must either repeat their calculations with the compositions corrected to Fo90 olivine, or simply cite the estimations of primary melt CO_2 for these segments from Le Voyer et al. (2019).

Overall, I believe that incorporation of these previous studies on the geochemistry of basalts and mantle sources along the MAR between Romanche and Chain transform faults will strengthen the arguments made by Yu et al. regarding CO_2 degassing being the driver of deep earthquakes in this region. The authors must at least give credit to those who have reported on CO_2 and mantle melting in this region previously for this manuscript to be considered acceptable.

A question does arise, however. Hauri et al., (2019) report that a "normal" primary MORB melt has 621 ppm CO_2, based on calculations from the dataset of Le Voyer et al. (2019). The primary melt CO_2 content along the ridge from Romanche to Chain transform faults is significantly higher at both the north and south segments. Why doesn't the south segment also display these deep earthquakes if it also has elevated CO_2 relative to "normal" MORB? Some discussion of this may be warranted to affirm CO_2 degassing as the source of the deep earthquakes and perhaps be an insightful addition to the manuscript.

Detailed remarks:
Abstract
Line 22: "centers" misspelled as "centres"
Lines 26-28: The calculations used to obtain a primary melt concentration of >0.7 wt% CO_2 do not account for crystal fractionation and is thus not representative of the primary melt. Additionally, this sentence reads as though this is a new contribution from this study, whereas it is not. Adjust accordingly following comments above.
Line 30: Suggest delete "in the mantle" from the end of this sentence.
Results

Lines 110-112: Should be <10 km because that's what thermal models predict? Please clarify for reader.
Discussion
Line 193: Recommend deleting the word "(dry)" because CO_2 solubility is strongly dependent on pressure in wet melts as well.
Lines 200-203: It is probably prudent to say somewhere in here that estimating CO_2 concentrations from trace element abundances relies on the assumption that the trace elements are reflective of the mantle source and have not been affected by secondary processes. This is important to say here because some of the samples plotted as "MAR south" in Figure 5 have been affected by secondary processes (see Le Voyer et al., 2015).
Lines 203-233: Strongly recommend complete rewrite incorporating previous works on the volatile contents of samples, interpretations made about mantle melting and mantle heterogeneity, and estimations of primary melt CO_2 concentrations for this section of the ridge.
Lines 203-206: I don't think K_2O/TiO_2 needs to be talked about here. It's not necessary for the discussion.
Line 214: I disagree that all samples from the southern MAR segment (0.2oS to 0.7oS) show normal values. The southern segment still has elevated trace elements relative to normal MORB (see Le Voyer et al., 2015).
Lines 227-230: Right, you can actually pull out the primary melt CO_2 estimation for each specific ridge segment (including the segments relevant to this study) from Supplementary Table 4 in Le Voyer et al. (2019).
Lines 230-233: Recommend deleting sentence starting with "The degassed CO_2-rich fluid that would migrate…..". This is not relevant to this manuscript.
Lines 248-250: This sentence states "Our evidence for a large amount of CO_2 in the melt". Please rephrase because this region having elevated CO_2 is not your evidence or a contribution of this manuscript.
Line 250: Could the text be more specific about what "a large amount of volatiles" refers to? Could you give a concentration limit on what "large" means here?
Lines 252-254: I am not convinced that seismicity at 10-20 km depth means that gabbroic rocks are forming in the mantle. This statement seems completely unfounded. I recommend deletion of this sentence.
CO_2 estimation from mid-ocean ridge basalts (MORB)
Lines 402-411: These calculations are not correct because they do not account for fractional crystallization. Recommend deleting this entire part and referring to the previous works estimating primary melt CO_2 concentration. If the authors wish to redo their calculations, they must first correct the compositions to be in equilibrium with Fo90 olivine.

Figure 5:
•Having the map be in a different orientation than all the other maps in the manuscript is very confusing for the reader. Please consider having North pointing in the same direction as the other maps in this manuscript.
•I suggest removing the K_2O/TiO_2 vs. CO_2 and Nb vs. CO_2 plots. They are a bit overkill to have in addition to the Ba and Rb plots.
•Must replot the data titled "MAR north…" and "MAR south…" because CO_2 data does exist for most of these samples (see Le Voyer et al., 2015). Additionally, this data needs to be corrected for crystal

fractionation before being plotted.

•Four samples are missing from the Ba vs. CO_2 plot. These samples have Ba data, so I'm assuming that was a mistake. If it was not a mistake, please comment in the caption why these data points were excluded from the plot.

•The original sources of the "MAR" geochemical data must be cited, rather than just saying "PetDB". These studies can be cited in the figure caption.

•The melt inclusions in this figure should be cited differently. My understanding of what is being plotted as "melt inclusions" here is that it includes melt inclusions from Figures 4a and b of Le Voyer et al. (2019). The citation for this data needs to either be stated as "summarized in Le Voyer et al. (2019) and Hauri et al. (2018)" or their original data sources should be cited, which can be found in the caption of Figure 4 of Le Voyer et al. (2019) but also includes Saal et al., (2002), Le Voyer et al. (2017) and Hauri et al. (2018). Please confirm and cite appropriately.

