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Solid Earth
Discussions

Interactive comment on "Nature of orogenesis and volcanism in the Caucasus region based on results of regional tomography" *by* I. Koulakov et al.

Anonymous Referee #2

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The study addresses a topic of wide interest and it is of potentially great significance for better understanding Caucasian orogeny. The authors interpret a selected part of seismic tomography results obtained in another study and reported by the first author in another paper (Koulakov 2011, JGR).

The specific problems tackled by the study and reported in the manucript are nicely listed at the end of the introduction and regard the evolution of the „mantle part of the continental lithosphere during continent-continent-collision" and „the nature of the active Cenozoic volcanism" in the region. I appreciate chapter 2 as a good summary introduction to the „geodynamics and volcanism of Caucasus and surrounding areas".

The chapter concludes with the sentence: „In this paper we will provide additional arguments for" lithosphere delamination causing the Late Miocene-Quaternary volcanism „based on recent tomographic images of the upper mantle". The problems I see with the manuscript result from the fact that, unfortunately, the seismic tomography images are obviously not of the requested resolution to address these questions. The authors are actually documenting this in Figures 6 and 7 with a checkerboard and with synthetic data testing.

In the region of interest, at best, the seismic data is able to resolve structure with dimensions of 150km*150km*150km and at least 3% average velocity variation. This should not come as surprise since similar resolution has been documented by various earlier studies for the ISC data set (e.g. Bijward & Spakman; Piromallo & Morelli). Such resolution, however, is insufficient to address questions of volcanism and even to address questions regarding the mantle lithosphere in the continent-continent collision zone. Note that the tomographic cross sections presented in Figure 5 exhibit minimum low velocity anomalies (-3%) in the depth range of the crustal root directly underlain (coinciding in lateral extent, hinting at a possible vertical leakage problem) by a large volume of relatively low velocity (-1.5%). Taking into account the significance level for the tomographic images –estimated by the reviewer as approximately +/- 1% based on synthetic test Figure 7- the two continental lithospheres involved in the collision are only in patches resolved outside the 400km to 600km wide collision zone. In conclusion, the presented tomographic images could be interpreted (1) as documenting no mantle lithosphere at all beneath the Caucasus region and a mountain crustal root resting directly atop the asthenosphere upwelling. Alternatively (2), I would prefer to see additional local seismic data to be included in the tomography study Koulakov (2011) to significantly improve resolution and reliability of tomographic results before addressing the important questions raised in this manuscript introduction and chapter 2.

In addition, I would like to direct the authors attention to the following specific points:

Abstract, line 11. „.. supported by strong deformations indicating weak properties ..."

you mean weak lithosphere?, what deformations exactly do you mean – if uplift, what about isostasy?

Abstract, line 22. „dominantly felsic composition of the crust which is favorable for the upward heat transport „ Why should the felsic composition promote the vertical conductive heat transport? Do you refer to extra heat source due to radioactivity?

Introduction, p.13-24. You seem to favour the model of somehow misteriously thickening the continental crust in the orogenic root zone without involving mantle lithosphere in a plate tectonic sense. Please add references to back this up and please add references where you refer to „actively discussed in the scientific community".

Chapter 3, Figure 2 is not refered to in text (should be after Fig 1 and before Fig. 3). Also missing is important information on the data set: how many events, how many rays, how many unknowns/cells, what is your estimated observation uncertainty.

Page 646, Line 25

& page 647, lines 1-10. It is nice to caution the reader about „interpretation of absolute values of anomalies" and to refer to the strong noise in ISC data. However, solution overdamping probably is the least of your problems and it can well be seen by the reader thanks to synthetic data tests. As documented by several previous glocal tomography studies using this data set (i.e. Spakman and coworkers in several studies), the real limitation of ISC data for such „local studies" stems from strong inconsistencies and numerous blunders in the data.

Page 647, line 29. „the shapes of all features are correctly reconstructed." In this general term this statement is certainly wrong. Take a look at your figure 7 and reconsider.

Page 648, line 17. „Arabian and European lithospheric plates is about 250km" , this may not be deduced from these tomographic images and it should be discussed in the light of other information reported by previous studies.

Page 657, Figure 3. Mark volcanoes with clearly visible symbols. Color scale should

correlate with resolution power, with this noise in the data and with your cell size (you show lateral velocity variations of less than 100km extent) you will not be able to resolve reliably 0.3km/s velocity variations (see below remark on significance level).

Page 658, Figure 4. Same as for Figure 3 and 5, adjust significance level in color scale.

Page 659, Figure 5. Please calculate the significance level and adjust your color scale accordingly!

Page 660, Figur 6. Please use this figure in text to explain your resolution. From this image I suggest with P-velocity the smallest structure resolved must be of 150km size in all directions and possibly 2% or 3% anomaly. For S-anomalies I guess this would be 400km size and 4%. If you disagree, please add reasoning as more tests are needed to clearly define it.

Page 661, Figure 7. In your synthetic model you show small separated volumes of light blue of about 1% amplitude and size of about 100km. These features are –not surprisingly- totally distorted or wiped clear in the recovered images since they are below the limits of resolution. In the central part of your model between 100km and 300km depth you introduce an extended approximately 2% negative velocity anomaly. In the recovered P-velocity image I notice an amplitude of 3% or more in the center part of this anomaly, an obvious artefact pointing toward a strong local (mostly vertical) leakage problem and suggesting a significant UNDER-damping (50% increase in amplitude!) of your solution. Note that in same figure and in same location the recovered S-velocity field remains OVER-damped. Finally, in either P- and S- velocity anomalies there is an obvious leakage between distance 1500km/depth200km and distance1800km/depth700km in the order of 1% anomaly! This points toward your significance level that for most parts in your cross section could be as high as +/-1.2% for 200km size anomalies, making it impossible in my opinion to interprete the tomographic images to the details that you desire in your study.

Interactive comment on Solid Earth Discuss., 4, 641, 2012.