General comments

This paper presents a comprehensive - and impressive - analysis of S receiver function data over the continental US, with dense sampling owing to the availability of data from USArray. The paper is well written and the presented cross-sections show some interesting features, adding to the existing evidence for complexity of lithospheric structure contrasted across the boundary between the western tectonically active part of the US and the central craton. It also offers additional support for the presence of a thin low velocity zone atop the 400 km discontinuity.

My primary comments concern the intriguing interpretation given by the authors (Fig. 10) and described in detail in section 4 of the paper. I am somewhat skeptical about the lines drawn on figures 8 and 9, and meant to describe dipping structures. It is quite clear that the authors document the rather abrupt termination of the ~100 km depth western US LAB somewhere between the Sevier Thrust Belt and the Rocky Mountain Front, and that the structure changes very significantly going into the craton. However, I find the dipping broken lines drawn in figures 8 and 9 reflecting more a point of view of the authors rather than a hard fact. So, it would be important to also show these cross-sections without the interpretation lines. Indeed, what *I see*, particularly at latitudes below 43^{o} (sections g->n in figure 8) but also looking across the N-S sections in Figure 9e-g, is that east of about 110^{o}W longitude, there is a thick zone of almost continuous "negative" reflectors (blue colors) in the depth range 100 to 200 km, with no obvious justification for drawing tilted lines across them. This zone is particularly reflective and strong in sections 9b and 9e, which are singled out by the authors for their cartoon shown in Fig. 10. However, it is also present in other sections, although less clearly, indicating significant lateral variability of the reflectors.
In fact, this thick zone of negative reflectors corresponds well with what was already documented, a long time ago, from long range profile experiments, as a zone of strong scattering (e.g. Thybo and Perchuc, 200x. It also agrees with the zone of somewhat reduced velocity found in some tomographic models under all cratons (e.g. Lekic and Romanowicz, 2011). In other words, it fits well with the idea of a layered cratonic lithosphere, with an MLD marking the top of a complex zone of varying thickness and, on average, somewhat reduced shear velocities, and an LAB within the craton that is around 200 km depth, and with less topography, as found from waveform anisotropic tomography (Yuan and Romanowicz, 2010; Yuan et al., 2011).
I can just as well draw a vertical line at about -112-113^{o}W in panels i,j,k,l of Figure 8 marking the sharp transition between the thick cratonic lithosphere to the East and the thinner western US one to the west , consistent with tomography (e.g figure 11ab in Yuan et al., 2011), and, for example, the observation of SP phases horizontally deflected along the Rocky Mountain Front (e.g. Zheng and Romanowicz, 2012).

This layering and its abrupt change across the RMF is also clearly visible in azimuthal anisotropy (e.g. Yuan and Romanowicz, 2010). Here the authors mention that they find no evidence for azimuthal anisotropy from receiver functions. There is actually evidence for azimuthal anisotropy layering in the craton from many regional studies of SKS splitting (e.g. Deschamps et al., 2008) or surface wave analysis (e.g. Derbyshire et al., 2013) and most recently, from an analysis combining surface wave dispersion, SKS splitting and receiver functions (Leiva et al., AGU abstract, 2014; Bodin et al., *in preparation*), yielding results that are in good agreement with anisotropic waveform tomography beneath a sample of stations on the craton. The fact that the authors do not find evidence for anisotropy in their receiver functions may be due to 1) a faint signal in the receiver functions coupled with insufficient azimuthal coverage at most stations that have not been recording for long enough times; 2) the use of a reference 1D model for the migration to depth.

How the MLD defined by the long wavelength azimuthal anisotropy study (Yuan and Romanowicz, 2010) relates in detail to the observed onset of scattering in the receiver functions remains to be determined.

Further comments:
On page 5, line 23-27, the authors briefly mention that one can analyze converted phases without deconvolution, but they make an intriguing statement: "However, the advantages of deconvolution in improving the signal to noise ratio prevail". It would be nice if the authors would expand and provide supportive argumentation to this statement. Indeed, one can also stack records without deconvolution, leading to superior signal to noise ratio. One can discuss the advantages and disadvantages of deconvolution versus direct convolution using synthetic seismograms (e.g. Bodin et al., 2014) at length, but brushing aside the latter approach as inferior without justification is not very satisfying.

In figure 1, the authors show a map of earthquake coverage for their study. I think this is a bit misleading: most likely no one station (except for some long term permanent stations) sees such a good azimuthal coverage. It would be more informative to show several panels, corresponding each to a particular time period of the deployment of USArray.

Technical Details
On page 8, line 3 - typo: "discontinuity IS given"
In figure 9 caption: please explain the abbreviations: ST, RMF, MCRG.

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