•An alternative suggestion to this figure would be to create a new figure that shows the range of primary melt CO_2 estimations for the entire Mid-Atlantic Ridge, highlighting the primary melt CO_2 concentration of this segment for comparison.

Additional comments:

•Overall comment about figures including bathymetry: A rainbow colored pallet is not color-blind friendly. Consider changing the pallet for the bathymetry to a pallet that is color-blind friendly.

References to accompany this review:

Gale, A., Dalton, C. A., Langmuir, C. H., Su, Y., & Schilling, J. G. (2013). The mean composition of ocean ridge basalts. Geochemistry, Geophysics, Geosystems, 14(3), 489-518.

Hauri, E. H., Maclennan, J., McKenzie, D., Gronvold, K., Oskarsson, N., & Shimizu, N. (2018). CO2 content beneath northern Iceland and the variability of mantle carbon. Geology, 46(1), 55-58.

Hauri, E. H., Cottrell, E., Kelley, K. A., Tucker, J. M., Shimizu, K., Le Voyer, M., ... & Saal, A. E. (2019). Carbon in the convecting mantle. Deep carbon: past to present, pp. 237 - 275. DOI: https://doi.org/10.1017/9781108677950

Le Voyer, M., Cottrell, E., Kelley, K. A., Brounce, M., & Hauri, E. H. (2015). The effect of primary versus secondary processes on the volatile content of MORB glasses: An example from the equatorial Mid-Atlantic Ridge (5° N–3° S). Journal of Geophysical Research: Solid Earth, 120(1), 125-144.

Le Voyer, M., Kelley, K. A., Cottrell, E., & Hauri, E. H. (2017). Heterogeneity in mantle carbon content from CO2-undersaturated basalts. Nature Communications, 8(1), 14062.

Le Voyer, M., Hauri, E. H., Cottrell, E., Kelley, K. A., Salters, V. J., Langmuir, C. H., ... & Füri, E. (2019). Carbon fluxes and primary magma CO2 contents along the global mid-ocean ridge system. Geochemistry, Geophysics, Geosystems, 20(3), 1387-1424.

Saal, A. E., Hauri, E. H., Langmuir, C. H., & Perfit, M. R. (2002). Vapour undersaturation in primitive mid-ocean-ridge basalt and the volatile content of Earth's upper mantle. Nature, 419(6906), 451-455.

Schilling, J. G., Hanan, B. B., McCully, B., Kingsley, R. H., & Fontignie, D. (1994). Influence of the Sierra Leone mantle plume on the equatorial Mid-Atlantic Ridge: A Nd-Sr-Pb isotopic study. Journal of Geophysical Research: Solid Earth, 99(B6), 12005-12028.

Schilling, J. G., Ruppel, C., Davis, A. N., McCully, B., Tighe, S. A., Kingsley, R. H., & Lin, J. (1995). Thermal structure of the mantle beneath the equatorial Mid-Atlantic Ridge: Inferences from the spatial variation of dredged basalt glass compositions. Journal of Geophysical Research: Solid Earth, 100(B6), 10057-10076.

Responses to the review comments

(The black words show the review comments; the blue words show our responses)

Reviewer: 1

Reviewer #1 (Remarks to the Author):

Although the authors have put a large amount of work into assessing the robustness of their event depth estimates I am still not convinced by them for the following reasons:

1) In the rebuttal letter the authors state that by using the "modified" Wadati approach the origin time is cancelled out and that we can directly observe/measure the vp/vs ratio. This is only correct if we have a constant vp/vs ratio in the whole model as otherwise the ray path of P and S are different and then of cause the distance terms in the equation provided are cancelling out. I think we do all agree that vp/vs at MAR will not be constant with depth.

We agree that the Vp/Vs ratio in our study region cannot be constant, but upon testing the optimum Vp/Vs ratio selection (see Supplementary Fig. S5), we found that a ratio of 1.7 can result in the lowest RMS values. The Vp/Vs ratio between 1.7 to 1.9 can yield better results than the others, indicating that the study region has a normal Vp/Vs ratio. In addition, our previous seismic tomography results (Yu et al., 2023 *EPSL*) showed that the Vp/Vs ratio along the present Mid-Atlantic Ridge (MAR) is not very variable, lying around 1.7 (see Figure R1 below). To further demonstrate this, we displayed the earthquake depths using the different Vp/Vs ratios (1.7-1.9), all of which resulted in deep events beneath the MAR (see Figure R2 below). Thus, we believe that our choice of Vp/Vs value is appropriate.

