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Second Revised Manuscript Submission

February 1, 2023

Dear Ulrike Werban, dear Editors,

on behalf of all co-authors I re-submit the second revised manuscript entitled *"Formation and geophysical character of transitional crust at the passive continental margin around Walvis Ridge, Namibia"* for publication in Solid Earth.

We have received additional comments from one referee, which we have incorporated in the revised manuscript. We hope that the additional modifications to the manuscript clarify any remaining ambiguities. The comments and our answers are appended to this letter.

Again, please don't hesitate to contact us, if you have any further questions.

Kind regards,

Gesa Franz

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Answer to Reviewer #3:

Thank you, for taking the time to engage in a second review and for your additional comments. We have worked on our manuscript again to include your suggestions and clarify ambiguities.

The Reviewer has mentioned the difficulty to reply to our answers, because their original comments were missing. Therefore, we have now included all of their comments. To facilitate the identification of new answer, we marked all our new responses in green.

Line numbers in this response refer to the tracked revised manuscript, where manuscript alterations are easy to identify.

Point 1 (Original comment, first review):

I may not understand the methods that are used. However a question that might arise for a reader when considering the model was how error was handled. The model that forms the basis of the manuscript examines the electrical resistivity and density at each model cell. But the propagated error calculation that indicates what variability may be caused by uncertainties in the input data to the properties of each model cell is not presented. For a reader that may not be familiar with this type of model, this may be something that is handled within the model generation but for a general readership it might help to describe how such errors are handled so that the reader can have confidence in the conclusions that are being made. For example, in figure 3 the XY plot with individuals symbols would might benefit from x and y error bars in order to assess the distinctness of the clusters. This would therefore permit the reader to assess how different the clusters may be from one another and how robust this differentiation might be.

Author response (First review)

Handling of errors of the clustering have been discussed in the previous answer to referee #2. As I understand, this issue concerns the errors of the models themselves. The electrical resistivity and density model errors are difficult to derive due to the ambiguity of geophysical inversion. Therefore, it's not a common practice to specify model errors. Usually model anomalies are tested for their necessity, which we performed in the previous publication (Franz et al., 2021). Here, the analysis of models is focused on comparing differences in the models' parameters along the passive margin. The clustering algorithm is used to identify zones of common parameter relationships and distinguishing from zones with different parameter relationships. This zoning is then linked to geological processes to enhance passive margin interpretation.

Additional reviewer comment from second review:

The authors have addressed the specifics of the clustering mechanism. However, as the authors have correctly interpreted, my query relates to the points on Figure 3. Each point represents an X-Y coordinate that the authors refer to as 'data' in response to the prior comments on error handling of the clustering. Based on the comments above, these points are not data but model outcomes/parameters. The issue I am raising is that of error propagation – one that the authors must address before this work can be published. Each model result has an inherent uncertainty, yet the clustering analysis assumed these to be discrete points with no associated errors (e.g., the points on Figure 3). Accordingly, the statistical treatment of clustering of these points is invalid as it has not propagated the error from the original model. Simply put, how can any value be placed in the clustering when the dimension of potential errors of each point is unknown? Given this is a novel technique, and the probability that this manuscript will be used as a citation for the repetition of this technique elsewhere, it is thus necessary that this issue is fully addressed now lest this issue become a point of contention going forward. I understand that the errors may be complex to handle but perhaps engaging with a statistical specialist may help.

Our model comprises more than 600,000 model parameters and a single forward calculation can take on the order of an hour. This makes both probabilistic MCMC based approaches as well as traditional SVD based resolution and covariance estimation methods prohibitive. This is reflected in the current literature where the uncertainties of 3D resistivity models is assessed through sensitivity tests as no formal estimate of uncertainty can be given (e.g. Munch & Grayver, 2023; Comeau et al., 2022; Murphy et al., 2023; among many others). We evaluated the reliability of our inversion models by investigating data fit, and various model anomaly sensitivity tests. These tests and synthetic inversions have been described in the previous publication (Franz et al., 2021). We added a description of the two most relevant model uncertainties (l. 244 ff.). These are a conductive model artifact at the northern edge of Walvis Ridge, and the smooth, shallow, conductive anomalies which complicate the differentiation between sediments and upper crustal anomalies. The vertical conductive artifact along the Florianopolis fracture zone is discussed in the manuscript in l. 404 ff. and we refrain from its interpretation as described in l. 630 ff.. The second issue concerning the difficulty to differentiate sediment from crustal anomalies is addressed by integrating seismic constraints to separate the sediment from the crustal domain for clustering and interpretations (l. 253 ff.).

