Authors' reply to RC2

SPECIFIC COMMENTS

RC2-1: 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.
We have changed "Paleo-Eocene" to "Paleocene-Eocene".

RC2-2: p7, line 2: Maybe identify the source of the gridded velocity model - (the GFZ Data Archive?)
There is no specific "source" to be referred to at this point. One of the authors, James Mechie, has archived the seismic data in the GFZ data archive since the 1990s. The models along the various KRISP profiles from which the gridded velocities are taken, are all published and referenced in Khan et al. (1999) and Khan et al. (1999) is referenced in the present paper.

RC2-3: 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.
These constants have been derived from the measured properties as presented in Birch (1961, 1964).

RC2-4: p8, line 7: The drho/dVp 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?)
Both equations represent empirical relationships between compressional wave velocities and densities of rocks. Birch's law and the empirical constants of it have been derived from general measurements on crustal and mantle rocks, while Ravat et al. developed their equation for the mantle rocks beneath the Kenya Rift. As the mantle rocks, much more so than the crustal rocks, beneath the Kenya Rift are probably much closer to the solidus than normal, drho/dVp can be expected to be significantly smaller than normal in the mantle here. Hence it is justifiable to use a different relationship.

RC2-5: 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?
We agree with the reviewer that we should be clearer about these issues, so we have implemented some modifications in the revised manuscript:

Added to the third paragraph of section "3.4 3D gravity modelling" of the revised manuscript:
"Details on the reduction of the original gravity data to obtain Free-air and Bouguer anomalies (e.g. assuming a constant density of 2670 kg m^{-3} for the Bouguer plate) are presented on the website (http://icgem.gfz-potsdam.de/ICGEM/). To warrant comparability between the Bouguer anomalies and the calculated response of the 3D density model, all masses located above sea-level have been removed from the model (thus referring to the Bouguer plate above the geoid)."

Added to last paragraph of Appendix A:
"Further, although these density heterogeneities existing above sea-level have transferred inaccuracies to the Bouguer reduction of gravity data (which is based on a constant Bouguer plate density of 2670 kg m^{-3}), neglecting the volcanic edifices in our modelling is not an obstacle to uncovering deep crustal density anomalies due to the edifices' limited spatial extents compared to first-order gravity anomalies."

The footnote to Table 2 has been rephrased: FROM "#not considered for calculations of the Bouguer gravity since the volcanic edifices are located above sea level […]" in the original manuscript TO "#not considered for calculations of the gravity response of the 3D density model (see main text)".

RC2-5: 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?
We would like to refer to the first part of our comments to **RC1-9**.

RC2-6: There is inevitably a degree of 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).
We would like to refer to the second part of our comments to **RC1-9**.
The reference model is described in section 3.4 of the revised manuscript.

RC2-7: 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).
Added.

RC2-8: 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.
We have followed this advice and checked Figure 8b of Pasyanos et al. (2014) showing global crustal thickness of the final LITHO1.0 model. Our comparison of the models, however, was based on the downloaded grid of Moho depth published as a "supplement" to Pasyanos et al. (2014). It seems that Figure 8b does not reflect the full resolution of the grid (it does not even show the Kenya Rift anomaly) and so there are major differences with respect to the grid. As an example, while the figure suggests that the Moho in NE Kenya is widely located at depths shallower than 35 km, only one of 11 grid points for this domain show depths of <35 km, while six points even exceed 39 km (up to 45km). For this reason, the figure suggests that the Moho of LITHO1.0 is shallower than the presented model in this area, while the grid points of the global model mostly plot below the Moho of our model.

RC2-9: 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?
The horizontal resolution of the density model has been chosen according to (i) the distribution of available data (most importantly KRISP and the gravity anomalies) and (ii) the dimensions of the type of density domains to be "mapped" (regional basement domains). Increasing the resolution would

have meant over-interpreting data (e.g. velocity anomalies) for wide parts of the study area. Because of the scatter of constraining data, we have decided not to fit gravity anomalies remaining under approximately 250 km (half-wavelength). We find that the modelled residual gravity already brings along much significance as it indicates areas of (probable) mass deficits and excesses. Any further investigations of those would require additional (geophysical) data and the model points to the regions where these data should be acquired.
Concerning the Nyanza Rift, this area of pronounced mass excess in the final density model is defined by 24 model nodes forming a consistent structure. It can well be seen in the observed gravity. However, any earlier investigations of this wider central Kenya rift area have missed covering the area indicated by the residual gravity anomaly. KRISP lines D and G run past this area, either further north or further south, respectively. Also the gravity analysis study of Mariita and Keller (2007) including valuable density profiles miss this part of the study area, probably since these studies integrated the knowledge gained by the KRISP campaigns.

RC2-10: 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'.
We have rephrased this according to the reviewer's suggestion.

