Dear authors,

Thank you for your very interesting and important study on the formation and evolution of transform faults in the early Southern Atlantic Ocean. The manuscript provides many new insights into the structure of transform faults based on various kinds of geophysical data. The study builds on a previous study by Thomas et al. (2022) that presented 3D broadband seismic reflection data. Using the structural information from the seismic reflection data, the new study analyses and models potential field data to get a better understanding on the lithology and potential metamorphic processes in the lower crust. The study is of high interest and provides many original aspects. However, before the manuscript could get published I would recommend some moderate to major revisions.

We thank the reviewer for the thorough analysis of our submitted manuscript. The comments are very helpful for us and guided us while improving the manuscript. Especially the recommendation to add a schematic figure in the discussion helped us to strengthen the link between metamorphic conditions and magnetization of the lower crust. We appreciate that the reviewer appraises our study as interesting and important and hope that our revised version will be valuable for scientific community working on buried transform faults. Below we address all comments suggested by the reviewer individually.

The data and methods chapter is not yet well elaborated and needs substantially more details about the actual pre-processing and processing of the potential field data. At least proper references should be given, that it is possible to understand, which corrections were applied. E.g., how the ship-borne data were tied into the global reference net? What is the actual resolution of the data, and what are the uncertainties? Did you run any resolution tests? In some cases, it seems to me that you try to overfit the data. Also, the ERR values do not really represent the uncertainties as can be seen from various figures.

We understand that the data and methods chapter may come a bit short compared to the other sections. At this point, we want to emphasize that the processing of the potential field data was not done by us, but by CGG Multi-Physics. Therefore, less references are given in the manuscript. However, a processing report is at hand for us, which allows us to judge that the processing of the potential field data has been caried out carefully and extensively. Nevertheless, we significantly extended the Methods section, as suggested by the reviewer. As the main focus of the manuscript is about the interpretation of buried fracture zones, we shifted most of the gravity and magnetic processing information to the Appendix.

The processing section of the revised manuscript is split in two subsections “A1.1 Gravity data processing” and “A1.2 Magnetic data processing”. Especially in Section A1.1, we introduce equations and explanations, which presumably help the reader to comprehend the corrections applied to the gravity data. There we also explain, how the gravity measurements were tied in the world gravity network. Gravity data were gridded in 200 m distance. Regarding the data quality, CGG used high-quality assessment tools, yielding in an expected gravity data quality of 0.5-1 mGal and less. As indicated in the figure captions, ERR represents the standard (deviation) error of the residual gravity and magnetic data. In this context, it represents a proxy for the fit of the data rather than the inherent error of the data itself.

I wonder a bit that only the lower crust is considered as a source for magnetic anomalies. To me it is not yet fully clear, how you can rule out differences also in the shallower crust. What are typical magnetic susceptibilities for the various rock types (shallow and lower crust) from literature data? The same is about the main “gravity sources”, in my opinion seafloor, basement and Moho topographies are major sources for gravity anomalies beside density variations in the individual layers or bodies.

We agree with the reviewer that the lower crust may not necessarily be the primarily source for magnetic anomalies. To investigate how the upper crust contributes to the magnetization, we have extended our analysis by inverting only for the susceptibility of the upper crust, while keeping the susceptibility of the lower crust constant. We added a paragraph in the Results section and extended the Appendix by another figure. The results show that the main structures are maintained, while the amplitude increases to unrealistic values. This underlines that there is not enough space in the upper crust to explain the magnetic anomalies.

In general, we agree with the reviewer that the main density contrasts in the subsurface are located at the seafloor, the Moho boundary and at top basement. In Section 3.3 we added a sentence, stating that the layers shown in the previous section represent the major density contrasts in oceanic lithosphere. In our modelling procedure these density contrasts are acknowledged in the background model. The remaining signal is modelled by lower crustal density variations.

For your modelling and inversion, you only allow changes to values within one standard deviation. Is that meaningful at all?

We believe that the range of the standard deviation is suitable to transfer the estimates of the clustering analysis to the final model. Allowing higher changes in density or susceptibility of the tectonic blocks would improve the residual data fit, but would not be meaningful in terms of geological plausibility.

Regarding your results, why TNDR 3 and TNDR5 are that different?

We thank the reviewer for pointing out the differences between TNDR 3 and TNDR 5. We noticed that the density of TNDR 3 was not modeled properly. Its value is 2.92 g/cm^3 rather than 2.89 g/cm^3, which is closer to the other lower crustal blocks. Accordingly, we revised the parts in the results, where we describe the densities of the TNDR 3 body. At this point, we want to make clear that the overall interpretation does not change from the density adjustment. Nevertheless, we are aware that there are still significant differences in the volume of TNDR 3 and TNDR 5. In the discussion, we added a paragraph that relates the TNDR volume variable budget of magmatic addition.

