Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-44-RC2, 2019

Interactive comment on "Anisotropic P-wave traveltime tomography implementing Thomsen's weak approximation in TOMO3D" *by* Adrià Meléndez et al.

Anonymous Referee #2

Received and published: 22 April 2019

#####General comments

The authors present a modification to TOMO3D seismic tomographic inversion code which allows to include anisotropy parameters (assuming VTI symmetry) in the inversion. Such code is a valuable and interesting contribution, as most of the existing tomographic codes assume an isotropic medium. The manuscript presents necessary evaluation of reliability of the code. Formally, the structure of the paper is correct, it contains all necessary parts and is generally well written, although some parts (details below) are hard to follow. The main goal of the manuscript is to evaluate three characteristics of the code/method: the accuracy of the forward code, sensitivity of the method

Printer-friendly version

Discussion paper

for anomalies of model parameters and resolving ability of the inversion. In my opinion, the papers gives satisfactory answer for first two questions are basically satisfactory, while the third question is studied only partially (details below). An interesting outcome of the inversion tests is the discussion about poor sensitivity of the traveltime data on the δ parameter, even in case of 'ideal' measurements geometry (spherical geometry and uniform angular spacing of sources/receivers locations).

#####Specific comments

From the parametrization used (Thomsen parameters) it implicitly follows that the VTI symmetry of the medium is assumed (also, the inversion does not solve for orientation of the symmetry axis, so I guess it is assumed to be vertical) but it is not clearly stated in the text, and statements like 'anisotropic tomography' (also in the title!) suggest at the first glance that the code can be used for media with more general and more complicated symmetries. If I'm right about this, I would recommend to state it explicitly that the code can model VTI media only. This would make clear that it cannot be used e.g. for modeling of azimuthal anisotropy (which is usually due to HTI or TTI medium).

L120: The feasibility of determination of the δ parameter is widely discussed in the text, but the definition of the parameter itself is not given. I think it should be added to the text.

L128-132: I understand the relationship between anisotropy parameters and Vnmo, but the comment and conclusion about the Vnmo in L128-132 is unclear for me.

-Accuracy and sensitivity tests:

Fig. 3: what is the meaning of the 'normalized difference'? And why 'relative difference' for v is constant for all angles, while 'normalized difference' is sinusoidal?

L198: The authors wrote "..for each of the four parameters. . .25% anomaly was added, while . . . the rest of parameters was homogeneous" – This seems to be contradictory (or it is explained in a misleading way). Parameters v, vT (I use vT instead of the symbol used in the manuscript for v in perpendicular direction) and ε are interrelated by Eq. 2, so changing vT and keeping v constant must change also ε – in result, two parameters are changed. I know that this is not a problem for inversion, as parameters vT and ε are used alternatively, not in the same time, but such description introduces confusion in interpretation of the sensitivity test results. Also, even if anomalies of every parameter are the same (25%), we can hardly compare cases of ε and vT anomalies: - 25% ε anomaly results in value of 0.2 (compared to background 0.16), while: - for vT, the same anomaly (25%) with respect to background 2.32 results in vT=2.9. This, connected with v in the anomaly being 2.0 (undisturbed, so equal to background), results in ε=0.45 –an anomaly few times larger than in previous case. (This is probably the reason why in Fig. 3 sensitivity for ε reaches $\sim$0.7%, while sensitivity of vT is few times larger and reaches 4.5%)

-Inversion tests:

The inversion tests are done properly, assuming several variants and various strategies, and results (especially concerning the poor recovery of δ parameter) are interesting, but their main drawback is assumption of near-ideal experimental conditions: - the measurements geometry (spherical geometry and uniform angular spacing of sources/receivers locations, which results in unrealistically uniform ray coverage). - no noise assumed. Such conditions are almost never possible in case of seismic in-situ experiments, where sources and receivers locations are usually limited to the earth surface, resulting in quite unfavorable ray geometry for solving inversion problem. Therefore, presented tests provide good estimate of 'maximum capabilities' of the code. For the case of modeling the VTI medium in ideal conditions (which is valuable because it shows 'weak points' of the code and parametrization assumed – if the method fails in some aspect in ideal conditions, it will fail even more in case of real data). But such tests give no or very little information about reliability (expected resolution, dependence on the noise level, dependence on the initial model etc.) of a typical seismic experiment.

Printer-friendly version

Discussion paper

I think that the manuscript would improve a lot if the authors could add more realistic synthetic tests: in order to properly check behavior of the code in case of typical data form seismic experiment, the tests should assume noisy data, surface location of sources/receivers, and also the dependence of the result on various initial models should be studied.

Figs 4-9: It should be marked which column represents which parameter.

####Summarizing, I think that a moderate/major revision, taking into account the above comments, is needed.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-44, 2019.