RC2-11: Was there a reason for using thermal gradients rather than heat flows for comparison with the observations?
As copied from our reply to RC1-12: The first-order result of calculating the 3D conductive thermal field is a temperature configuration. The first-order measurement in heat-flow assessments is temperature, too (Nyblade et al., 1990; Wheildon et al., 1994). For this reason, we compare model results and observations in terms of temperatures (geothermal gradient differences; Fig. 8c). Comparing modelled and "measured" heat flow would mean to depart from the original modelling and measurement results, since in both cases heat flow is calculated based on Fourier's law, i.e. assuming a certain thermal conductivity as the coefficient to be multiplied with the (modelled or measured) thermal gradient.

RC2-12: p22, line 20: Is 'largely controlled' an overstatement? There is a thermal contribution as well, although its relative impact is difficult to judge.
Rephrased to "strongly controlled".

RC2-13: 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).
Ashwal and Burke (1989) present a model scenario that might explain why volcanism seems to be restricted to the off-cratonic areas of Africa. They relate volcanic activity to the presence of fertile mantle as opposed to cratonic areas showing depleted mantle compositions. We do not discuss this point further for two reasons: (i) For such a discussion, one would need a larger model that includes cratonic and off-cratonic domains in equal measure. Our model only integrates a relatively small portion of the Tanzania Craton – not enough for such a discussion as we think. (ii) We prefer to directly integrate smaller-wavelength mantle heterogeneities as indicated by the P- and S-wave velocity data. Beside their strong temperature-dependence, these variations in mechanical behaviour are also known to be compositionally-driven, even if less strongly (Priestley and McKenzie, 2006).

RC2-14: 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.
To emphasize this point, we have included a sentence in the fourth paragraph of chapter 6.2.2 of the revised manuscript in the following way:
"According to our present-day the 3D model, the plume-related lithospheric thinning would have been taking place beneath a compositionally and rheologically heterogeneous crust (Fig. 7; Table 4) – even though its structural configuration and, above all, its thermal state have certainly not been the same in the past"

RC2-15: References: I haven't checked these in any detail, but did notice that Goetze and Poirier (1978) should be Goetze (1978).
Corrected. See also RC1-33.

RC2-16: Figure 6: Have the authors considered also showing the calculated gravity field prior to model adjustment in this figure?
As this would be only one of a large number of figures that would represent preliminary stages/results in the modelling procedure, we refrain from showing any of them (for the sake of a concise paper). Possible figures would be, e.g. gravity response of a model with a homogeneous crust, depth distribution of the top of the Basal Crustal Layer after (purely mathematical) interpolation/extrapolation, gravity response of a model with a two-layered crust, etc.

RC2-17: 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.
Indeed, the thicknesses of sediments in western Kenya and along this line do not exceed 3000 m, while the topography is locally >3000 m.a.s.l. Thus, the figure correctly represents the model.

TECHNICAL CORRECTIONS

RC2-18: p2, lines 31-33: suggest rewording this sentence
Rephrased to: "However, detailed regional-scale assessments on the interaction between mantle dynamics and the overlying compositionally heterogeneous lithosphere in order to explain rift localisation and the mechanical predisposition of fault propagation have not been attempted yet for the region."

RC2-19: p3, line5: delete 'has' at end of line
Outdated as this part is rephrased as part of the new chapter "2 Geological Setting".

RC2-20: p3, line 26: guided > guides
Done.

RC2-21: p5, line 4: 'earliest extension' repeated
Corrected.

RC2-22: p6, line 25 (and elsewhere): the authors of the model use the term 'LITHO1.0'
Corrected.

RC2-23: p8, line 9: 'the authors have' > 'those authors'?
Corrected.

RC2-24: p9, line 7: delete 'slightly'?
Done.

RC2-25: p9, line 30: '(2012) performed for a' > '(2012), which was performed on a'

Corrected.

RC2-26: p11, lines 2-3: suggest rewording this sentence
Rephrased.

RC2-27: p13, Eq. 3: should a dot be used rather than an asterix in the heat equation?
Corrected.

RC2-28: p20, line 6: ’size’ > ’extent’
Rephrased.

RC2-29: p24, lines 3-6: suggest rewording this sentence
Rephrased.

RC2-30: p25, line15: ’local mass defecits (positive gravity residuals >+30 mGal’ > ’local positive gravity residuals (>+30 mGal’.
Rephrased.

RC2-31: p25, line 20: delete ’one step’?
Deleted.

RC2-32: p26, line 5: not sure about ’strikingly’
Deleted.

RC2-33: p26, line 26: suggest deleting ’detailed’
Deleted.

RC2-34: Figure 1: maybe identify that II and III comprise the Mozambique belt
Done.

RC2-35: Figure 2 caption: ’and from a newly developed...’ > ’with a ...’ + add a reference to the source of the global sediment thickness information
Rephrased and added.

RC2-36: Figure 2 caption: ’the topography, respectively bathymetry’ > ’topography’?
Done.

RC2-37: Figure 4: the densities on the colour bars for (d) and (e) are in Mg/m3 rather than kg/m3
Corrected.

RC2-38: Figure 10: possible to use subscript for 10 in log10?
Done.

RC2-39: Table 5 caption: cross reference locations in Figure 10a?
Done.

RC2-40: Table C1: does XFe need explanation?
Explanation added.