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Solid Earth 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.

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.

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.

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.

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).

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" ker-

nels. 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.

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.

page 796 line 14: I assume the number of 10ˆ9 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)?

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.

page 806, line 1: Dziewonski is misspelled

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.

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

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