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Discussions

Interactive comment on "Triplicated P-wave measurements for waveform tomography of the mantle transition zone" *by* S. C. Stähler et al.

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> Interactive comment on "Triplicated P-wave measurements for waveform tomography of the mantle transition zone" by S. C. Stähler et al.
Referee 2
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Traditional travel-time tomography uses teleseismic arrivals whose ray paths are mainly vertical and has relatively poor vertical resolution, especially for structures deeper than 300 km. In this paper, the authors try to measure the finite-frequency travel-times of triplicated P waves and demonstrate that regional rays, which travel horizontally and have better vertical resolution, can be used for future investigation of transition-zone and upper-mantle structures. This is an interesting paper.

My main concerns are:

1) In teleseismic case, the inaccurate source location, depth, and origin time can be removed by a baseline shift to the travel-time residuals. In regional triplicated waves, the source parameters could cause a big problem.

Thank you for your detailed and constructive review.

We agree that source parameter estimation is a major challenge, which is why we put significant effort and sophistication into estimating epicenter location, source time function, and moment tensor (based on the method by Sigloch Nolet 2006, as described in 2.3.) This constitutes part of the novelty of this paper: after carefully correcting for source signature in the waveforms (across all frequency bands), we can be confident that the remainder is of structural origin.

We have revised the section about source inversion (2.3.) to explain this part of the processing more in detail, and we have added a flow chart for illustration (new figure 4). More specifically:

> 2) In order to get source time function of each earthquake, the authors use teleseismic records and deconvolve with Green's functions. In this approach, the magnitude, source duration, and attenuation have large trade-offs. A 1D attenuation model of PREM may not be good enough.

There is no tradeoff between magnitude and source duration, as we do not parametrically estimate source duration, but rather deconvolve the entire source time function. Mis-estimates in magnitude do not change the shape of the STF estimate, but rather will be reflected in the station amplitude corrections (or more precisely, their mean, since all seismograms are equally affected). Station amplitude corrections are additional free parameters, which are required in any case to compensate for miscalibrated station gains (but ultimately we also harness these measurements of amplitude anomalies to invert for attenuation in cases where the data are of high quality). For details see Sigloch & Nolet 2006.

NEW:
We briefly describe the procedure here and in Fig. 4, for details see Sigloch (2006): We remove obviously problematic stations and align all waveforms to the arrival of the P phase VanDecar (1990). We then choose a reasonable candidate depth range to survey, 1 – 50 km for shallow events and NEIC depth ± 30 *km for deep events. Then we execute the following scheme for each candidate depth: First a joint deconvolution of the synthetic seismograms, calculated with the NEIC moment tensor* M_0 *from the measured seismograms is done, resulting in an STF estimate* $\dot{m}(\tau)$*. Source orientation is assumed to be constant during the rupture, so that* $\dot{m}(\tau)$ *is identical for all components of the moment tensor. Second, with this STF, an update for the moment tensor* δM *is calculated and the amplitudes of all stations are corrected individually. The new moment tensor* $M_0 + \delta M$ *and the amplitude corrections are used to derive a new STF estimation and this is repeated, until the RMS misfit between synthetics and broadband seismograms has converged.*

After all depths have been treated, we manually choose the depth, at which the RMS misfit is minimal and the STF does not contain any significant negative parts, which would be unphysical. The STF and moment tensor results for this "most likely" depth are retained for kernel calculation and tomography.

For the regional wavepath geometries relevant here, a wrong reference model for attenuation would affect all synthetic seismograms in more or less the same way, so that this waveform distortion would be absorbed in the shape of the source time function. The same is true for lateral anomalies in attenuation that occur near the source. (We always attempt to invert for a single STF that fits all global teleseismic data, but in about one out of three earthquakes, we need to allows for two or more regional STF solutions, e.g., when the European station cluster cannot be fit to the same STF as the North American cluster.) Our view of a "source time function" is pragmatic: we want it to absorb all signal that is common to all seismograms, even when that signal does not

derive from the source rupture sensu strictu. The remaining signal can then safely be interpreted to be 3-D structural. We have modified Section 2.3 to explain this.

NEW:
We always attempt to invert for a single STF that fits all global teleseismic data, but in about one out of three earthquakes, we need to allow for two or more regional STF solutions, e.g., when the European station cluster cannot be fit to the same STF as the North American cluster. This may be due to structure close to the source, like a subducting slab or be an effect of source directivity. Our view of a "source time function" is pragmatic: we want it to absorb all signal that is common to all seismograms, even when that signal does not derive from the source rupture sensu strictu. The remaining signal can then interpreted as an imprint of the structure along the wave-path.

> For shallow earthquakes, they may also trade-off with earthquake depths.

This is correct, and in fact we discuss this in Section 2.2. "Waveform modelling". We invert STFs at different trial depths (typically in increments of 2 km), and then choose the best-fitting STF/depth pair. We changed 2.3 to state this explicitly, see above.

> 3) The authors use IASP91 as the starting model, which may not be a good model for western United States.

That may well be correct, although our goal here is to describe the method, which is independent of any specific reference model (as long as it is spherically symmetric). The actual reference model could be chosen differently for every regional tomography study. We have updated Section 4.1 and 5 (discussion).

NEW:
Figure 4 shows a comparison between matched filters and real waveforms for the seismograph station ontop Mount St. Helens crater, which is by no means an ideal station. Still three out of five bands have > 0.8 *and could contribute to an tomography. These seismograms were calculated using the IASP91 velocity model. For an actual tomogra-*

phy of Northern America it may be beneficial to calculate matched filters and kernels in a regional 1D model; for a recent regional reference model and a comparison between previous results see **?**.

