

Interactive comment on “Variations of the crustal thickness in Nepal Himalayas based on tomographic inversion of regional earthquake data” by I. Koulakov et al.

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Dear reviewers,

Thank you very much for your careful consideration of our manuscript “Variations of the crustal thickness in Nepal Himalayas based on tomographic inversion of regional earthquake data”. Your comment have helped us to include many changes into the paper that improved considerably the presentation of our results. In particular, we have doubled the number of figures according to your comments and made considerable changes in the text. The detailed answers to your comments are presented below. The

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corrected parts in the manuscript, which is provided as supplement, are highlighted with violet color.

We hope you will find our paper improved and consider it again for possible publication at Solid Earth.

Best regards,

Ivan Koulakov, on behalf of all coauthors.

Answers to the reviewers’ comments (authors’ replies start with “Rep”). Reviewer 1 Review ‘Variations of the crustal thickness in Nepal Himalayas based on tomographic inversion of regional earthquake data’ by Koulakov et al.

This article provides a small-scale depth variation in the Moho beneath Nepal with travel time tomography and discusses spatial correlations among the estimated Moho depth, gravity anomaly, and magnetic anomaly. Although I am not familiar with detailed collision processes in this region, results and interpretations in this paper appear to be reasonable. I have some comments, particularly on the methodology.

P 2871, line 15: I do not understand what ‘four different grids with different basic orientations’ mean. Please explain it more. A figure showing grid nodes might be helpful.

REP1: We have added Figure 2 with the distributions of two grids according to the ray density. Additional information on the grid construction is added to the text (lines 117-121)

P2871, lines 21–26: I suggest that the authors show a figure of the reference velocity model because the 1D velocity model plays a crucial role in the determination of the Moho depth.

REP 2: According to this comment, we have created the new figure 3, which presents the starting 1D model and average velocities in the resulting model. The corresponding changed in the text are made in Lines 132-133 and 141-145.

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P2872, line 17: Were random noises with average deviation of 0.5 sec added to both P and S waves? Did the author use different values for P and S waves, depending on picking accuracy? Please clarify this point.

REP 3: In the previous version, there was a mistake, and the actual deviation of noise was 0.1 s (corrected in line 157). It was same for the P and S data. However, we do not consider the S model in our interpretation; therefore, the value of noise for the S data does not have crucial importance for our story.

There is no information on the reduction of arrival time residuals during the inversion. Please comment on it.

REP 3: We have added the requested information on the residual reduction during the inversion of the observed data (lines 176-180).

The authors delineated the Moho by tracing the lower limit of the low-velocity anomaly in many cross sections. Although I know that there is a large ambiguity as discussed in the manuscript, this strategy seems to be somewhat subjective. In lines A1–B1 and A2–B2 in Figure 3, a dashed line is alternative interpretation, but the authors consider that it is less plausible because the crust becomes to be too thick. If so, what is the origin of a low-velocity anomaly below the Moho? Are there any tectonic regions? Is it just due to low resolution of velocity images? I would recommend adding a brief argument, if possible.

REP 4: This zone of thicker transition zone is especially well seen in absolute velocity plots presented in the new version of the paper. In lines 204-208, we mention that it might be due to underthrusting of one continental crust underneath another one. At the same time, we state that the robustness of this feature is not very high and that we should be prudent with the interpretation.

In introduction, the author reviewed conventional studies that estimated the Moho depth. I understand that some results provided too smoothed Moho depth and some

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are too localized. However, it would be worthwhile comparing the present results with the Moho depth derived from receiver function analysis or active seismic survey, to know the differences and agreement among the models. Because receiver function analysis or active seismic survey would provide more reliable Moho depth because of their advantages in determining velocity discontinuities, the comparison with the pre-existing results could support the validity of the present inversion results.

REP5: In the new version of the paper, in Figures 6 and 7, in addition to our results, we present Moho depth determinations from (Hetényi et al., 2006) and (Schulte-Pelkum et al., 2005) in corresponding sections. We have added the discussion on comparison with these models in lines 209-217.

Reviewer 2

In this manuscript, the authors present a new P-wave and S-wave velocity model for the Himalayas region and interpreted the bottom of the shallower negative P-wave velocity perturbation as the crust-mantle boundary. The seismic velocity model is computed using a regional seismic tomography code. About 800 stations, 10000 P-and 5000 Sphases are used, with a fair coverage of the central portion of the investigated volume. The results are compared with maps of gravity and magnetic anomalies. Thinned crust is interpreted as weaker crustal section, possibly related to the presence of thick sedimentary layers, where less intensive shortening occurred in the past. On the other hand, thicker crust is interpreted as stronger crustal section due to past magmatic intrusions.