While it is important to have some understanding of the variability of the model parameters as discussed in our manuscript, the purpose of the cluster analysis is not to derive statistical properties of the clusters. Instead we use the analysis as approximation of geological units with different properties. Any reader familiar with the geological interpretation of geophysical results will understand that the plotted domains will have some uncertainty even if a formal estimate of this uncertainty cannot be given. We also added references to articles where the authors have performed similar clustering analyses (called geology differentiation) based on models resulting from geophysical inversion (l. 236 f.).

Point 3: Original Comment:

A major issue in this manuscript is the discussion as it relates to the mantle. The manuscript asserts that the difference in resistivity of the mantle relates primarily to differences in depletion associated with a mantle plume. Specifically, the increased magma generation associated with the plume reduced the iron and hydrogen content of the residual mantle, thus increasing the resistivity. This hypothesis relies on the assumption that i) magma generation south of the Walvis Ridge is from melting of an upper mantle without the significant influence of a plume. This concept is alluded to earlier in the manuscript on line 79 where it is suggested that the continental flood basalts in this region are 'mainly of upper mantle composition instead of a deep plume' and also later on line 447/8 (see comments in the line by line). ii) Magma generation at the Walvis Ridge area is the result of plume melt. These assumptions may be problematic:

A) The origin of continental flood basalts in this region is not universally considered to be in the shallow upper mantle (i.e., lithospheric mantle) as suggested in the manuscript. While some authors argue for this source as correctly pointed out by the citation used, others present counter arguments. Please read and incorporate the following citations:

Thompson, R.N., Gibson, S.A., Dickin, A.P., and Smith, P.M., 2001, Early Cretaceous basalt and picrite dykes of the southern Etendeka region, NW Namibia: windows into the role of the Tristan mantle plume in Paraná–Etendeka magmatism: Journal of Petrology, v. 42, p. 2049–2081.

Ewart, A., Marsh, J.S., Milner, S.C., Duncan, A.R., Kamber, B.S., and Armstrong, R.A., 2004, Petrology and geochemistry of Early Cretaceous bimodal continental flood volcanism of the NW Etendeka, Namibia. Part 1: Introduction, mafic lavas and re-evaluation of mantle source components: Journal of Petrology, v. 45, p. 59–105.

Gibson, S.A., Thompson, R.N., and Day, J.A., 2006, Timescales and mechanisms of plume–lithosphere

interactions: 40Ar/39Ar geochronology and geochemistry of alkaline igneous rocks from the Paraná–Etendeka large igneous province: Earth and Planetary Science Letters, v. 251, p. 1–17.
B) Plume sources are considered to have more water and iron than the depleted upper mantle – please research works by Dixon and also Herzberg. While melting of a plume source may lead to depletion, it would require all the material to have been melted. There are further questions on this model as noted below.
C) The depth over which the model is sensitive is ~300km, and at least 100km is being interpreted in the manuscript - as presented per the manuscript text and figures. This extends below the thinned lithospheric mantle along this continental margin and is within the convecting upper mantle. This would suggest that melt depleted mantle material has remained within the convecting upper mantle over an extended interval. The manuscript does not present a mantle flow field argument supporting that this is possible. Moreover, the upper 300km of mantle in the region has seen material from the African LLSVP intrude into it (see recent paper by O'Connor Nature Communications in 2020). Melting of such material may not occur until about 120km depth if the mantle potential temperature is 1530C. This would result in a complex mantle with residual and enriched materials. How might hybrid compositions of pyroxenites impact the interpretations of the model?

On the basis of these points, the hypothesis posed in the manuscript is interesting but requires further support and clarification.

Response from authors Part 1
In our manuscript we state the hypothesis, that the crustal structure south of Walvis Ridge and along the ridge differ as a result of the direct plume impact at Walvis Ridge latitudes. We note, that the involvement of the Tristan plume is a topic to debate (l. 99 f.). This comment helped us to realize inconsistencies in our hypothesis. What we actually wanted to state, is a difference in the crust and upper mantle related to the degree of mantle and melt depletion. And to link the higher depletion below Walvis Ridge to the impingement of the Tristan hot spot and the corresponding extraction of volatiles. The residual upper mantle would then be more depleted compared to the hypothesized "rift related" crust south of Walvis Ridge. We have rephrased the parts of the discussion and conclusion to clearly describe this hypothesis (l. 509ff. and 665 ff.).

