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Interactive comment on "Seismic structure beneath the Gulf of Aqaba and adjacent areas based on the tomographic inversion of regional earthquake data" *by* Sami El Khrepy et al.

Anonymous Referee #1

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Summary:

The paper describes a joint inversion of local earthquake travel times for the P- and S-wave velocities and hypocentral locations, and a subsequent interpretation of the results. The paper is generally well-written though a language review of a native English speaker is needed to improve the readability of the text. The paper would also benefit from a reorganization of some of the sections, e.g. the introduction is in my view a mix of a background/motivation for the study with a geologic/tectonic setting. Though the methodology and data selection procedures etc are fairly clearly explained, there are some aspects here that in my view are missing, and this is in fact where I have my main difficulties with the presented work (point 1-7 below). Otherwise the content is within

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Discussion paper

the scope of SE, and contains potentially interesting material. I am not really familiar with the study area, so I am not familiar with the state of knowledge of the area. I hope a different reviewer is as I might easily have missed something. . .

Data selection:

1) Data appears to come from two sources – ENSN (the Egyptian National Seismic Network) and ISC (the International Seismological Centre). The authors claim they combine data from (the two?) different catalogues, but they do not say how (Line 121). This seems like a good idea as the ENSN presumably only has stations on the western side of the Gulf of Aqaba. It appears natural to assume that when common events are found in the catalogues, the readings are combined to construct just one event. However, the authors state that “priority was given to the data of the ENSN” (L124), which leaves me in a state of confusion what they have actually done. I think this should be clarified. And what time period are the data from? Maybe it would also be good to indicate (with e.g. a colour coding) which stations in Fig. 3 belong to which network.

2) The authors mention that data from “approximately 300 seismic stations” (L127) were used, but “only 53 of the stations were located in the study region (Figure 3)”. In Fig. 3 I am not able to understand what the study region is. Is the study region the entire area in Fig. 3? If so, why is a smaller area shown in Fig. 4 (and onwards)?

3) On L125 the authors state that their data “are part of a dataset that covers a much larger area than presented in the resulting maps. This helped us avoid some of the edge effects that occurred when stations and/or events were located close to the limits of the processed area.” I am confused. In what sense is it an advantage? Do they mean that they use station and events outside the study area? How are then the parts of the rays that fall outside the study area accounted for? Fig. 4 appears to show raypaths coming from outside into the (study?) area – which to me suggests this is actually what they do. But how?

4) No travel time vs offset plots are shown. I think this would be helpful, as it appears from the cross-sections in Figs. 8 and 9 that also the upper mantle is modelled. Then I assume that Pn- (and Sn-)phases are used. If so, are those phases also used to locate the events?

Modelling procedure:

5) How many unknown model parameters do the models contain? Table 1 tells that the model cells are 10x10x3 km in dimensions. Though no km-scale is given in any of the figures (and the model is possibly extending outside what is shown in the panels) I estimate that the models are at least 380x250 km horizontally and 69(?) km vertically. This results in nearly 44000 unknown model cells (minimum) for the P- and S-models combined, plus 9000x4 unknown hypocentral parameters. This totals about 80000 unknowns, which is about the same number of data – yielding a (slightly) over determined P-model and an underdetermined S-model. I find this information crucial, and suggest the authors account for it.

6) It appears that the crustal thickness varies significantly within the model (from 20 to 40 km, L228-230). Yet there is no velocity discontinuity in the model at the crust-mantle interface (Table 2). The authors acknowledge (L311) that there is a trade off between crustal velocities and crustal thickness. Though it is tempting to read a crustal thickness from the cross-sections in Figs. 8 and 9, what is the iso-velocity they used to define the crust-mantle boundary? Maybe it would be nice to see some hypothesis tests on allowed variation of crustal thickness vs (lower) crustal velocities?

7) On the same theme: The authors first identified a 1D starting velocity model using “trial” (and error?) – L167). Considering the large variation in crustal thickness in the area, I assume that the event distribution would largely control what 1D model is “the best”. One could also imagine a different procedure acknowledging the large variation in crustal thickness, and assigning a different starting model to the different model regions. Maybe the authors should explore this option – or at least provide argument

against it.

Structure of the paper:

In addition to splitting the “Introduction” into two separate parts (see above), it is a bit confusing to me why the “Data and Algorithm” is not in separate sections. In my view it would improve the paper if they were kept separate. Similarly to the “Results” section which contains information of both the synthetic reconstruction tests and on the results from the real data. I also suggest the authors go over once more what belong in the “Results” and what in “Discussion”, it appears to me that there is some repetition. Also, there are new things appearing in the “Conclusions” that have not appeared before, e.g. the comparison with the model of Eastern Mediterranean (L367). This is not a “conclusion” to me, but a “discussion”.

Other questions/comments:

Line 2: depths of what?

L31-34: unclear sentence – e.g. what do “this” and the double “it” refer to?

L40: One of the largest faults in the world? Reference, maybe?

L49-50 and L72-74: A variable displacement of the fault (Garfunkel, 1981) is not consistent with a model as shown in Fig. 2. Is this an outstanding geologic controversy? If so, I think the authors should bring it out more clearly that they want to try to address it. Or – considering the age of some of the references, is this controversy solved long ago? If so, is it at all relevant to account for it here?

L140 and L146: The tomography algorithm they use account for the sphericity (curvature?) of the Earth, but uses an approximate raytracer (the “bending method”). To me it appears that for a model region this small the latter would generate much larger errors than using a “flat” Earth would... Or am I wrong? Maybe the authors could motivate their choice in this regard.

L163: Many of the parameters in Table 1 is completely incomprehensible. . . For example, what does an “Amplitude damping, P and S” of “0 and 0” mean?

L182: Are the synthetic travel times computed with the same (forward) algorithm as in the inversion? If so, 1) why iterate that it is a “bending method”? And 2) the authors are in fact committing the (very common) “inverse crime”, i.e. doing the same approximation (error) in the forward as the inverse computations, thus the errors in some way cancel. As the “inverse crime” is so common, the authors may be forgiven, but maybe they should (as stated earlier) explore a little bit what limits in model perturbations (in amplitude and wavelength) are OK for the ray bending approximation. . .

L204-205: No, the resolution in the P- and S-models do not appear to me to be similar (see 1 on “Modelling procedure” above).

L235: That the P- and S-wave models are similar may also be an effect of Vp/Vs ratio damping – if such a constraint is applied. And it is not obvious to me as I do not understand all the algorithm parameters applied.

L309-310: No, it is not clear at all to me.

L317: “to similar to” – huh?

Figures:

The figures are generally OK, but a km-scale would be helpful in most of them.

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