> 4) For a triplicated waveform, there are 35 arrivals depending on distance. I don't know whether the authors use only the first arrival or not. A common problem of triplication is the trade-off between the shallow and deep turning waves. This trade-off can be easily seen from Fig. 5.

This question is answered in section 5 "Discussion". We use a time window that contains all phases. From a traditional ray-theoretical perspective, this may seem unintuitive, but an onset pick (isolation of one infinitely broadband phase) is a very different (idealized) measurement from a cross correlation delay, which can be observed for any time window, containing any number of phases. Our method also allows to realistically model sensitivity kernels for arbitrarily complex phase sequences. How well the minimization of this misfit will work towards updating the structural model remains to be seen when we actually invert the data for a mantle model.

> 5) If there are sharp structures (e.g. slabs) near the earthquake or the some stations, the triplication pattern will be more complicated.

That is correct. The drawback of our linearized method is that we cannot use waveforms from such stations, so we must exclude this kind of non-fitting data. However, this is no different from other modeling approaches: any near-source structure may complicate fitting to the point where data become unusable.

> My other comments are:
> 1) Lines 5 and 9, page3, the authors used km as unit for distance. Later, they use epicentral distance in degrees.

Thanks for pointing this out, we unified the units throughout the paper.

> 2) Line 9, page3, the authors state "The reason is that these regional waves

generate much more complex seismograms than teleseismic ones, for the very reason that they have extensively interacted with the MTZ discontinuities." This is not true.

We do not understand the reviewer's objection, since the obvious difference between teleseismic and regional body-waves is that the latter show additional triplications, caused by the discontinuities. Perhaps what was meant is that complexities are not the reason that the waves have not been used, but we do mean to say that this has been the case. We added a second sentence to make this point explicit:

The reason is that these regional waves generate more complex signals than teleseismic ones as they have interacted more extensively with the MTZ discontinuities. Such observations do not lend themselves to abstraction into isolated pulses and the associated, idealized modeling by ray theory.

> 3) Line 15, page 7, the authors state that the depth phase "arriving 7 s after the first" In the caption of Fig. 1, they indicate the depth of the earthquake at 6 km. These two statements do not agree.

Correct, thank you. This was also pointed out by Reviewer 1, who identified the second phase as a reflection from the sea surface. We changed Section 2.3 correspondingly.

> 4) Line 24, page 8, PREM is not correctly cited.

Thanks, the missing n in Dziewonski was a bug in the Latex stylesheet and was corrected manually.

> 5) Page 33, source time function should be all positive. However, their source time function has negative ruptures.

The true, physical STF is indeed expected to be non-negative. However, we do not force the STF inversion to yield a non-negative solution, we only encourage it to do so. Given our pragmatic view of the STF as a bin for all common signal (discussed above), non-negative wiggles may be real (e.g., reverberations from a slab near the source), or they may be artificial (waveform data is always noisy and sparse). We choose not to enforce non-negativity because we actually find it to be a very informative indicator of events that are too complex to be decently modeled and thus must be rejected. We updated Section 2.3 to explain this (see your remark 2 above).

> 6) Fig. 4 is not referenced and discussed in the paper. Even the cross-correlation coefficients are large; the waveform fits are not good in a waveform modeler' opinion. The reason may be that the station VALT is above a volcano, which is not a good case.

Thanks for pointing this out, we moved Figure 4 to Section 4. It is now Fig. 8 and explicit discussion is added to Section 4.1:

Subjective assessment of a "good" fit is of course partly in the eye of the beholder (and certainly the fits are not as simple as for teleseismic waves, the commonly used waveforms). However, our point is in terms of methodology: cross-correlation coefficient is objectively quantifiable, and we know from experience with teleseimic P-waves that coefficients exceeding 0.80 can be used very successfully for finite-frequency tomography. Should it turn out that 0.80 is still too low, then we would just choose a stricter threshold. The message of the figure (added to the new version in Section 4.1) is that in this example, three out of five bands can be fit successfully, yielding three independent data for tomography (despite and independent of possible complexities of the Mount St. Helens location).

NEW:
Figure 8 shows a comparison between matched filters and real waveforms for the seismograph station at Mount St. Helens crater, which is by no means an ideal station. Still three out of five bands have > 0.8 *and could contribute to tomography. These seismograms were calculated using IASP91 velocity model. For an actual tomography of Northern America it may be beneficial to calculate matched filters and kernels in a regional 1D model; for a recent model and a comparison between previous results see*

Chu (2012)..

> 7) Line 26-27, page 23, this statement is not true.

We are not sure which of our statements raised this objection. The sentence in question is probably: *"Analysis of triplicated waveforms has so far been mostly limited to deep events (Tajima et al., 2009)"*, we weakened it:

NEW:
Analysis of triplicated waveforms has been mostly applied to deep events (Tajima, 1995, Tajima et al., 2009).

There is a significant amount of literature modeling seismic sections of triplicated phases (latest:
Chu, R., Schmandt, B., Helmberger, D. V. (2012). Upper mantle P velocity structure beneath the Midwestern United States derived from triplicated waveforms. Geochemistry Geophysics Geosystems, 13(1), 1–21. doi:10.1029/2011GC003818 – added in the revised version).
But these papers try to fit the move-out of the separate phases on a large number of seismograms, rather than single waveforms. The sentence in question explicitly treats triplicated waveforms, not arrival times of the triplicated phases.

If the reviewer is aware of studies that are similar to ours in this regard, we would like to know about them.

Interactive comment on Solid Earth Discuss., 4, 783, 2012.