Overall, the manuscript is clear and well-organized and the figures are acceptable, with some minor changes. However, the results presented are not convincing, mainly due to the sparsity of the data-set (as recognized by the authors) and due to the assumptions adopted to “measure” the Moho depth in the region. Also, as mentioned by the authors, a number of “measures” of Moho depth have been previously published in the last decade. The authors shall explicitly illustrate how their results improve our knowledge

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of the Moho depth in the area with respect to old studies. In my opinion, a major revision is needed before considering the manuscript for publication on Solid Earth. In the following, I list my minor and major issues.

Major points.

(A) Absolute P-wave velocity value. In general, a seismic velocity models should be presented using the absolute seismic wave velocity instead of the velocity perturbation with respect to a 1D initial model. This fact can improve the readability of the model for a wider audience. In this particular case, where the Moho depth is mapped across the P-wave velocity model, absolute P-wave velocity values are strictly required. The crust-mantle boundary should be presented as the transition to mantle P-wave velocity (e.g. 8.1 km/s).

REP6: We have added the plot with absolute velocities both for the observed data and synthetic results in Figures 5 and 7. We agree that thanks to this comment, the representation of our results has become much clearer. In all figures, the Moho interface corresponds to the yellow level with the velocity of 7.4. We have modified the discussion of absolute velocity models in lines 190-193 and in other places.

(B) Too subjective choice of the Moho depth. Mapping the Moho depth using a subjective velocity perturbation can be misleading. I strongly recommend to picking a velocity for the mantle and track it through the model. To avoid subjective choice of the P-wave velocity, different P-wave velocity values can be selected and different Moho maps can be presented in the Supplementary Materials.

REP7: Because of using absolute velocities together with anomalies, the Moho picking is less subjective now. Also, we have added the comparison with other results that gives another check for the reliability of our results.

(C) Comparison with previous Moho depth estimates. As correctly reported by the authors, a number of previous studies mapped the crust-mantle boundary in the area.

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I expected to find some figures (e.g. the three profiles) where the authors presented their results compared to previous measures of Moho depth. For example, plotting punctual measures from Receiver Function analysis for stations deployed along the three profiles. This part of the analysis is completely missing from the manuscript. The author must add a section in the manuscript illustrating how their results improve the previous knowledge on the Moho depth in the area.

REP8: According to this comments, in our resulting profiles, we have added the information on the Moho depth from other studies which appeared to be most representative in our opinion. The comparison with these models is discussed in Lines 209-217.

(D) Resolution. The data coverage is fair in the central portion of the study area, but is quite poor in the rest of the volume. The authors report the results of a number of synthetic test to assess the resolution of the data (and the technique) in the area. The well-resolved area seems to be highlighted in Figure 4, where map of the Moho depth is plotted only for the central portion of the study area (but this is not clearly indicated in the figure caption). The authors should illustrate in the text the criteria used to evaluate the resolution in the model and indicate the position of the well-resolved volumes in all the figures (i.e. in the cross-sections in Figures 2 and 3).

REP 9: According to this comment, we have defined the areas of fair resolution in sections 2 to 5. Outside these areas, the results are shaded. The corresponding changes in the text are included in lines 186-188.

Minor points.

Figure 2. The figure can be improved showing the synthetic model used to compute the synthetic travel time. This plot can help the reader to better evaluate the resolution of the data in the region. As discussed in point (D), the boundaries of the well-resolved area should be plotted in this figure.

REP10: The shapes of the synthetic anomalies are indicated in the vertical sections.

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In our opinion, showing the resolved area here would be not optimal. We should give a reader a possibility to identify by himself the robustly resolved areas. However, if the reviewer insists, we can very easily add the indications of the resolved areas.

Figure 3. As for Figure 2, well-resolved area should be clearly highlighted in this figure. Also, using absolute value can be useful to the interpretation of the dashed line along profile A2-B2.

REP11: Well resolved areas are highlighted in the new versions of figures 6 and 7. Plots with absolute velocities are presented. Some discussion on Section 2 is added in Lines 204-208.

Figure 4. Well-resolved area must be clearly highlighted also in the maps of magnetic and gravity anomalies. Traces of the profiles reported in Figures 2 and 3 should be plotted (at least, in Figure 4A).

REP12: The locations of the profiles are included. For the gravity and magnetic map, we do not have the information on the resolution; thus, it cannot be presented here.

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/6/C1403/2014/sed-6-C1403-2014-supplement.pdf>

Interactive comment on Solid Earth Discuss., 6, 2867, 2014.