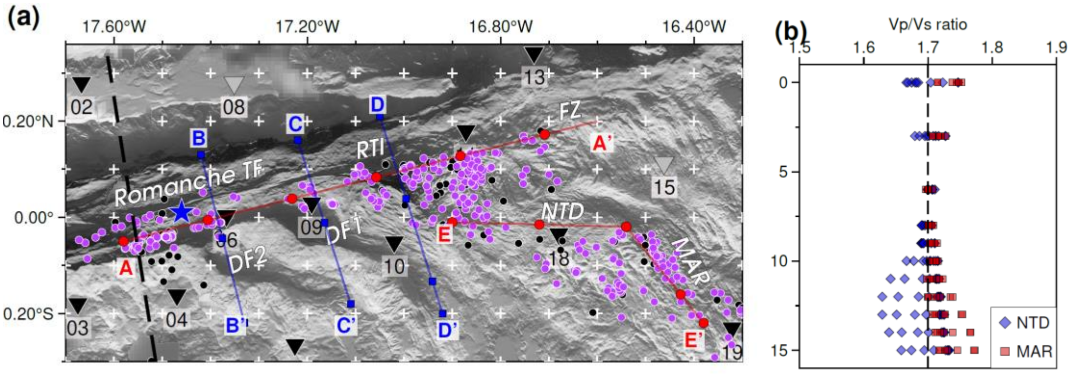

Figure R1. Vp/Vs results along the MAR from seismic tomography results by Yu et al. (2023) *EPSL*. (**a**) Bathymetric map shows earthquake locations. (**b**) The blue diamonds and red squares show Vp/Vs ratios along the non-transform continuity (NTD) and MAR, respectively.

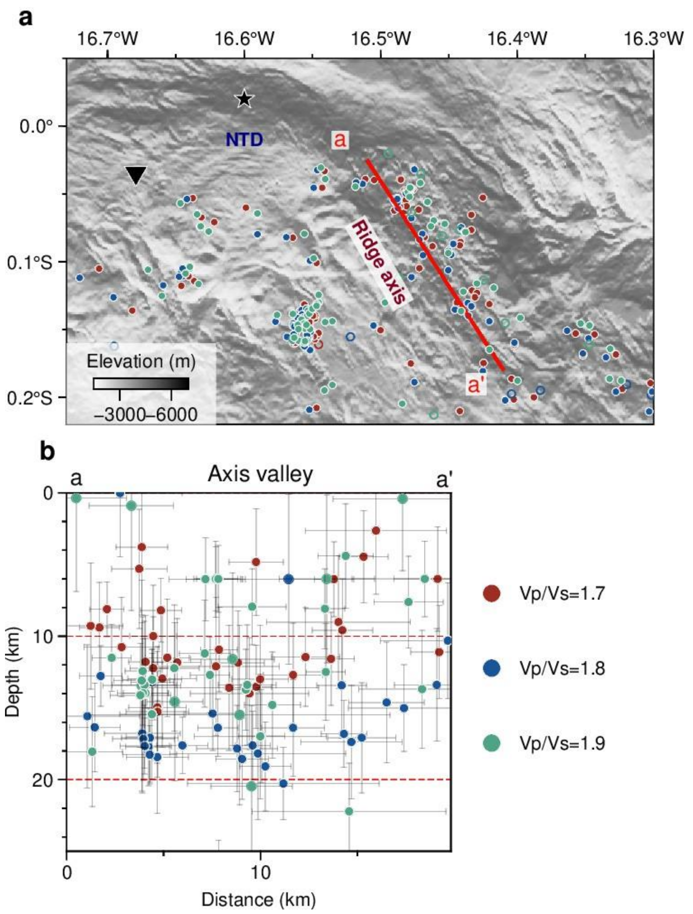

Figure R2. Earthquake depths along the MAR. (**a**) Bathymetric map and located events. Solid and open dots indicate earthquakes with a depth uncertainty of ⩽5 km and 5-10 km, respectively. Coloured circles indicate the location results using the different Vp/Vs ratios shown in (**b**). The earthquake depths along the transect a-a′ are shown in (**b**). The black star indicates an inactive hydrothermal mound observed during the submersible dive. (**b**) The focal depth distribution of earthquakes along the transect (aa′). Grey lines mark the uncertainties. Coloured circles show the location results using different Vp/Vs ratios from 1.7 to 1.9.

2) In the panels provided in S4 (Wadati plots) we can clearly see that there are problems with quite a lot of the relative travel time measurements in the standard but also in the modified form as most of the outliers have very large positive deviations. Have they been discarded for the location or left in with the hope that the automatic re-weighting of NonLinLoc gets rid of them? What is concerning me even more is that both plots show intercepts of -0.39s and +0.5s which according to the theory/formula you use should not be. This clearly shows that the assumption of a homogeneous vp/vs ratio does not work. 0.5s can easily give a depth change of 4-5 km which is the value which is of interest for this paper.

Thank you for your concern. For the large positive deviations, yes, they will be reweighted and removed automatically during the NonLinLoc calculation. We replotted the Wadati diagram using the travels with a weighting factor greater than 0.6. The results show that many large deviations were removed, and we can obtain a more linear and narrow slope (see new

Supplementary Fig. S4), which suggests a Vp/Vs value of ~1.7. Note that for Wadati diagrams, it is unlikely that the P- and S-travels will be perfectly aligned along a line. As we answered in the last comment, the Vp/Vs ratios in this study range from 1.7 to 1.9, without much variation. In addition, we wish to clarify that all we have done in this section is try to get a better 1-D starting velocity model rather than obtaining a true velocity model.

