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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 #1

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The paper presents interpretation of a Caucasian part of the recently published seismic tomography model of Asia. Key features of the model are low P- and S-velocity anomalies in the crust and upper mantle below the highest mountains and volcanic centers. Based on this model authors suggest that mantle lithosphere and mafic lower crust beneath mountain ranges was delaminated, what in turn caused magmatic activity and heating of the crust. I'll attempt answering two major questions related to this paper. (1) How robust is seismological model? (2) How robust and innovative is its geodynamic interpretation?

(1) I think that the major features of the model that are high velocities related to Ara-

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bian and Eurasian plates and low velocities below highest mountains are quite robust. These features are visible in both P- and S- models and synthetic tests show that large-scale features can be indeed resolved by the model. Correlation of lowest velocities and volcanoes also looks quite convincing. To be on the safe side I would still check (by synthetic tests) if thick crust below the high mountains and very low velocities in the crust below young volcanoes could be smeared down to the mantle, thus significantly contributing to the observed low-velocity anomalies.

I'm not entirely convinced that smaller features of the tomographic model (like high P-wave velocity body at 800 km of Section 2 at depth 50-200 km, Fig. 5, that is interpreted as active delamination pattern in Fig. 8) are robust. This particular body is not so clear evident in the S-wave model. Moreover it is located right below the gap in the seismic network. To my opinion that feature may be or may be not real. Anyway, because of its possible importance for geodynamic interpretation this particular region deserves higher resolution seismic study.

(2) I think that most reasonable interpretation of the robust features of the presented tomographic model and volcanic activity in the region is indeed delamination of the mantle lithosphere and mafic lower crust followed by the heating of the remaining felsic crust. So I support interpretation suggested by the authors. However, exactly the same process (tectonic shortening–crustal thickening–eclogitization of the lower crust that triggers delamination of the mantle lithosphere) was previously suggested based on petrological arguments (Kay and Kay, Tectonophysics 1993) and modeled (Sobolev and Babeyko, Geology 2005) for Central Andes. Rate of this process was analyzed by Jull and Kelemen (JGR 2001). I think that authors should clearly indicate that their interpretation is identical to that previously suggested for another orogen (Andes) and confirmed by geodynamic modeling. Actually this statement will even add credibility to their interpretation.

In general I think that this is an interesting paper, suggesting a reasonable geodynamic interpretation of the interesting seismological model for Caucasian orogen. However,

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authors should not interpret details of their model without thorough analyses of their robustness and they should clearly indicate that the geodynamic interpretation they suggest is not new but has been previously discussed in details in relation to central Andes.

Some minor issues: English should be improved. Show paper to the English native speaker. Page 648, line 17. Lithospheric thickness of 250 km should be typical for Archean cratonic lithosphere but not for the Arabian plate. Such thickness in your model is likely due to the vertical smearing. Page 650, line 3. I guess authors actually mean paper by Babeyko et al (EPSL 2002) discussing possible convection in the thick continental crust.

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

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4, C306–C308, 2012

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