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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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**> This is an important paper, that for the first time uses the very high station density of modern broadband seismic networks to dissect the seismogram around the arrival time of the upper mantle triplicated P waves in great detail. As the authors correctly observe, the transition zone is of crucial importance in geodynamics: its thickness is an indicator of temperature differences, the jump in elastic parameters and density at the 660 boundary may one day tell us whether mass exchange between upper and lower mantle in unhindered by the phase transition or not - in the latter case leaving only cold slabs and hot plumes able to cross the barrier.**

**> The second half of the paper deals with the interpretation of cross-correlation**

**delays for bandpassed arrivals. Using finite-frequency theory the authors demonstrated the complicated sensitivity of these data. The paper stops short of an actual tomographic inversion, but this is understandable. These are new types of data; their usefulness in (tomographic) interpretations is not guaranteed. Their observaibility warrants a discussion of its own. > The authors have done an admirable job with the graphics, and this paper should be required reading for any student of the triplicated phases if only because of figures 1,2,3,5 and 9 which are highy instructive.**

**> Unfortunately the discussion is marred by a few inaccuracies that need correction. Here is a list with a few suggestions to fix them and improve the paper:**

**> section 2.1: Figures 1 and 2 are exchanged when discussed in the text.**

We are sorry that the version for the reviewers contained this avoidable error. This had already been fixed in the Solid Earth Discussion online version of the paper.

**> page 788, line 3: while the narrow spatial sensitivity indeed depends on the wave being a delta function, the validity of ray theory is broader. If the wave is not dispersed by diffraction effects the eqs (1) and (2) are still valid, even if the sensitivity is spread out.**

We agree, and we corrected this statement. Derivation of the eikonal equation, and with it eqs. 12, does not use the frequency content of the signal, only the fact that the medium is non-dispersive (although one can often read the opposite).

OLD:
*Strictly speaking, the approximation only holds for the case of a spectrally white minimum phase signal ($\delta$-pulse).*

NEW:
*Strictly speaking, the approximation only holds for the case of a non-dispersive medium. Even when the medium is not intrinsically dispersive, velocity heterogeneities*

*may introduce a frequency-dependent group velocity, which is the motivation for finite-frequency tomography.*

**> page 788, line 13: if dc/dr>0 there is a low velocity zone and there will be a discontinuity in the T-Delta curve, contrary to what is stated here. Also, it is the derivative of c/r that should be looked at, not c itself.**

The sentence in l.13 might have been confusing due to the fact that we defined r as depth, not as radius, which is inconsistent with eqs. 1 2. and has been changed.

NEW:
*The literature does not provide any hard constraints for a triplication to occur. However, it is clear that a discontinuity in c(r) or dc/dr will be sufficient.*

**> page 789, line 16: 7s delay is large for a depth of 6 km. Inspection shows that this earthquake is under the ocean, so in this case the ghost phase is a water phase (pP if you wish) and there is no sP. The low P wave speed in water explains the large delay.**

Thanks very much for the close inspection. This makes a lot more sense. We adopted this interpretation into the sentence and renamed the phase to pwP.

OLD:
*The secondary pulse, arriving 7 s after the first, can therefore be recognized as a surface reflection (pP or sP)*.

NEW:
*The secondary pulse, arriving 7 s after the first one, can therefore be recognized as a reflection from the ocean surface near the source (pwP)*.

**> page 790, line 12: the authors repeat here a widely held misunderstanding that the wave is only influenced to a distance equal that of a (half) wavelength. It is the width of the Fresnel zone that is the limiting distance, not the 'diffraction limit' used for lensing systems (which would be useful if we had so many sources and**

**stations that we could actually use them as a perfect lense, but that is not the case).**

What the reviewer describes is exactly what we were trying to say. The term "path difference" pointed towards the definition of the Fresnel zone. We did not intend to state that the width of the sensitivity zone is lambda/2. We tried to make the sentence clearer:

OLD: *Additionally, waves of finite wavelength are influenced by scatterers off of the direct path, if the path difference is less than l/2.*

NEW: *Additionally, waves of finite wavelength are influenced by scatterers off of the direct path, if the detour to reach the scatterer is less than l/2; this is the definition of the Fresnel zone.*

**> page 793, line 19: If one accepts the relationship (5), which is linear, one has already used the Born approximation, and all methods to calculate K are therefore "Born" kernels. However, K depends on the wavefield in the background model (see eq 11), and this wavefield may be approximated by paraxial ray theory, as Dahlen et al. proposed, or by more sophisticated numerical methods that include reverberations or (in 3D) multiple scattering. This again is a misunderstanding about finite-frequency that seems to be widespread.**

Again, our sentences were opaque. As the reviewer says, the Born approximation is already in eq. 5, which goes back to Marquering (1999), to our knowledge. Dahlen (2000) extended it to body waves and especially proposed the paraxial ray algorithm to calculate these first order kernels quickly. We clarified that.

