Response to the comments of the 2nd reviewer (Dr. Bart Root).

The original comments are in italic.

The authors present a regional study of the lithosphere underneath Egypte. The density structure is studied by combining gravity data with other geophysical information of the crust and upper mantle. The motivation of the study is to see if there is a relation between the observed seismicity and the density structure of the subsurface. I find this an interesting approach and application for the presented gravity field modelling done in the study. And in my opinion this relation could even be relatively more addressed. The study is performed with a well documented methodology. I believe this paper could become interesting after some improvements:

Thank you for the positive evaluation of our study. We appreciate all the comments and will implement them in the revised manuscript.

My suggestions for improvement:

R1: Transparency of data and methods. I find the data and models used should be more explicitly discussed. This would give a better understanding of the robustness of the presented model. on page 5 line 19-20, data from Stolk et al. (2013) is used. But what data is this in Egypte (location, value, uncertainty.) page 6 line 4 "available seismic models" -> which models? page 7 line 5 "...several regional datasets" -> which ones and how did this affect the model? line 8 "... various data-sets..." similar questions?

Actually, in this section we describe only the „general modelling approach“, which is applicable to any region, without specifying particular data sets. All the data are described in the following section 3.3 “Model of the crust”. This is clarified. Also, we strengthen discussion of the robustness of the used data-sets in the revised manuscript.

page 11 line 9-10: which seismic model is used for the deep Earth and how does it relate to Schaeffer and Lebedev (2013) model? Is it compatible? And by removing the deep Earth, is only the gravity field of the mantle anomalies removed or also the dynamic signal due to the mantle convection?

The Schaeffer and Lebedev (2013) model provides velocities only for the upper mantle. Therefore, for the deeper layers we use the model s40rts of Ritsema et al. (2011). For the upper mantle it well corresponds to the first model, but its resolution is lower. We take into account the dynamic effects of the mantle convection. These issues are clarified.

How is this related to your later isostasy study? page 13 line 6-8 "The technical details... (2015a, Supplementary)", could this be described in a few line, such that the reader does not have to go to this other literature. It will improve the readability of the manuscript.

For the isostasy study, we intend to separate the gravity anomalies, which are chiefly related to the density inhomogeneities in the crust, which are not compensated in both ways: via density heterogeneity of the lithosphere or dynamically from the mantle. Therefore, the residual anomalies not adjusted in the inversion represent a large scale part of the isostatic anomalies, which might be responsible for the stress concentration and seismicity. This is clarified.

Page 19 line 11-12 "many other factors controlling seismicity" -> which are?

Primarily, these are deformations related to plate motions. This is clarified.

R2: Overall, I find the authors could elaborate and discuss more on their findings, because this is the most interesting part of the paper. Some examples: page 12 line 20 "however, they have several principal differences" -> which are, please discuss them.

These differences are chiefly related to different relative amplitudes of the anomalies depending on the depth to the anomalous body responsible for them. This is clarified.

page14 Figure 7 shows the densities in the upper mantle. I miss the discussion between figure 5, where also densities of the upper mantle are shown. Why are there differences and what can they teach use about the subsurface. And maybe to keep the comparison fair, similar depths and wavelength bandwidths should be used, because now it is difficult to compare the quantitative differences. Could the differences tell use about different compositions in the upper mantle?

The densities in Fig. 5 represent the initial model of the mantle, and in Fig. 7 – the final model after the inversion. In the revised manuscript we show in Fig. 7 also the corrections to the initial model, which fully demonstrate the changes after the adjustment. In the revised manuscript we show density variations in Fig. 5 at the same depths as in Fig. 7.

R3: One of the most interesting issues is the relation of the seismicity to the (non-)isostasy. I find this should get more attention in the manuscript, only after page 18 I read about it. One of the conclusions is (page 19 lines 7-13) is that seismicity occurs in zones with high gradients. Would it be better to plot the gravity gradients and find out? Maybe use invariant of gradients, this would remove the reference frame dependencies, or another method? It would back-up this conclusion.

Thank you for this advice. In the revised manuscript we show a zoomed figure, which demonstrates variations of the maximal value of the horizontal gradient of the isostatic anomalies for Sinai and surrounding area together with the seismicity.

Minor comments:

m1 table 1: might be better to use a graph, because than it is better to compare to other literature that uses graphs. Or might be good to use both.

We still prefer to use the table format since it is important to see exact values of the reference densities. An additional graph would be excessive to our opinion.

m2: Figure 2, please add to what degree and order is used in this figure in the caption.

Added.

m3 Figures with longitude and latitude: I miss the labels in many of the maps.

They have been added to the figure captions.

m4 Figure 8: Why was the Moho not inserted in this figure. It would be a good addition to the cross-sections and give the reader a better understanding of the constructed model.

We have added the Moho in the profiles in Fig. 8.

m5: page 5 line 21: how does the uncertainty in the empirical relationships of Christensen and Mooney 1995 affect your results?

These uncertainties might be significant and correspond up to approximately 30 mGal in terms of the gravity field. Therefore, making the inversion we allow for additional corrections of the crustal densities. This is clarified in the revised manuscript.

m6: textual detail: page 13 line 18 and 21 "as mentioned above" was used twice. Also, this is a bit ambiguous, is it about the sentence, section, or whole paper above this line.

Corrected.

m7: page 18 line 23: "long-wavelength fileld" -> field. Nad what is meant with longwavelength, specify with d/o.

The boundary wavelength corresponds to the maximum resolution of the density model (1x1 degree), therefore it is equal to approximately 180 d/o, which is clarified.