Reviewer comment on revised manuscript - Part 1
The response has helped clarify the manuscript but the adjusted line in the conclusion retains the binary view of plume/rift. For example line 666 (the line numbers quoted in the response are not correct) states "Our hypothesis is, that these variations in the mantle composition may result from different degrees of mantle depletion, linked to the differentiation between a rift-related southern complex, and a plume-driven Walvis Ridge regime." This perpetuates the idea that the magmatism in the south has no plume influence – in contrast with the data in the papers I cited previously. If the authors wish to state that the presence of the Tristan tail in particular enhanced melting in the region in question, that would be fine but the current binary of plume-rift is inaccurate.

We removed the binary interpretation of a rift- and plume related differentiation. Instead we ascribe the difference between the southern and along Walvis Ridge domain to the increasing volcanic activity at Walvis Ridge, related to the impingement of the Tristan plume (l. 675 ff.).

Response from authors - Part 2
We are not specialists in isotope geochemistry, but have evaluated the proposed papers. We believe that their conclusions do not contradict our statements. We state that the earliest phase of continent break-up is associated with rifting and that that early magmatics are mainly of upper mantle composition (in

l.100 f.). Gibson et al. (2006) also link this earliest stage of the CFB emplacement (~145 Ma) to melts at the mechanical boundary layer (MBL) at ~150 km depth and not a deep plume source. We do not rule out involvement of plume material in the Etendeka CFB in the subsequent stages. In fact we point out the interaction of the Tristan plume and the lithosphere and heterogeneous composition (l. 131 ff.) of intrusive magmatics, which we link to the ascend of magma which forms dykes and eventually the CFB (l. 136, 142).

Reviewer comment on revised manuscript - Part 2

It might be helpful to seek the input from the many isotope geochemists at GEOMAR – they also work in the same region. The conclusions in Gibson do indeed create difficulties if it assumed that magmatic activity south of the Walvis ridge is restricted to a rift and decompression melt of the ambient upper mantle. That manuscript presents a model whereby the alkaline activity is generated by conductive melting of a lithospheric mantle with a proposed 250 degree heat differential from ambient (a plume). Accordingly, the melt is initially caused by the thermal influence of the plume (and why the initial melt is at the MBL). As the lithosphere thins large scale melting of this mantle with elevated temperatures occurs. The authors state in their response that 'do not rule out involvement of plume material in the Etendeka CFB in the subsequent stages' – however this is precisely what is implied by the conclusion sentence in the revised version noted above. This could all be clarified if the authors used less binary terms and making it clearer that there was a more significant plume influence along the Walvis Ridge area. However, more on this issue below.

As noted above, we removed the binary interpretation of a rift- and plume related differentiation.

Response from authors Part 3

Concerning the comment about the model depth and depth of depleted mantle: Thank you for describing this problem of a mismatch of mantle convection and our statements about a different mantle structure south of-, and along Walvis Ridge. We understand that there needs to be clarification, because we haven't clearly stated that interpretations should be confined to the upper/lithospheric mantle only. We added appropriate statements: We point out, that the resolution capabilities of the electrical resistivity model decrease with depth, and the statements therefore become more vague with depth (l. 635 f.). Additionally, we clearly phrased that interpretations of the mantle domain should not extent below the LAB in l. 389 ff. In our discussion of the mantle clusters, we also added the explicit statements, that our interpretations concern the shallow, lithospheric mantle (l. 513 f. and 562 f.).

Reviewer comment on revised manuscript - Part 3

It is good to see this clarified but it isn't clear everywhere. For example, the line in the conclusion states only 'mantle' and not lithospheric mantle. Everywhere 'mantle' is mentioned it must be changed to lithospheric mantle throughout the document. Otherwise, confusion will continue for those only reading the paper quickly. However, this now brings up an important question – what is the nature of the depleted lithospheric mantle that the authors are referring to? Ostensibly, the authors suggest that the depleted lithospheric mantle in the Walvis Ridge domain relates to plume related-melting. However, this lithospheric mantle in this region should be residual continental lithospheric mantle and thus has been depleted by melting associated with a change in the geotherm (see Gibson paper above for the mechanism). If this is the case, then depletion of the lithospheric mantle by melt creation from within it is a widespread process and not limited to the Walvis Ridge area. The manuscript is very unclear on this point and needs to consider what exactly has been depleted and how.

