

To the Editorial Board of *Solid Earth*

Dear Editor and Reviewers:

5 First of all, we would like to express our sincere gratitude for the huge amount of work that was done by both reviewers. We very much appreciate their careful consideration of our manuscript. The comments were rigorous, but very constructive and friendly. Not only were the recommendations and advice useful for us in the context of the present work, but such comments will also be valuable for our future papers.

10 We have considered very carefully all the comments of the reviewers, and for the vast majority of the comments, we have made corresponding corrections in the manuscript. Among the most important changes, we can single out the following revisions:

- 15
1. We have considerably changed the structure of the main sections in the paper and reformatted the text correspondingly.
 2. We have added a test with synthetic Moho.
 3. We have added Fig. 3 with travel times versus distance.
 4. In Fig. 2, we have added a plot showing the general distribution of all stations of the ENSN.

20 All the changes are described in detail in the attached response letter.

We hope that you will find our paper improved and suitable for further consideration in *Solid Earth*.

Sincerely yours,

25

Sami El Khrepy, on behalf of the coauthors

30 Response letter for the paper by El Khrepy et al. “Seismic structure beneath the Gulf of Aqaba and adjacent areas based on the tomographic inversion of regional earthquake data”

The author’s responses are highlighted with violet and indicated with “REP”

35

Anonymous Referee #1

Summary:

40 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
45 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 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.

50 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.
55 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.

60 REP: We have added more details to the description about the data merging to address this issue (L115–118). In addition, we have added a map showing separate symbols for the ENSN and ISC stations (Fig. 2).

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 (Fig. 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)?

65 REP: In the updated Fig. 2, the left plot is a general map of the study region with all stations used in this study. The right plot limits are exactly the same as in the resulting figures.

70 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?

REP: We have added the following sentence clarifying this issue: “Using the stations and events from a much larger area surrounding the region of interest enabled us to improve the azimuthal coverage and increase the number of picks for some events” (L115–116).

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?

REP: We have added Fig. 3, which plots the travel times versus epicentral distances in the initial catalog, as requested by the reviewer. We have also added some descriptive text related to this figure (L126–127).

Modeling 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 44,000 unknown model cells (minimum) for the P- and S-models combined, plus 9000x4 unknown hypocentral parameters. This totals about 80,000 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.

REP: We have added more details to the description of the grid construction (L144–150). Actually, the nodes were installed only in areas with sufficient amounts of data. In the vertical direction, the grid spacing was usually larger than 3 km depending on the ray coverage. Therefore, the number of nodes was considerably less than that estimated by the reviewer. On the other hand, the results would not be invalid if the number of nodes was larger than the number of data. Indeed, all the velocity parameters are linked with each other through smoothing equations, and they cannot be considered as independent. In our algorithms, we prefer to always use grid spacings much smaller than the resolution capacity to avoid grid dependency of the results. The resolution of the solution is merely controlled by the smoothing coefficients.

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 crustmantle 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?

REP: Thanks for this comment; we have added a new test in Fig. 6 in which we explore the possibility of estimating the Moho depth variations based on the continuous tomography models. We have also added a paragraph describing this test (L206–219).

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.

115 REP: We provided more details on estimating the starting 1D velocity model (L160–170). We do not understand how different ID models could be assigned to different regions? What would happen on the boundaries? How we can compute travel time if a ray path passes through different zones. We suspect that such artificial subdivision of the area would strongly affect the final result.

Structure of the paper:

120 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.

REP: In the new version of the paper, each of these two sections are separated into two separate parts.

125 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.

REP: The sections with Results and Synthetic modeling information have been separated. The issues related to the discussion have been moved to the Discussion section from the original Results section.

130 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”.

REP: This statement has been moved to the Discussion section.

Other questions/comments:

135 Line 2: depths of what?

REP: We have added the phrase “with the bottom depth...” for clarity.

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

140 REP: We have rewritten these sentences as follows: “This sedimentation regime differs from the Gulf of Suez, which is located on the other side of the Sinai Peninsula. Many consider the Gulf of Suez to be a zone of ongoing crustal extension (McClusky et al., 2003; Mahmoud et al., 2005), and it is nearly fully covered by young sediments (Gaullier et al., 1988; Cochran and Martinez, 1988)” (L98–100).

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

REP: This statement has been removed from the text.

150 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?

155 REP: We present a rather simple model without considering the difference in slip rate in different parts of the DST. Otherwise, this would require more sophisticated modeling tools and it could not be accomplished within this tomography study. For the purpose of qualitative interpretations of our tomography results, such a “scissor and paper” model is sufficient to illustrate our hypothesis. We have made some changes to the description of this model (L321–327).

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.

REP: We have added the following sentence: “This is especially important for rays with long offsets displaying regular bias if sphericity is not taken into account (in a flat model, travel times are always slower)” (L138–139).

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?

REP: Some parameters, which were not important, have been removed from this table.

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”?

REP: The ray paths in the synthetic model and those used for tomographic inversion are considerably different. The first reason for this involves the difference between the starting 1D model and “unknown” true 3D velocity model. Second, we start the analysis from the location of sources in the 1D model, which results in considerable shifts of some events. Iterations are necessary to bring back, as much as possible, these events toward their true locations and to build ray paths in the updated 3D models.

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.

REP: Actually, we have never heard about using more realistic forward modeling in such types of problems. If, for example, we perform elastic wave modeling and obtain full waveform seismograms, then picking these data will take an unrealistically long time. Certainly, it does not make sense to do this for our purposes.

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).

REP: We have removed the phrase regarding the statement on the similarity.

L235: That the P- and S-wave models are similar may also be an effect of V_p/V_s 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.

REP: No damping for the V_p/V_s ratio was applied! In this sense, the P- and S-anomalies are independent (only slightly linked through source parameters) (L153–154).

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

REP: This part of the discussion has been removed.

L317: “to similar to” – huh?

REP: This sentence has been rewritten.

Figures:

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

REP: We have created kilometer distance scales for all maps where there were no other kilometer scale indicators (i.e., such as locations of the profiles).

205