OLD:
*Obviously, this kernel depends on the velocity structure of the earth, since it is influenced by the path the wave takes. The computationally demanding part is the calculation of the wave fields. Dahlen (2000) proposed to calculate these kernels under the*

*Born approximation (single scattering assumption) in a layered velocity model. This is justified by the observation that the magnitude of lateral mantle heterogeneities $\Delta V_{\mathrm{P}}$ is small, typically on the order of a few percent, so that multiple scattering can be neglected.*

NEW:
*Obviously, this kernel has to be calculated using a reference velocity model. Equation 5 and the construction of the kernel $K_k$ contain the assumption that the traveltime anomaly $\delta T_k$ originates exclusively from single scattering off of anomalies $\Delta V_P/V_P$, which quantify the difference between the reference model and true earth structure. This so called Born approximation is justified by the observation that the magnitude of lateral mantle heterogeneities $\Delta V_P/V_P$ is small, typically on the order of a few percent, so that multiple scattering can be neglected. Kernel $K_k$ is the first Fréchet derivative of $\delta T_k$ towards $\Delta V_p(r_x)$ (Marquering, 1999).*

**> page 794, line 13: contrary to what is stated here, interactions can very well be modeled with ray theory. In the case of triplicated waves there are in my view two reasons to resort to AxiSEM rather than ray theory: ray theory cannot handle the caustic singularities because it will predict infinite amplitudes, and the computation of K in (5) becomes less efficient if one is obliged to sum every arrival.**

We agree only partly: in addition to the two drawbacks mentioned (caustics and separate simulations for each phase), another problem occurs around discontinuities: Dahlen's approach calculates the phase delay Omega for off-path scatterers from the Hessians M' and M" (eqs. 91, 106 and 109 in Dahlen (2000)) on the ray path itself. This works strictly speaking only for a continuous velocity model. Close to first order discontinuities, this approach fails, since the Hessian of a path in 660+dx km depth does not reflect the fact that the velocity drops by 8% at 660 km.

See the imprint of the 660 on the 35° and 40° distance kernels (Fig. 5). Dahlen's paraxial code would calculate almost symmetric kernels here.

OLD:

*Triplicated waveforms, with their interactions of refracted and reflected phases around the discontinuity, are not adequately modeled by the ray theoretical formalism. Hence we need to calculate the kernels from the full wavefield instead.*

NEW:

*Triplicated waveforms, with their interactions of refracted and reflected phases around the discontinuity, are not adequately modeled by the ray theoretical formalism. The caustics at turning points A-F (Fig.4) would lead to infinite amplitudes at the corresponding distances. Moreover, the ray-centred approach by Dahlen (2000) works strictly speaking only in a continuous velocity model. Hence we need to calculate the kernels from the full wavefield instead.*

**> page 796 line 14: I assume the number of 10 9 is obtained by multiplying the number of waveforms by the number of cpu-hours/wavefield. But should one not simply use the sum of sources and stations rather than the number of waveforms (or wavepaths)?**

If one uses composite event-kernels (approach of Tromp or Fichtner), one ideally needs only two simulations per event and iteration. But with any iterative or multiscale approach, the whole process has to be repeated in each of the 10 2 iterations.

**> page 803, line 4: Houser et al. (GJI 2008) show a depression of the 660 under the N American craton. It would be nice to compare directly with her topography.**

Thanks for pointing out this observation. Houser finds the 660 at 668-675 km depth in our region (northern Mexico), which fits our observation very well.

ADDED (Section 4.3):

*Indeed, studies from SS precursors Houser (2008) showed a 660 depressed to 668-671 km in Northern Mexico.*

**> page 806, line 1: Dziewonski is misspelled**

The Copernicus Latex style sheet seems to suppress the diacritic. We corrected this.

**> general comment: the authors use no less than three different methods to compute synthetics (WKBJ, reflectivity, AxiSEM). I can guess there are good reasons for this, but it would be nice to tell these to the reader.**

ADDED (Section 5):

*We note that the mixed use of forward modelling codes (WKBJ, reflectivity method) has historical reasons and reflects an unfinished (though functional) stage of development. Ultimately all steps will be carried out with the most complete method, Axisem. For inversion of source time functions from teleseismic data, WKBJ has been an efficient tool since Sigloch2006. It is completely adequate for teleseismic waves, and this STF inversion step is treated as independent from the modelling of triplicated waveforms. Reflectivity is used for efficient forward modelling of the triplicated broadband seismograms (WKBJ cannot easily compute triplicated phases, and Axisem is not yet set up to do true broadband computations efficiently for the number of computations needed). In order to compute sensitivity kernels, the full wavefield is needed, and we obtain it from full numerical forward modelling using Axisem (but not yet routinely up to frequencies of 1 Hz).*

**> The legend of figure 6 could be much clearer if 'regional' is replaced by 'the first arriving'.**

Done.

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