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

Anonymous Referee #1

Received and published: 1 April 2019

__Overall appreciation__

This paper presents an extension to the code TOMO3D to allow inverting travel times for transverse anisotropic media. The accuracy of the forward operator is evaluated by comparing the output with an analytic solution, and the sensitivity is also quantified. The main part of the paper contains the results of a series of inversion tests to assess the performance of the proposed approaches. Overall the paper is well structured and follows a logical reasoning. There are however a number of points that should be addressed, as detailed in the following section.

__Specific points__

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Discussion paper

1 - Does the paper address relevant scientific questions within the scope of SE?

Yes, very few codes exist for travel time inversion in anisotropic media and I found interesting the idea of evaluating the sensitivity as function of anisotropy parameters. I also welcome the fact that the models retrieved from inversion are compared quantitatively with the true model (too often a qualitative appreciation is presented).

2- Does the paper present novel concepts, ideas, tools, or data?

The paper is pretty classic in its form and approach. An extension to an existing code is presented, that allows inverting travel time data for anisotropy parameters.

3- Are substantial conclusions reached?

Partly. Tests were done with synthetic data, which allow evaluating quantitatively the performance of the inversion. However, I think that the conclusions are not fully supported by the presented work, this for three reasons. First, the authors did not study the influence of noise on the robustness of the results. Inevitably, noise is present in field data (picking accuracy, timing accuracy, statics, etc) and at least the effect of some gaussian noise should be investigated with the synthetic data.

Second, the data acquisition geometry could never be achieved in reality. For the presented tests, an anomalous spheric body is surrounded by sources (Tx) and receivers (Rx) in the whole space. This geometry supposes first that the location of the anomalous body is known, and second that access is possible underground almost everywhere around the body. At best, surface and a few borehole Tx & Rx are typically available for typical surveys. For such geometries, the forward operator does not allow uniform resolution, such as illustrated in the results of the paper. So I think that the capacity to resolve the anomaly is over optimistic.

Third, the initial model is quite close to the true model (the initial model is equal to the background of the true model). I don't think that it is realistic to know that well the properties of the background, especially when the background itself is anisotropic.

How could one know the anisotropy parameters epsilon & delta of the background?

4- Are the scientific methods and assumptions valid and clearly outlined?

Some assumptions are not realistic (see point above).

5- Are the results sufficient to support the interpretations and conclusions?

See point 3.

6- Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes, I see not problem for this point.

7- Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes

8- Does the title clearly reflect the contents of the paper?

Yes

9- Does the abstract provide a concise and complete summary?

Yes

10- Is the overall presentation well structured and clear?

Overall yes, but the section describing the inversion strategies is somewhat hard to follow (especially the details of the sequential inversion). I think that a figure with flowcharts could help.

11- Is the language fluent and precise?

Yes

12- Are mathematical formulae, symbols, abbreviations, and units correctly defined and

used?

Overall yes, but I think that the anisotropy parameters epsilon & delta should be formally defined in section 2. Some parameters in equation 4 are not defined.

13- Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Perhaps the terms for the reflected rays could be removed from the equations (the case of reflected rays is not treated in the paper).

14- Are the number and quality of references appropriate?

Yes, but some references cites in the text are not in the list at end of the paper.

Other comments can be found in the annotated version of the paper.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-44/se-2019-44-RC1-supplement.pdf

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