In Figures 8 and 9 you show seismic reflections (or migration artefacts?) in the lower crust. They seem to spatially correlated with the positive magnetic anomalies. Did you try to model specific bodies within the lower crust that are different in the reflection characteristics? If these reflections are real and not artefacts, can you rule out that they are not related to later magmatic phases (e.g., hot spot magmatism)?

The seismic reflections in the lower crust are likely not a migration artefact, because the Gaussian Beam Migration is extended to 18 s TWT, such that the entire crust is included. For Section 5 (Figure 8) the reflections partly correlate with the boundaries of the tectonic blocks, which are identified by the clustering. We added a line in the manuscript that explains this connection. We also added a summary of the lower crust reflectivity description from Thomas et al. (2022) in Section 3.2. These steeply dipping reflectors are similar to other seismic examples, which are reasonably common in oceanic crust and are generally thought to be related to magma intruded faults or mylonitized shear zones on the flanks of the magma chamber. They are therefore early features during spreading. We also point out that there are almost no indications of post breakup magmatic activity in the dataset at the distance of ~100 km from the Cameroon Volcanic Line (Section 2).

In my opinion, you are not yet convincing in the discussion about the metamorphic processes. You should discuss it in a better way to support your preference for metamorphic processes and why it cannot be related to serpentinization or later magmatic activity. Maybe, a schematic sketch could also help to illustrate your interpretations.

We thank the reviewer for raising this point. We reorganized the discussion and added a section, where we compare the implications of the crustal structure along the fracture zones of the study area with previous studies, suggested by the reviewer. In this section, we also discuss that serpentinization is unlikely to explain the modeled lower crustal properties and the role of magmatic addition during the transition of transform faults to fracture zones.

Furthermore, we extended the discussion by adding a schematic figure on the evolution of lower crustal magnetization. Based on this scheme, we revised section 5.3 (previously section 5.2) and more thoroughly discuss how metamorphic facies and magnetization are related to distance to ridge axis and transform fault. We are confident that the revised discussion helps to convince the reviewer and readers that enhanced tectonic and thermal activity during the formation of transform faults involve stronger metamorphic processes than previously thought.

How do oceanic transform faults compare to other strike slip faults? Can you identify flower structures?

We have not identified flower structures in our study.

Are there any heat flow data (studies), supporting your interpretations and conclusions?

We checked the Global Heatflow Data Base if heat flow data exists for the study area. Only two heat flow data points are available, located outside of the polygon defining the study area (see Figure). These heat flow data points are of course modern values while the processes described in the manuscript are Albian in age.

Heat flow points east of Sao Tome; screenshot taken from the Global Heat Flow Database at 26 June (https://ihfc-iugg.org/viewer/).

There are still many sentences that could be formulated more clearly. Sometimes, strange terms like "proxy" are used (e.g., line 211). The figures have overall a very good quality, but font sizes have to be enlarged. Abbreviations should be explained in the figure captions. Some figures should be enlarged (e.g., fig. 7).

We have adjusted the font sizes in Figure 6a, highlighting the labels of the clusters. We have also ensured that the new Figure 11 is readable. Otherwise, we think that the font size is appropriate. We have enlarged Figure 7 and carefully checked that all abbreviations are explained in the figure captions. At this point, we want to point out that the figures shown in the document will be uploaded separately with a higher resolution. That may help to better comprehend labels of e.g., coordinates.

How does your study differ and compare to classical (e.g., Lin et al. 1990/Nature or Prince & Forsyth 1988/JGR) or more recent studies? Did you also try to calculate derivatives of the potential field data to better localize the source of variations of density and magnetic susceptibility? Did you try to calculate Bouguer anomalies from gravity data or pseudo-gravity from the magnetic data?

As mentioned in a previous comment, we added a paragraph in the first part of the discussion, where we relate our study to classical studies mentioned by the reviewer. We thank the reviewer for making us aware of these references.

We did not calculate derivates of the potential field data and other Fourier-based transition, because we believe that the availability of a high-resolution seismic data set is sufficient to interpret depth and lateral extension of the sources seen in the shipborne potential field data.

A final technical comment, maybe more towards the journal than to the authors: I find the font size of the main text too small. It is very difficult to read.

We agree that Font Size 10 is rather small. However, we followed the guidelines of the journal when submitting the manuscript.

I hope, you will find my comments and questions constructive, and that they will help to improve your manuscript.

Indeed, your comments were very helpful and we are confident that based on your review the quality of the manuscript has been improved. We hope that it will have impact on the scientific community!

With best regards, Wolfram Geissler