

Interactive comment on “The Kenya Rift revisited: insights into lithospheric strength through data-driven 3D gravity and thermal modelling” by Judith Sippel et al.

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GENERAL COMMENTS

This is a nice example of an integrated geophysical study and is appropriate for publication after minor revision. The process of constructing a three-dimensional structural, thermal and rheological model for a large area and with limited constraints is, necessarily, subject to considerable uncertainty, but the authors show sound judgement in the methodology they develop and the interpretation they place on the results. The paper is well-written and the procedures are generally well-documented, although some possible improvements are identified below.

C1

SPECIFIC COMMENTS

p5, line 4: 'Paleo-Eocene', is not common usage and should be spelled out. I'm not sure whether the Paleocene part of this is valid in the context of the references cited at the end of the sentence. The volcanism that pre-dates the rifting is dated at 45-39 Ma (middle Eocene) by Ebinger and Scholz (2012) and Morley et al. (1992) suggest the earliest rifting occurred in the Oligocene.

p7, line 2: Maybe identify the source of the gridded velocity model - (the GFZ Data Archive?)

p7, Eq. 1 (also Appendix B): It is justifiable to assume a linear relationship between velocity and density in the crystalline crust and it is correct to cite Birch (1961) in support of this. The chosen relationship provides densities that seem reasonable. However, the source of the constants quoted for the original Birch relationship is unclear as they do not match any of the solutions presented in the 1961 paper.

p8, line 7: The ρ/ρ_0 term in the Ravat (1999) equation for the mantle (Eq. 2) is much lower than the equivalent term in the ~Birch equation for the crust (Eq. 1). Some justification for this should be included (proximity to the solidus with the former?)

p11, lines 10-14 (also p29, lines 3-4; footnote to Table 2): The way the Bouguer anomaly was calculated needs to be identified. If it was by assuming a uniform density of 2670 kg m⁻³ (the standard reduction with EIGEN-6C4) then the assumption that lateral density contrasts above datum can be ignored is not necessarily valid. It may well be that sensitivity analysis indicates that the inaccuracies involved are small compared with the scale of the anomalies under investigation, but that should be explained. Was the computation surface the sea-level datum?

p12: The method used for modifying the density structure of the upper and basal crustal layers and the top of the basal crustal layer should be described. Was this by manual adjustment or an automated procedure? There is inevitably a degree of

C2

non-uniqueness in the way the adjustments are partitioned between the different variables and it might be better to describe the resulting densities as 'guided' rather than 'constrained' by the gravity data. Lateral changes in density are presumably better resolved than the absolute values, as there is a degree of trade-off between the latter and the way the background or reference model is defined (which also needs to be described).

p15, line 23: It would be advisable to present this initial reference to integrated strength in scientific notation as well as logarithmic notation (given that the former is more commonly used in other studies involving this parameter).

p16, lines 30-32: The maximum depth difference between LITHO1.0 and the present model appears to be underestimated, at least on the basis of visual inspection of the figures of Pasyanos et al. (2014). I recommend checking these figures.

p19-20: The model resolution is very coarse (50 x 50 km), raising the concern that this factor influences the details of the residual anomalies discussed on these pages. For example, the spatial relationship between a residual gravity low and the Nyanza Rift is discussed in some detail, but is actually only defined by a handful of model nodes. Not sure how this concern can best be addressed: maybe the authors should look at the more detailed gravity imaging of Mariita and Keller (2007) to see whether it helps with the analysis?

p21, line 14: Thermal modelling is difficult in this region, for the reasons the authors describe. In view of this it might be better to describe the steady-state conductive model they present as an 'appropriate general approximation' rather than a 'suitable approximation'. Was there a reason for using thermal gradients rather than heat flows for comparison with the observations?

p22, line 20: Is 'largely controlled' an overstatement? There is a thermal contribution as well, although its relative impact is difficult to judge.

C3

p23, line 14: The authors have previously judged (p16) that it would be too speculative to implement lateral heterogeneities in mantle composition to assess the related influence on the gravity field. However, reference to the Ashwal and Burke (1989) hypothesis probably justifies at least a qualitative mention of its possible influence on the model, given that depleted mantle has a lower density than undepleted mantle for a given VS (Priestly and McKenzie, 2006).

General note for Section 5: There should be reference to the differences between present-day (modelled) conditions and those that applied at the time rifting was initiated.

References: I haven't checked these in any detail, but did notice that Goetze and Poirier (1978) should be Goetze (1978).

Figure 6: Have the authors considered also showing the calculated gravity field prior to model adjustment in this figure?

Figure 9: Is the sedimentary fill of the Kenya Rift properly represented in this figure? It only appears to be indicated by a slight deviation of the zero depth line.

TECHNICAL CORRECTIONS

p2, lines 31-33: suggest rewording this sentence

p3, line5: delete 'has' at end of line

p3, line 26: guided > guides

p5, line 4: 'earliest extension' repeated

p6, line 25 (and elsewhere): the authors of the model use the term 'LITHO1.0'

p8, line 9: 'the authors have' > 'those authors'?

p9, line 7: delete 'slightly'?

p9, line 30: '(2012) performed for a' > '(2012), which was performed on a'

C4

p11, lines 2-3: suggest rewording this sentence

p13, Eq. 3: should a dot be used rather than an asterix in the heat equation?

p20, line 6: 'size' > 'extent'

p24, lines 3-6: suggest rewording this sentence

p25, line15: 'local mass defecits (positive gravity residuals >+30 mGal' > 'local positive gravity residuals (>+30 mGal'.

p25, line 20: delete 'one step'?

p26, line 5: not sure about 'strikingly'

p26, line 26: suggest deleting 'detailed'

Figure 1: maybe identify that II and III comprise the Mozambique belt

Figure 2 caption: 'and from a newly developed...' > 'with a ...' + add a reference to the source of the global sediment thickness information

Figure 2 caption: 'the topography, respectively bathymetry' > 'topography'?

Figure 4: the densities on the colour bars for (d) and (e) are in Mg/m³ rather than kg/m³

Figure 10: possible to use subscript for 10 in log₁₀?

Table 5 caption: cross reference locations in Figure 10a?

Table C1: does XFe need explanation?

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