

## ***Interactive comment on “Tie-points for Gondwana reconstructions from a structural interpretation of the Mozambique Basin, East Africa, and the Riiser-Larsen Sea, Antarctica” by Jennifer Klimke et al.***

**Anonymous Referee #1**

Received and published: 11 September 2017

Dear authors,

your manuscript presents interpretations of structural geological features in seismic data from two conjugate segments of extended continental margins that formed during the breakup of Gondwana. It contends that the features interpreted, because they are unusual for extended continental margins, must have been contiguous during breakup and that they can therefore be used as tie points for reconstructing the supercontinent. It goes on to use those tie points for the purposes of such a reconstruction, which it describes as defining a two-stage Gondwana breakup history in which an early NW-SE

C1

directed phase is followed by a later N-S directed phase.

A two stage history like this is not new, as the text makes clear. Instead, what is new here is the identification and interpretation of unusual structural characteristics, and the dating of the change from NW-SE to N-S directed plate divergence to around chron M38. Earlier studies place the change merely at some time before M25 - around 10-11 Myr earlier.

For the manuscript to be taken further, I think it needs to treat its new aspects more strongly and clearly, as follows.

Structural interpretation: Right now, it is unclear how the unusual structural features fit into the proposed breakup history. If I understand the manuscript clearly, these features are interpreted as flower structures, with both transpressional and transtensional segments on the Mozambique margin, and transpressional only on the less well-sampled Riiser-Larsen Sea side. I further understand that motion along these structures should have been occurring until chron M38, after the onset of seafloor spreading in a NW-SE orientation. This set-up requires ~NE-SW oriented relative motions, parallel to the interpreted COTZs but perpendicular to the NW-SE directed phase of movement that is supposed to have ended with the deformation in the tie-point zones. The manuscript needs to make clear how NW-SE and NE-SW directed motions can have happened at these margins the same time, or it needs to present an alternative interpretation more clearly.

Timing of the change from NW-SE to N-S directed plate divergence: Although the extrapolations of M41 and M38 onto lines A and C are shown in figures 2 and 4, you do not describe on what basis the extrapolation was done (i.e. was it by noting the distance of M38 from the COTZ further west, or by prolongation of the mapped M38 from further west?) or how accurate the extrapolation is. Figure 8 shows, at least, that the extrapolation has been made over a long distance and that the seismic profile of line A lies sub-parallel to the isochron. This is likely to have made the task of extrapolation

C2

difficult, and the result rather inaccurate. This figure would be easier to use if it were clearly to map the downlap trace of the MJ reflector and its relationship to the extrapolation of the proposed M38 isochron at the Mozambique margin, and if it accompanied a clear description of the extrapolation method in the text. Without these, it is at best difficult for readers to assess what kind of error to expect in your assessment, and at worst likely to leave them actively sceptical of the viability of your results.

Some of the following more detailed observations feed back into these two main points, others stand alone:

Line 26: "NE-SW" should be 'NW-SE'.

Line 79: What are the details of this 'considerable debate'? It's important to describe the details because right now, one of the main new points of your work is a proposal that would end this debate.

Line 85-88: The sentence is not clear. Clarify whether the recent studies show older isochrons because they have found new reversal anomalies at marginward locations in new data, or because they have reinterpreted anomalies that were previously assigned to isochron M22.

Line 117: It is 'Knowledge of' the crustal nature of Beira High that is required, not the crustal nature itself.

Line 119: Why is knowing the location of the COTZ really so important for making pre-rift reconstructions? Is it the only or best route to making such reconstructions?

SECTION 3: Please make clear to readers which profiles are being presented in a publication for the first time here, and which ones have been published elsewhere already.

Line 168: Consider a different descriptor. It is difficult to see how two zones of deformation, one of which (according to figure 8) is around 200 km long, the other a fraction of this length, can really be considered as tie 'points'. The features as described allow

C3

for several tens of kilometres of uncertainty in any pre-rift reconstruction built by uniting them.

Line 179: Give clear reasons for the strong resemblance referred to here.

Line 182: How does the faulting increase? Is it the number, spacing, throw, length, or some other descriptor of the faults that changes?

FIGURE 8: The figure should map the extents of the compressional and extensional structure interpretations in the deformed basement areas.

Line 223: Remind readers of the start and end times of the JMQZ.

FIGURE 1 and/or FIGURE 8 (cf Line 290): should show the location of the downlap of reflector MJ.

Line 230: Is there an equivalent reflector to MJ in the Riiser-Larsen Sea? If so, how does any date given to it by Leitchenkov et al (2008) compare to that determined from the Mozambique Basin? Are there clear bends in fracture zone traces (for example in the vertical gradient of gravity data from the Mozambique, Riiser-Larsen or West Somalia basins) that can be placed near the M38 isochron and so support your interpretation?

SECTION 4.3: this section should clearly state how transform-related motions along the NE-trending COTZs can be reconciled with NW-SE plate divergence and/or N-S directed transform-related motions along the Davie and Gunnerus ridges.

Line 284: The assertion only applies alongside the assumption of synrift sediment supply.

Line 357: I think that Mueller and Jokat adjusted the timing of the jump to 164 Ma, which would be more consistent with your scenario.

FIGURE 9: How are the reconstructions here built (what Euler rotations were used), or which earlier publication are they based on? Why do the deformation zones shown

C4

differ in length from those in Figure 8? Why do the reconstructions shown an untenably large overlap between India and the Napier Peninsula?

end of reviewer comments.

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2017-80>, 2017.