Dear Prof Dr. Koelemeijer

We express our gratitude to you and the reviewers for the constructive review. We have now taken into account every comment and suggestion. In this letter, we present our detailed response to the reviewers and the revisions we have amended to the manuscript. Aside from this, we have improved the quality of the figures for better readability.

To serve as a guide, phrases in black refer to the reviewers' comments verbatim, in blue is our response to some remarks, and in green are the changes and/or additions to the manuscript. Overall, the peer-review process is indeed helpful to improve the quality of the manuscript. We now look forward for it to be ready for publication in Seismica.

[Seismica] Editor Decision

Dear John Keith Magali:

I have now received two reviews of your submission to Seismica, entitled "A Bayesian approach to the tomographic problem with constraints from geodynamic modeling: Application to a synthetic subduction zone". Based on the reviews received, your manuscript may be suitable for publication after some revisions.

Both reviewers appreciate your contribution, finding it a valuable addition to the existing literature. However, they both also note that your inversion fails to recover the input parameters and would like to have more insights as to why this is the case. I would agree with their comments and hope that it may be possible to investigate this using the suggestion from Reviewer A.

The additional numerical experiment regarding the use of a surrogate model in the forward approach has now been carried out. The main result is presented in Figure 10.

When I myself had a look, I noticed that you did not include an overall Discussion section with some outlook thoughts and implications for how this work may be extended or applied to real data. It would be valuable to see such a section added, also noting how your inversion setup would work in a more realistic geometry or complex 3D setting, e.g. what are the limitations, what parameters may you still recover, when may this work, how do you see this being developed further.

Duly noted. We now have included a separate *Discussion* section. Here, we added a short paragraph regarding some forthcoming strategies we can implement for the method to be applicable to real Earth data. This is summarised in Figure 14.

I encourage you to complete these revisions in a timely manner and to upload a revised manuscript when you are ready to do so.

Best wishes,

Paula

Reviewer A:

This proof of concept study is designed to test the ability of 'geodynamic tomography' to recover the parameters associated with a model of a subduction zone given synthetic surface wave observations created from a geodynamic forward model. This is a valuable and timely contribution to our longstanding effort to use seismic anisotropy to place constraints on mantle flow.

To my mind, the major difficulty with the work as presented is that the McMC Bayesian inversion fails to recover the input parameters both for the isotropic and anisotropic cases. That is, the input parameters (black dots and red lines in Figures 9 and 10) almost always fall well outside the posterior probability distribution returned from the inversion (colours and histograms). It is thus hard to tell if the approach is really beneficial. This is noted and explained briefly in the text on page 13 where difference in the way the CPO is calculated in the forward and inverse models (explicitly using DRex and via a surrogate artificial neural network, respectively) is given as the explanation for the discrepancy. However, I cannot see that this explanation has been validated. Ideally, the numerical experiment would be repeated using the surrogate model for both forward and inverse steps and this would lead to the inversion recovering the input parameters. Without such a test, I cannot see why, for example, an argument that surface wave data cannot recover details of simple models of subduction zones cannot be excluded. It is not clear to me how much computational effort would be required to repeat the experiment in this way.

Agreed. To test whether surface wave data is indeed viable to recover complex deformation patterns coming from instantaneous models of subduction, we now include an additional numerical experiment that uses the surrogate model in both the forward and inverse steps. The main result can be found in Figure 10. Aside from this, we also added few remarks about this result. Please refer to lines 384-396 of the manuscript.

My other comments are essentially presentational. (1) The way the paper is written relies heavily on the reader being familiar with Magali et al. (2021), which is published in GJI. I think some reworking of the manuscript such that this contribution is accessible to readers unfamiliar with that work would be valuable. (2) It's not clear to me how useful the geometrical detail provided on page 3 really is. Some of this information could almost live in comments in the code used to build the model. (3) Around Equation 6 it may be worth saying explicitly which terms are assumed to be known (beta, viscosity and temperature pre-factors, for example). (4) One line 135 you use theta for azimuth of propagation. This letter has already been used for the dip angle of the slab. (5) It is probably worth saying why E is treated differently around line 177, not just that it is. (6) I think the parameter space is five-dimensional not six-dimensional (e.g. Figure 10).

(1) Duly noted. We added a bit of context by drafting a new *Introduction* that is centered on surface wave tomography and its limitations. This can be found in lines 33-70. Furthermore, we added a new section called *Background: Geodynamic tomography* to introduce the method. We then combine this with the previous *Introduction* (i.e. the one from the first draft). Please refer to lines 71-135.

(2) Corrected. We now have removed unnecessary details regarding how the thermal structure is numerically designed.

(3) Corrected. We have clarified in Subsection 3.1.2 which parameters are inverted for, and which ones are held constant.

(4) Corrected. We now replaced theta with psi.