3) The authors use a lot of layers for their 1D model. Looking at the event distribution provided in the rebuttal letter as well most events are part of one depth cluster and only a handful are shallower. To say the least, this is a very unfavorable data set to estimate simultaneously event depth and velocities in the overburden. VELEST needs hypocenters at all depth to robustly estimate the minimum 1D model. By using so many layers as you do the problem becomes completely non-linear and depends strongly on the starting model.

It should be noted first that we used a continuous velocity model to locate earthquakes with the NonLinloc program (see Supplementary Fig. S2). Second, we fully understand your concerns about the number of layers for the calculation using the VELEST program. We have conducted a new test to reduce the layer number of the model; see Figure R3 below. The results show that the earthquake locations did not change much. Thus, we believe that our setting of the starting model is reliable.

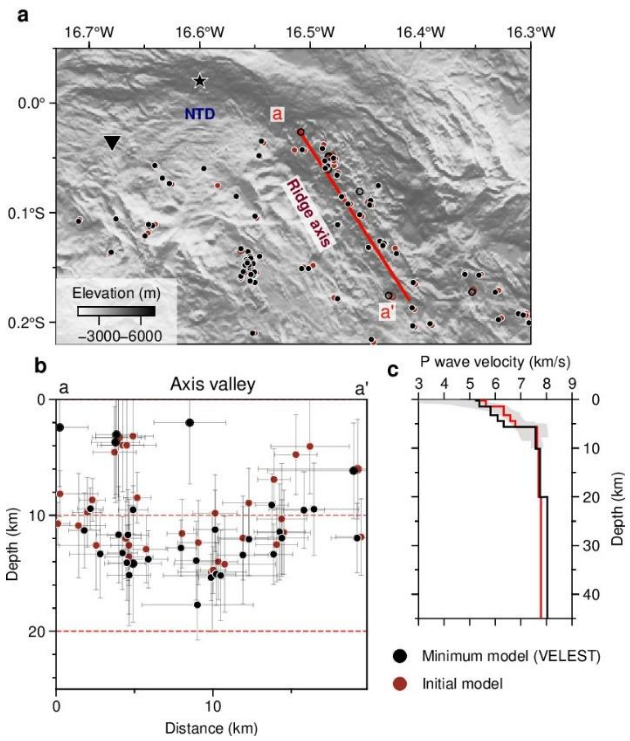

Figure R3. Earthquake depths along the MAR. (**a**) Bathymetric map and located events. The coloured circles indicate the results using different velocity models shown in (**b**). The other labelling is the same as that shown in Figure R2. (**b**) The focal depth distribution of earthquakes along the transect (aa′) in (a). The velocity models are shown on the right, including one model (black line) with the decreased layers in the VELEST program.

Unfortunately, I cannot offer a solution of what to do and how to estimate robustly the event depth. I think all epicentre positions are robust and if you can find a way to rely more on the epicentral positions this could be a very interesting paper. However, as event depth is so important, I think the current data set will not be sufficient to locate the events with travel times only to the accuracy needed to support your conclusions.
We are sorry that our previous revision did not satisfy your requirements. We think that we have now addressed your concerns with the latest revision.

Reference

1. Yu, Z., Singh, S. C. & Maia, M. Evidence for low Vp/Vs ratios along the eastern Romanche ridge-transform intersection in the equatorial Atlantic Ocean. *Earth and Planetary Science Letters* **621**, 118380 (2023).

Reviewer: 2

Reviewer #2 (Remarks to the Author):

The authors carefully addressed the issues raised in the reviews. In my opinion the paper can be accepted in its present form.
Thank you very much for your positive comments.

Reviewer: 3

Reviewer #3 (Remarks to the Author):

Review of Yu et al. “Deep mantle earthquakes linked to CO_2 degassing at the Mid-Atlantic Ridge” submitted to Nature Communications

This study uses seismic data collected via ocean-bottom seismometers to suggest the presence of deep earthquakes (10-20 km) along a section of the Mid-Atlantic Ridge between the Romanche and Chain transform faults. The authors attribute the source of the deep earthquakes to CO_2 degassing from melt beneath the ridge. The manuscript is well written.

My expertise is within the field of geochemistry, and I therefore defer to the other reviewers for their judgments on the geophysical methods and interpretations presented in this study. In this review, I will focus on the geochemical interpretations laid forth by the authors.

Thank you very much for your positive comments.

To assess the hypothesis that CO2 degassing is the cause of the deep earthquakes, the authors download data from PetDB (https://search.earthchem.org/) for the region, and use incompatible trace element abundances (Ba, Rb, Nb) to estimate the CO2 concentration of the primary melt. The ratio of CO2/Ba and CO2/Rb has been shown to be approximately constant in undegassed MORB magmas (e.g., Saal et al., 2002; Hauri et al. 2017; Le Voyer et al., 2019), and using these ratios to estimate the primary melt CO2 content is a valid approach.