On line 561 of the new manuscript is the following:

“We attribute these high mantle values to the remnant signature of the upwelling plume, where volatile elements are extracted from melts and rise to the surface to form flood basalts, volcanic flows, and the new oceanic crust (Mutter et al., 1988). The depleted material left in the shallow, lithospheric mantle is highly resistive due to the lack of fluid phases and elements like iron and hydrogen (Baba, 2005; Evans et al., 2005; Matsuno et al., 2010; Selway, 2014).”

This line is relevant to the points above as it explains what the authors’ model is. Firstly, there are serious issues with shifting topics in this sentence that make it ambiguous. As written, this sentence implies that volatile element are extract from melts. I suspect the authors mean ‘by melts’. Moreover, the ‘and rise’ should be ‘that rise’. From this model, it would seem that the lithospheric mantle in this region is residual from the plume and not residual continental lithospheric mantle. Please clarify.

We added the term lithospheric to all occurrences of interpretations concerning the mantle and altered the sentence in l. 568 ff. according to the suggestions. Additionally, we expanded the description of the above stated sentence (l. 568 ff.). In our model, the emerging of the Tristan plume leads to large amounts of mantle melts, intra-crustal magmatism, crustal thickening, uplift, and surface volcanism. All of these features result in a significantly high crustal electrical resistivity. The depletion of the underlying lithospheric mantle is a result of this increased magmatic/volcanic activity and also results in high upper lithospheric mantle resistivities. Our analysis of large scale electrical resistivity and density variations is not eligible for further interpretation regarding the mantle composition or origin.

Point 4: Original Comment
An additional area of concern relates to the conductivity measurements in the upper crust north of the Walvis ridge. This region is known to have significant salt deposits. There is no discussion of the impact of even small salt horizons in this region. There is an allusion to this with respect to highly conductive layers, for example associated with mineralization of lavas. However, it wasn’t apparent that any discussion has occurred in relation to these already mapped salt horizons. The authors must address this directly in their models as workers in this region will be familiar with these deposits and it would raise questions that would detract from this important work.

Response from authors
Salt deposits north of Walvis Ridge have been mapped offshore Angola in the Kwanza basin north of ~15°S (e.g. Blaich et al., 2011; Moulin et al., 2010; Strozyk et al., 2017, Torsvik et al., 2009). The salt directly adjacent to the FFZ may have been sheared off to the South American margin during the Albian ridge jump. The latitudes north of 15°S are not included in our model area. Therefore, we do not discuss any inclusion of salt horizons in our model region.

Revised Manuscript Comment
There is salt in the basin directly north of the Walvis Ridge (and this basin is very much not north of 15S). The Namibe basin has been mapped as having 0-70m of “Evaporites – gypsum and anhydrite. Halite in subsurface” by Jerram et al., 2019 (doi:10.1016/j.tecto.2018.07.027)

The authors will need to address the potential for salt in the crustal rocks and the implications on the observations and potential vertical smearing of such highly conductive units. While some authors have interpreted there to be no salt based on the seismic lines, the physically mapped rocks show these interpretations to likely be erroneous. The magnitude of the salt is much reduced in comparison to the north, leading to the potential of it not being detected with seismic methods. However, given the *sensitivity of MT to such deposits, it is important to assess the potential for this material in the sedimentary layers of the model.*

The named reference (Jerram et al., 2019) describes the 0-70 m “Evaporites – gypsum and anhydrite. Halite in subsurface” sequence in the Kwanza and Namibe basins. In their figure 1, the location of the three study regions: Kwanza, Benguela and Namibe basins are mapped. The southern most basin is mapped slightly north of Port of Namibe. This port is located in the city Moçâmedes, which has the coordinates of 15°11’S, 12°07’E. Our northernmost MT stations is located at 18°08’S, 9°56’E.

We do not see any reference for salt layers in our electrical resistivity models, which should be high electrical resistivity, or in the conjoined seismic velocity models by Planert et al. (2016). A density model which we have previously used as reference (Maystrenko et al., 2013) does not mention any salt in the area, either. The seismic profiles in Strozyk et al. (2017) image small amounts of evaporites in the northern Namibe basin (latitude ~15°S), and explicitly none in the southern Namibe basin (latitude ~17.5°S) (their figures 1 and 7). Therefore, we still do not see any need to address the salt basins along the Angolan coast.

Line by line

New MS Line 174: “The different depositional environment and possibly variable chemical composition due to a different melt source, distinguishes them from the initial continental flood basalts (McDermott et al., 2018)”

This line was changed in relation to my comment:
“what evidence exists for chemical heterogeneity. No citation is provided and I'm not aware of one in this locale.”