(5) We briefly explain this in line 192 where we say that:

"We choose E as an unknown in order to demonstrate the ability of geodynamic tomography to constrain some properties of the medium rheology. This is essential because we expect that larger values of E make the cold slab more rigid, and thereby lessening the amount of strain-induced anisotropy across it. Since seismic data contain the surface manifestation of strain-induced anisotropy, they then provide potential clues about the rheological structure of the Earth's interior."

(6) Corrected.

Recommendation: Revisions Required

Reviewer B:

The author presents an application of the methodology named Geodynamic tomography presented in Magali et al., 2021, GJI, to a 3D steady-state solution of a subducting plate.

The results indicate that the methodology is capable of constraining model parameters minima when including mantle fabrics and seismic anisotropy, and the thermal structure of the subduction zone. In contrast, the methodology at present fails to retrieve the true value of model parameters due to possibly "the inherent complexity of the deformation patterns considered".

The paper is well organized and I recommend publication after the following comments will be addressed.

Line 113: It has been demonstrated that at convergent margins assuming steady-state flow is not a good assumption, and that the anisotropic patterns can be largely biased (Faccenda and Capitanio, 2012, GRL; 2013, G3). In general, you should discuss more the model assumptions, including the employed mantle flow law (Newtonian, when the mantle fabrics form with non-Newtonian behavior)

Agreed. We added a few comments regarding the implication of our model assumptions, i.e., steady-state assumptions and Newtonian mantle flows. These can be found in lines 454-477.

Line 161: I think you need to better justify the a-priori knowledge of the unknown parameters, as for example slab dip can be up to 90°, their length can be much longer than 200 km, and mantle rheology is likely much more complex than the simplistic flow law shown in eq. 5. Indeed, the a-priori knowledge of the unknown parameters is often centered over the true model parameters (L=150km, θ=35°, R=120km, Tc =800K). Does this help in finding a local minimum?

Whether the prior is centered around the true model parameters or not, this does not affect the overall shape of the posterior distribution since the width of the likelihood distribution is so narrow (i.e. prescribing very low noise levels) that its effect on the posterior is much more apparent than the prior. The same reasoning can be said when we allow for larger prior bounds. In essence, increasing the range of the prior bounds may increase the time it takes for the Markov chain to converge, but the overall shape of the posterior remains the same. Please refer to lines 441-453

Line 185: you should indicate what are the D-REX parameters used, and also for how long in terms of time or finite strain the mantle fabrics are computed for each particle.

Agreed. Please refer to lines 251-258 regarding the parameters used and the stopping condition for strain accumulation.

Fig. 3b: at which depth is the horizontal cross section taken?

Added. Figure 2b is at ~100 km.

Line 206 and Fig.4: is natural strain the natural log of a1/a3 of the FSE?

It is indeed. We clarified this in the manuscript. Please see line 278-281 and Figure 3.

Line 209: Here you should refer to Kaminski and Ribe, 2002, G3.

Corrected. Please see line 281-284.

Line 260: did you use the full elastic tensor or a simplified version of it with only hexagonal anisotropy?

Clarified. We used the full elastic tensor. Please see line 333-334.

Line 266. Can you please provide some performance information of a given step of a Markov chain for the model and processors (I assume one per Markov chain, right?) used here? Such as how long does it take in average to perform one geodynamic + seismological cycle and compute the misfit?

Duly noted. Please see lines 340-349 regarding some performance information.

Line 303: Do you have any idea about how to fix the shift in local minima of most model parameters that are not centered around their true value? Should you consider some other key model parameter and mechanical process or by directly computing mantle fabrics with D-REX at each step of the Markov chain should be sufficient to fix this problem? It is hard to quantify the differences in mantle fabrics and seismic anisotropy between the recovered and true models from Fig. 12 and 13 only. Please add a figure showing the differences (in temperature and Rayleigh phase velocity, anisotropy) at a given horizontal or vertical cross section between the retrieved model when including anisotropy, and the true model.

We attribute the shift in local minima to the use of a neural-network based surrogate model for the calculation of seismic anisotropy (i.e. in the inversion, we replaced the D-REX step with neural networks). Since the observed synthetics are generated by D-REX, the shift in minima is due to the slight incompatibility between ANN and D-REX. To prove this, we added an additional numerical experiment where we use ANN in both the forward and inversion steps. We believe that this numerical experiment is enough to settle the argument on the use of a fast surrogate model, its effect on the resulting distribution, and the viability of anisotropic surface wave data to constrain mantle deformation patterns.. We then added few remarks about this result. Please refer to lines 384-396 of the manuscript.

Line 304: the reconstructed mean temperature field shown in Fig. 11 are the average of the 20 Markov chains? Please, clarify.

Clarified. Please see line 399-400.

Manuele Faccenda

Recommendation: Revisions Required

Thank you very much!

On behalf of the authors,

John Keith Magali