However, this exercise was conducted incorrectly in this study (discussed below) and, moreover, it was not necessary for them to perform these calculations. Extensive research and discussion about the geochemistry, including CO2 content, of basalts and their mantle sources from this area has been published by Le Voyer et al. (2015, 2019) and Schilling et al. (1994, 1995). The authors do not cite Le Voyer et al. (2015) or Schilling et al. (1994, 1995) in the text and report that there is no CO2 data for the samples pulled from their PetDB search (Legend in Figure 5), but CO2 data (and extensive discussion on volatiles) exists for 11 out of 17 of the samples provided in Supplementary Table 6 (and plotted in Figure 5). I am assuming that the authors were unaware of these studies, although they do cite Le Voyer et al. (2015) and Schilling et al., (1995) in Supplementary Table 6, so I'm not sure how incorporation of these studies or the existing CO2 data got missed. I applaud the authors on providing the sample names from their PetDB search, but I must insist that they incorporate the studies associated with the geochemical data into the main text of the manuscript. This oversight is critical to address, as it has implications to the authors' interpretation of the source of the deep earthquakes. Therefore, I recommend major revisions, but not rejection. I support this work being published, but the authors must first reconcile with the previous geochemical studies conducted in this area before it can be deemed acceptable for publication.

We have now corrected this oversight and included the articles by Le Voyer et al. (2015) and Schilling et al. (1994, 1995) in the discussion; see lines 220-222. We also added the missing CO_2 data in the new Figure 5. The geochemistry part of the discussion went through a comprehensive overhaul. See lines 208-243.

Schilling et al., (1994, 1995) presents geochemical data (major elements, trace elements, radiogenic isotopes), as well as estimates of mantle potential temperature, crustal thickness, depth of melting, and melt fraction for the Mid-Atlantic Ridge from ~5oN to ~6oS, including the ridge between the Romanche and Chain transform faults. Le Voyer et al., (2015) add volatile measurements (H2O, CO2, S, F, Cl) to this same suite of samples (5oN to 3oS). In fact, much of Le Voyer et al. (2015) is focused on the ridge segment separating the Romanche and Chain transform faults. They report that the samples from the northern end of this ridge segment have elevated trace element patterns and elevated volatile contents relative to their neighbor samples along the ridge. Le Voyer et al. concludes that these samples are produced through low degrees of partial melting of an enriched source material, which is consistent with the conclusions of Schilling et al. (1995). I strongly recommend that the authors review Schilling et al. (1995) and Le Voyer et al. (2015) because the data and discussion provided in these two studies are directly linked to the interpretations that the authors wish to make about CO2 degassing being the source of deep earthquakes in this region.

We made some alterations to our discussion to include the conclusions of Le Voyer et al. (2015), see lines 228-231. We only selected the data within our OBS network for this study, but we fully agree with the reviewer that the conclusions of Le Voyer et al. (2015) are significant and very relevant to us.

Le Voyer et al. (2019) calculate the primary melt CO2 content of global mid-ocean ridge segments (segments as defined by Gale et al., 2013) via CO2/Ba and CO2/Rb systematics in undegassed MORB. They include an estimation for the primary melt CO2 content of segment MARR168 (Mid-Atlantic ridge between 0.202oN and 0.229oS), which is comparable to Yu et al.'s "North" in Figure 5, and MARR 169 (Mid-Atlantic ridge between 0.223oS to 0.643oS), which is comparable to Yu et al.'s "South" in Figure 5. Le Voyer et al. (2019) calculate the primary melt to have a concentration of 5248 ppm CO2 for segment MARR168 and 2060 ppm CO2 for segment MARR169 (see Supplementary Table 4 in Le Voyer et al., 2019). In Yu et al., the authors calculate a primary melt CO2 concentration of >7000 ppm from the same samples and using the same CO2/Ba and CO2/Rb systematics. The discrepancy can be explained by Yu et al. not correcting for fractional crystallization to a composition in equilibrium with Fo90 olivine before extrapolating the Ba and Rb concentrations to a CO2 content. Correcting the geochemical data to be in equilibrium with Fo90 olivine is necessary because, with progressive crystallization, Ba and Rb concentrations will increase in the melt and extrapolation of these concentrations to a CO2 content will therefore not represent the CO2 content of the primary, unfractionated melt. The authors must either repeat their calculations with the compositions corrected to Fo90 olivine, or simply cite the estimations of primary melt CO2 for these segments from Le Voyer et al. (2019).

Thank you for the nice suggestion. We have recalculated CO_2 concentration in primary melts using Ba and Rb corrected for fractional crystallisation (equilibrated to Fo_{90}). We changed the Figure 5 accordingly. Also, we removed Nb and K2O/TiO2 plots in the new Figure 5, in response to the reviewer's later suggestion. We also included in the discussion a sentence to give credit to Schilling et al. (1995) and Le Voyer et al. (2015) for the interpretation of elevated primary CO_2 in RC3 as a result of low melting degrees of an enriched mantle source; see lines 228-231.