The author response was “The main factor to distinguish SDR flows from CFB is surely the different prepositional environment. The possible chemical heterogeneity would be reasoned by the different melt source related to a later stage of rifting, compared to the initial CFB signature. We slightly rephrased the sentence to make it clearer, and added a reference, which characterizes SDR’s and describes how they may be built by
different lava types (l. 174 ff.).

McDermott presents no chemical data to distinguish the composition of the SDRs from the CFBs. Indeed McDermott uses inference to suggest the continued influence of a plume in the SDRs in the South Atlantic. Reference to chemical or compositional differences must be deleted unless the authors can provide an appropriate citation supporting this assertion.

Reference to chemical composition removed (l. 175).

Line 181 – “While thickened crust and the features described above (magmatic underplating, periodic magmatic flows, and magmatic dykes) characterize the COT zone south of Walvis Ridge, the crust north of the FFZ is distinctly thinner, with little to no magmatic signature (Aslanian et al., 2009; Blaich et al., 2011; Planert et al., 2017).”

Original comment “there is evidence of volcanic activity to the north, just much less. The transition isn't as abrupt as noted here. For example, the Namibe basin just north the FFZ has thick SDRs in the south and not much salt. Please examine the existing literature describing the marginal basins to the *north of the FFZ."*

Author Response: "The central southern Atlantic section is generally referred to as a magma-poor or non-volcanic passive margin (e.g. Blaich et al., 2011; Contrucci et al., 2004; Mohriak et al., 1990). Of course this does not completely rule out any volcanic activity, which is why we phrased "little to no" magmatic signature. For our models, the strongest reference is the seismic profile corresponding to our marine MT stations presented in Planert et al. (2017). They have interpreted the northern crust as oceanic crust. We follow their interpretation."

Revised Response: This interpretation conflicts with the cited paper by McDermott et al., 2018, who suggest that the flows of the Namibe basin are Type I SDRs. Also see Figure 7 of Strozyk et al., 2017 (10.1016/j.tecto.2016.12.012) who show SDRs in the southern Namibe basin. This is a far more complicated situation than the authors are portraying

We altered the description of the geological setting north of Walvis Ridge to emphasize, that the area is less affected by magmatic/plume overprint (l. 181 f. and l. 200). That does not rule out magmatic signatures in general and we have not excluded such signatures. The main point here is a major difference of the crust south of Walvis Ridge and north of it. From all mentioned references, it is clearly evident that the crustal structure changes drastically over the Florianopolis fracture zone.

References in this response:

Comeau, M. J., Becken, M., Grayver, A. V., Käufl, J. S., & Kuvshinov, A. V.: The geophysical signature of a continental intraplate volcanic system: From surface to mantle source. Earth and Planetary Science Letters, 578, 117307. https://doi.org/10.1016/j.epsl.2021.117307, 2022.

Franz, G., Moorkamp, M., Jegen, M., Berndt, C., and Rabbel, W.: Comparison of Different Coupling Methods for Joint Inversion of Geophysical Data: A Case Study for the Namibian Continental Margin, Journal of Geophysical Research: Solid Earth, 126, 1–28, 10.1029/2021jb022092, 2021.

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Munch, F. D., & Grayver, A.: Multi-scale imaging of 3-D electrical conductivity structure under the contiguous US constrains lateral variations in the upper mantle water content. Earth and Planetary Science Letters, 602, 117939. https://doi.org/10.1016/j.epsl.2022.117939, 2023.

Murphy, B. S., Bedrosian, P. A., & Kelbert, A.: Geoelectric constraints on the Precambrian assembly and architecture of southern Laurentia. In Laurentia: Turning Points in the Evolution of a Continent (pp. 203–220). Geological Society of America. https://doi.org/10.1130/2022.1220(13), 2023.

Planert, L., Behrmann, J., Jokat, W., Fromm, T., Ryberg, T., Weber, M., and Haberland, C.: The wide-angle seismic image of a complex rifted margin, offshore North Namibia: Implications for the tectonics of continental breakup, Tectonophysics, 716, 130–148, 10.1016/j.tecto.2016.06.024, 2017.

Strozyk, F., Back, S., & Kukla, P. A.: Comparison of the rift and post-rift architecture of conjugated salt and salt-free basins offshore Brazil and Angola/Namibia, South Atlantic. Tectonophysics, 716, 204–224. https://doi.org/10.1016/j.tecto.2016.12.012, 2017.