Our response is given in courier

Interactive comment on “The lithosphere-asthenosphere boundary observed with USArray receiver functions” by P. Kumar et al.

Anonymous Referee #1

Received and published: 13 February 2012

This manuscript presents receiver function imaging of the LAB beneath North America using the new large USArray data set. The authors find an LAB at shallow depths _100km that persists across the continent. This is in agreement with previous receiver function results from single stations but does not agree with tomographic estimates that find a deeper root (_200km). The authors briefly suggest that silicate melt from increased water (Mierdl et al., 2007) is the cause of the observation.
It difficult to assess the scientific quality of the paper since no methods are presented, and for a description of the technique the reader is referred to a manuscript that is currently submitted to Tectonophysics (Kind et al., 2012).
Similarly, it is difficult to determine the scientific significance since the authors state that some earlier version of these results was published in SRL (Kumar et al., 2012), however, this manuscript is not available online yet.

These papers are published electronically in the meantime; the full references are given. We have, however, extended the technical discussion.

Finding an LAB across the entire US at shallow depth is an important result that might have big implications. The figures are nice, and the result is exciting. However, the authors go into little to no detail relating these results to tectonic features, other observables, etc. Similarly, the interpretation and discussion is very brief, not comprehensive.

We have also extended the tectonic discussion and added a number of references.

Interactive comment on “The lithosphere-asthenosphere boundary observed with USArray receiver functions” by P. Kumar et al.

Anonymous Referee #2

Received and published: 27 March 2012

The manuscript provides a straightforward but brief assessment of exciting new S receiver function results from USArray. The main point is that a negative velocity gradient is present at 50-150 km depth beneath the entire area imaged. This feature is broadly consistent with expectations of lithospheric thickness in the tectonically active western U.S., but in stable regions of the U.S. the negative gradient appears to be within the thermal lithosphere as inferred from longer wavelength surface wave imaging and heat

flow. Recent views on this topic are presented in a concise and limited manner in the discussion.
Generally I think the text is well written and interesting, though more information regarding construction of the RF images is warranted. The abundance of figures and new results will attract many readers, but I get the impression the results presented overlap almost entirely with those in Kumar et al. 2012 SRL. Below are some suggestions and comments for the authors to consider.

1. The text should include more information on how the RF images are constructed. Are the 'string-like' features in the images individual RFs? Are the RFs gathered and stacked by common mid-points? If so, what is the length-scale for stacking? The quantitative procedure for making the images should be made clear to the reader or at least provide a reference if the identical procedure has been previously used. More on the S receiver function analysis also seems appropriate (e.g., data selection criteria).

We have extended the technical discussion accordingly. The individual 'string-like' features are indeed individual seismic records (receiver functions) with time transformed into space along the ray path and amplitudes color coded. This is a typical kind of migration procedure. Each profile collects traces within a width of 100km.

2. How was the 'LAB' interface picked – by hand or some automated algorithm? If it was automated, what is the procedure? Most sections in the figures are straightforward but in some cases it is more ambiguous.

The LAB lines through the seismic sections are drawn by hand and meant for marking the phases we identified as LAB.

3. The text is very brief considering the large volume of data processing and exciting new results. It seems that little effort has been made to consider the results in the context of North America tectonics.

We have done this now.

4. A relevant reference for discussion of the MLD in North America is Chu et al. 2012 G-cubed, who use refracted phases from small intra-plate earthquakes to study lithospheric structure beneath the central U.S. with higher frequencies.

This is a very interesting paper. Thanks for pointing it out to us. We have incorporated it into the discussion. They see the 8° discontinuity and the LAB near 200km depth. Their LAB is, however, a very weak signal (in P waves) compared to the dominating signal we see near 100km in S receiver functions.

5. In addition to Currie and Beaumont 2011, Schmandt and Humphreys 2010 EPSL also suggested that portions of the flat-subducting Farallon slab may have basally accreted to North America, specifically mentioning the Colorado Plateau and Wyoming. There is a strong correlation between a high velocity anomaly in body-wave tomography and surface wave tomography (Pollitz and Snoke 2010 GJI; Obrebski et al. 2011 GJI) and greater 'LAB' depth from S RFs in this region.

We have added these papers to the reference list and compared their results with ours.

6. The deep (_120-140 km) negative gradient beneath the TX/OK border is noteworthy as it is a prominent anomaly and positioned below a Proterozoic aulacogen. Any ideas why a failed _1 Ga rift would result in deepening of the LAB or MLD would be interesting. For a reference on the large-scale geologic provinces the authors may find Whitmeyer and Karlstrom 2007 Geosphere and references therein to be helpful.

We have mentioned this reference and the Oklahoma aulacogen, but we could not give any explanation. We also refer to the

Rhine graben rift, wherein contrast to the Oklahoma aulacogen, updoming of the LAB is observed.

7. A minor point: The authors generally have a preference for citing classic studies (which is good), however for receiver function analysis there were many studies in decades before 2000 as I'm sure the authors are aware. Perhaps it would be appropriate to cite some of the earlier work or a review paper since the current text gives the impression that the method is only about 10 years old.

We have made an appropriate reference to our recent review paper in Tectonophysics where historical aspects of the receiver function technique are given.