We feel it is important to note that the ridge segments of this study that we named RC2 and RC3 (Romanche-Chain segment numbers 2 and 3) are indeed the same as the ones named MARR168 and MARR169 in Gale et al. (2013) and Le Voyer et al. (2019). However, we decided against using the average CO_2 value calculated by Le Voyer et al. (2019) for the RC2 segment as their calculations are based on a single dredge: CON2806-018 and exclude samples from CON2806-007-001 and 13-12 49A, both of which are significantly more enriched and coming from other stations along the same segment. Thus, we recalculated the CO_2 concentration as you suggested.

We believe that a major point of conflict with the reviewer is primary vs. pre-eruptive CO2 concentration. The reviewer is interested in the causes of the CO_2 enrichment and therefore focuses on the primary CO_2 content in melts in equilibrium with their mantle source (Le Voyer et al., 2015 and 2019), while we are interested in the effects of the high CO_2 content in the pre-eruptive melts (which underwent melt storage and fractional crystallisation) as a possible source of the deep earthquakes. Before calculating the CO_2 solubility, we added a sentence stating that we are referring to pre-eruptive melts as magmas that underwent various amounts of fractional crystallisation; see lines 232-241.

Overall, I believe that incorporation of these previous studies on the geochemistry of basalts and mantle sources along the MAR between Romanche and Chain transform faults will strengthen the arguments made by Yu et al. regarding CO2 degassing being the driver of deep earthquakes in this region. The authors must at least give credit to those who have reported on CO2 and mantle melting in this region previously for this manuscript to be considered acceptable.

We feel that the new version of the discussion gives better credit to these studies. We also made some changes to how we named the two groups of samples, as we think it was the cause of some misunderstanding with the reviewer: the names “MAR south of 0.2” and “MAR north of 0.2” were probably too vague and were suggesting a boundary within the entire MAR at 0.2º N. We are limited by the bounds of our OBS network, so we renamed those groups: “RC2 basalts” and “RC3 basalts”; see Figure 5.

A question does arise, however. Hauri et al., (2019) report that a “normal” primary MORB melt has 621 ppm CO2, based on calculations from the dataset of Le Voyer et al. (2019). The primary melt CO2 content along the ridge from Romanche to Chain transform faults is significantly higher at both the north and south segments. Why doesn’t the south segment also display these deep earthquakes if it also has elevated CO2 relative to “normal” MORB? Some discussion of this may be warranted to affirm CO2 degassing as the source of the deep earthquakes and perhaps be an insightful addition to the manuscript.

Yes, there may be some deep events in the southern MAR close to the Chain transform fault, but unfortunately, these earthquakes are outside of our seismic observation network. We are unable to verify these suspicions.

Detailed remarks:

Abstract

Line 22: “centers” misspelled as “centres”

We used the British English spelling. We have standardized all these similar terms in the text.

Lines 26-28: The calculations used to obtain a primary melt concentration of >0.7 wt% CO2 do not account for crystal fractionation and is thus not representative of the primary melt. Additionally, this sentence reads as though this is a new contribution from this study, whereas it is not. Adjust accordingly following comments above.

We have rewritten this sentence and used the new CO_2 data with crystal fractionation corrections. See lines 22-24.

Line 30: Suggest delete “in the mantle” from the end of this sentence.

Done. See line 26.

Results

Lines 110-112: Should be <10 km because that's what thermal models predict? Please clarify for reader.

Done. We have clarified this statement. See lines 110-112.

Discussion

Line 193: Recommend deleting the word "(dry)" because CO_2 solubility is strongly dependent on pressure in wet melts as well.

Done. We have removed it. See line 202.

Lines 200-203: It is probably prudent to say somewhere in here that estimating CO_2 concentrations from trace element abundances relies on the assumption that the trace elements are reflective of the mantle source and have not been affected by secondary processes. This is important to say here because some of the samples plotted as "MAR south" in Figure 5 have been affected by secondary processes (see Le Voyer et al., 2015).

Done. We have added this statement to the discussion. See lines 217-220.

Lines 203-233: Strongly recommend complete rewrite incorporating previous works on the volatile contents of samples, interpretations made about mantle melting and mantle heterogeneity, and estimations of primary melt CO_2 concentrations for this section of the ridge.

We have rewritten this section, and correcting for fractional crystallisation was also addressed. See lines 208-231.

Lines 203-206: I don't think K_2O/TiO_2 needs to be talked about here. It's not necessary for the discussion.

We have removed the statements and the K_2O/TiO_2 plotting in Figure 5.

Line 214: I disagree that all samples from the southern MAR segment (0.2oS to 0.7oS) show normal values. The southern segment still has elevated trace elements relative to normal MORB (see Le Voyer et al., 2015).

Changes have been made, and this question doesn't exist anymore.

Lines 227-230: Right, you can actually pull out the primary melt CO2 estimation for each specific ridge segment (including the segments relevant to this study) from Supplementary Table 4 in Le Voyer et al. (2019).

We have added a Supplementary Fig. S13 to show the CO_2 abundance in the equatorial Atlantic Ocean using the data from Le Voyer et al. (2019).

Lines 230-233: Recommend deleting sentence starting with "The degassed CO2-rich fluid that would migrate…..". This is not relevant to this manuscript.

Done.

Lines 248-250: This sentence states "Our evidence for a large amount of CO2 in the melt". Please rephrase because this region having elevated CO2 is not your evidence or a contribution of this manuscript.

This part of the text was rewritten to give better credit to the relevant previous studies. See lines 268-270.

Line 250: Could the text be more specific about what "a large amount of volatiles" refers to? Could you give a concentration limit on what "large" means here?

We are sorry that we cannot provide such a concentration limit, which is beyond the aim of this manuscript.

Lines 252-254: I am not convinced that seismicity at 10-20 km depth means that gabbroic rocks are forming in the mantle. This statement seems completely unfounded. I recommend deletion of this sentence.

Done. We have removed this statement. See lines 272-274.

CO2 estimation from mid-ocean ridge basalts (MORB)

Lines 402-411: These calculations are not correct because they do not account for fractional crystallization. Recommend deleting this entire part and referring to the previous works estimating primary melt CO2 concentration. If the authors wish to redo their calculations, they must first correct the compositions to be in equilibrium with Fo90 olivine.

Now, we have recalculated the CO_2 concentration with fractional crystallisation corrections.

See new Figure 5 and Method section (Lines 423-450).

Figure 5:

•Having the map be in a different orientation than all the other maps in the manuscript is very confusing for the reader. Please consider having North pointing in the same direction as the other maps in this manuscript.

We have replotted the bathymetry map as you suggested. See Figure 5a.

•I suggest removing the K2O/TiO2 vs. CO2 and Nb vs. CO2 plots. They are a bit overkill to have in addition to the Ba and Rb plots.

We have removed the K2O/TiO2 vs. CO2 and Nb vs. CO2 plots as you suggested.

•Must replot the data titled "MAR north…" and "MAR south…" because CO2 data does exist for most of these samples (see Le Voyer et al., 2015). Additionally, this data needs to be corrected for crystal fractionation before being plotted.

The new version of this figure now includes all CO_2 values from Le Voyer et al. (2015) and we are correcting fractional crystallisation for Ba and Rb to an olivine Fo_{90}. See Figure 5b and 5c.

•Four samples are missing from the Ba vs. CO2 plot. These samples have Ba data, so I'm assuming that was a mistake. If it was not a mistake, please comment in the caption why these data points were excluded from the plot.

Looking at the references cited and carefully digging through petDB data, we have not found any additional samples in this area for the Ba/CO_2 plot. In Le Voyer et al. (2015), CON2806-010 and CON2806-013 are listed as sampled along what they call segment "8" (which could be relevant for us), but those samples are collected further south, outside the bounds of our OBS network, and would not plot on our maps.

•The original sources of the "MAR" geochemical data must be cited, rather than just saying "PetDB". These studies can be cited in the figure caption.

Agree and changes have been made. See figure legend in Figure 5.

•The melt inclusions in this figure should be cited differently. My understanding of what is being plotted as "melt inclusions" here is that it includes melt inclusions from Figures 4a and b of Le Voyer et al. (2019). The citation for this data needs to either be stated as "summarized in Le Voyer et al. (2019) and Hauri et al. (2018)" or their original data sources should be cited, which can be found in the caption of Figure 4 of Le Voyer et al. (2019) but also includes Saal et al., (2002), Le Voyer et al. (2017) and Hauri et al. (2018). Please confirm and cite appropriately.

We are now citing them together in Figure 5 and the caption.

•An alternative suggestion to this figure would be to create a new figure that shows the range of primary melt CO2 estimations for the entire Mid-Atlantic Ridge, highlighting the primary melt CO2 concentration of this segment for comparison.

We plotted the calculated CO_2 values for the entire MAR; see Figure R4 below. However, we feel that the scale is orders of magnitude larger than our studied area, thus we include it as Supplementary Material (Fig. S13).

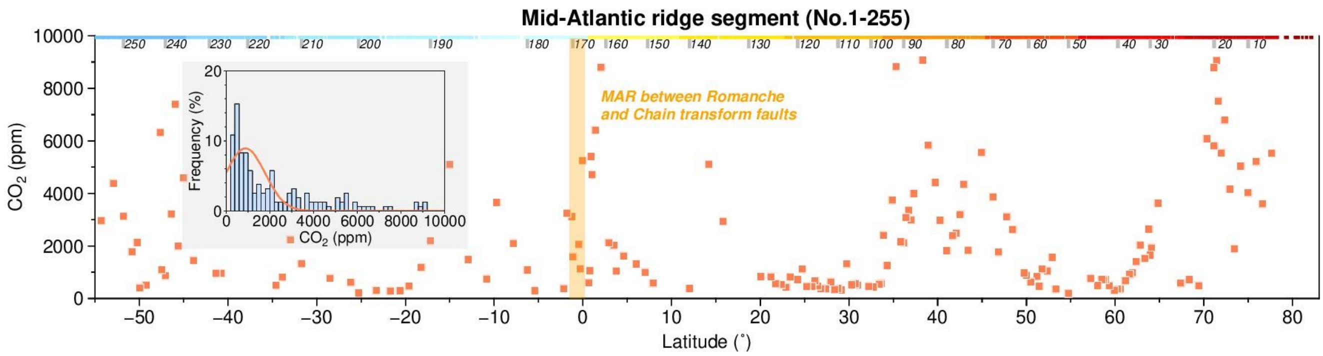

Figure R4. CO_2 contents in the primary magma along the whole Mid-Atlantic Ridge segments. Segment-averaged CO_2 content is extracted from Le Voyer et al. (2019), and the segment number (1-255) is shown on the top. The inset histogram shows the distribution of the primary melt CO_2 contents. The orange belt shows the CO_2 contents along the MAR segments between the Romanche and Chain transform faults.

Additional comments:

•Overall comment about figures including bathymetry: A rainbow colored pallet is not color-blind friendly. Consider changing the pallet for the bathymetry to a pallet that is color-blind friendly.

We are now using a more friendly colour scale (viridis) to show the bathymetry in Figures 1-3 and 5.

References to accompany this review:

Gale, A., Dalton, C. A., Langmuir, C. H., Su, Y., & Schilling, J. G. (2013). The mean composition of ocean ridge basalts. Geochemistry, Geophysics, Geosystems, 14(3), 489-518.

Hauri, E. H., Maclennan, J., McKenzie, D., Gronvold, K., Oskarsson, N., & Shimizu, N. (2018). CO2 content beneath northern Iceland and the variability of mantle carbon. Geology, 46(1), 55-58.

Hauri, E. H., Cottrell, E., Kelley, K. A., Tucker, J. M., Shimizu, K., Le Voyer, M., ... & Saal, A. E. (2019). Carbon in the convecting mantle. Deep carbon: past to present, pp. 237 - 275. DOI: https://doi.org/10.1017/9781108677950

Le Voyer, M., Cottrell, E., Kelley, K. A., Brounce, M., & Hauri, E. H. (2015). The effect of primary versus secondary processes on the volatile content of MORB glasses: An example from the equatorial Mid‐Atlantic Ridge (5° N–3° S). Journal of Geophysical Research: Solid Earth, 120(1), 125-144.

Le Voyer, M., Kelley, K. A., Cottrell, E., & Hauri, E. H. (2017). Heterogeneity in mantle carbon content from CO2-undersaturated basalts. Nature Communications, 8(1), 14062.

Le Voyer, M., Hauri, E. H., Cottrell, E., Kelley, K. A., Salters, V. J., Langmuir, C. H., ... & Füri, E. (2019). Carbon fluxes and primary magma CO2 contents along the global mid‐ocean ridge system. Geochemistry, Geophysics, Geosystems, 20(3), 1387-1424.

Saal, A. E., Hauri, E. H., Langmuir, C. H., & Perfit, M. R. (2002). Vapour undersaturation in primitive mid-ocean-ridge basalt and the volatile content of Earth's upper mantle. Nature, 419(6906), 451-455.

Schilling, J. G., Hanan, B. B., McCully, B., Kingsley, R. H., & Fontignie, D. (1994). Influence of the Sierra Leone mantle plume on the equatorial Mid‐Atlantic Ridge: A Nd‐Sr‐Pb isotopic study. Journal of Geophysical Research: Solid Earth, 99(B6), 12005-12028.

Schilling, J. G., Ruppel, C., Davis, A. N., McCully, B., Tighe, S. A., Kingsley, R. H., & Lin, J. (1995). Thermal structure of the mantle beneath the equatorial Mid‐Atlantic Ridge: Inferences from the spatial variation of dredged basalt glass compositions. Journal of Geophysical Research: Solid Earth, 100(B6), 10057-10076.The authors carefully addressed the issues raised in the reviews. In my opinion the paper can be accepted in its present form.

Thank you very much for providing the useful references.

REVIEWERS' COMMENTS

Reviewer #3 (Remarks to the Author):

This revised manuscript has improved significantly from the original version, especially with regards to the geochemical interpretations and discussion. The authors have addressed by concerns sufficiently and I recommend acceptance of the revised manuscript.

Responses to the review comments

(The black words show the review comments; the blue words show our responses)

Reviewer: 3

Reviewer #1 (Remarks to the Author):

This revised manuscript has improved significantly from the original version, especially with regards to the geochemical interpretations and discussion. The authors have addressed by concerns sufficiently and I recommend acceptance of the revised manuscript.

Thank you very much for your positive comments.