Solid Earth Discussions

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Comment on se-2021-40

César R. Ranero (Referee)

Referee comment on "Reflection tomography by depth warping: a case study across the Java trench" by Yueyang Xia et al., Solid Earth Discuss.,
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The use of PSDM images is unfortunately not yet widespread in seismic processing even though the method has well defined and robust procedures for more than 25 years. However, Geomar has a long tradition in seismic processing and imaging and the geophysical group there has used PSDM in many locations and settings to provide novel information and significant scientific results.

The Geomar group in this manuscript present a new approach to produce the macro velocity field necessary for PSDM which is in fact the most critical and arguably complicated step towards producing high-quality PSDM images. The scope of the manuscript is focused on providing the evidence of the superior performance of the NRM as an automatized method to iteratively produce a final model and the resulting images are little discussed in terms of the geological significance of the structures.

I think the work is a valuable contribution that should be published, but it requires a number of revisions and clarifications before it can be accepted.

The accompanying review by N. Bangs provides very valuable commentaries that I fully subscribe and I have a few more to add:

Comment 1)

There seems to be a misunderstanding in terms of the resolution of the technologies available to build macro-velocity models:

It is stated in Line 61 "The starting velocity model is normally retrieved from seismic data semblance velocity analysis (Neidell and Taner, 1971) of either non-migrated CMP gathers, pre-stack time migrated CIP analyses, wide-angle travel-time tomography, or full-waveform inversions if no additional in situ information is available (Gras et al., 2019; Górszczyk et al., 2019)"

In reality, the uncertainty intrinsic to define the reflector position and velocity above makes reflection-based travel time tomography a method with undefined accuracy that may be improved by using body waves. Further, full-waveform inversions (FWI) is not used to build initial models, but to increase the resolution of models created by some form of travel time tomography, potentially including those obtained from inversion of body waves or the method described in this manuscript.

Comment 2)

In line with my comment 1, the statement in Line 73 “The precision may be improved by setting a smaller vertical and lateral picking interval to maximize the reliability of the tomography. On the other hand, accuracy is strongly limited by signal interference, background noise, side echoes, and accurate depth error information “ and the statement in Line 77 “we improve the accuracy and precision of the depth error estimation without any hyperbolic assumption or predefined depth horizons of the subsurface structure” fall in the same category. This is that apparently the overall message collectively conveyed in the introduction of the manuscript is that reflection tomography provides the highest possible resolution for velocity determination, which is incorrect. My comment is not in detriment of the work presented, but an introduction should necessarily place the scope of the work in the right technological perspective.

Comment 3)

Section 2 . "Non-rigid and warping matching techniques” describes several different NRM technologies but it is unclear the advantages of some of the them over others, and which one they finally choose and why. In particular the last 2 paragraphs (lines 116-130) could be recast to clarify their choice.

Comment 4)

In Fig 2. “(d) Residual move-out picks calculated by recursive summation of the relative depth errors (b) at predefined depths to get the cumulative depth error. “

I may have gotten it wrong, but should not the vertical axis have a scale that is not the

same km of a) and c)

Comment 5)

Line 153-154. "An application of the NRM field to flatten the synthetic gather requires a recursive depth variant correction."

Since the manuscript deals with the method it would be great that the application is somewhat further detailed.

Comment 6)

Line 171. "Of importance for tomography is not the waveform, but only a correct depth error estimation along reflected events. "

This is again related to my comment 1 and 2, clarify that this is travel time inversion based on near vertical reflections-only.

In fact the authors may want to explore the potential of Vp uncertainty estimation. Since they have developed a method that largely automatizes velocity picking they could design for instance a Monte Carlo based strategy to evaluate the quality of the inversion solution.

In the current manuscript the initial model is only briefly described and there is no information how much influence the choice of an initial model may have in the performance of NRM and thus on the final velocity model.

Comment 7)

The following two sentences in the manuscript link to my previous comment 5 on initial models.

Line 207-210. ". The initial velocity model for the reflection depth tomography was merged from a velocity tomographic inversion of a collocated 2-D refraction seismic line covered by 46 ocean bottom seismometers (OBS) with a spacing of 6 km (Planert et al.,

2010) and a **manually estimated**velocity model for the near seafloor structure."

What is a manually estimated velocity?

Line 213-214 ". As a consequence, the **approximated velocity** at shallow depth was additionally smoothed before merging with the wide-angle velocity model and was used as the initial velocity for the NRM-tomography. "

What is the **approximated velocity?**

I think it would be good to introduce the initial model earlier in the manuscript and to explore its significance. In their example offshore Indonesia the seismic line had been previously intensely studied and a good quality model obtained from ocean bottom seismometers existed, which is not typically the case for most streamer lines. What would be the approach then? To obtain an initial model by another approach? or can NRM work with a simple 1D initial model? would this method work for land data where the initial model may be much more complex?

Comment 8)

Fig 4. Water Vp of 1590 m/s appears unusually large. In apparent accordance, the CIP gathers in Fig 5 (a) and (b) show un-flattened seafloor reflections. Is that Vp realistic? it is relevant because affects raytracing from across the water layer.

Comment 9)

If they need to condense material to include new or expanded sections I would recommend removing one example offshore Java, or possibly the line from Hikurangi which I imagine that was collected with 6 or 8 km streamer, but the effect of a different acquisition is not discussed and the geology is also rather briefly described.

Also sections 3.2 and 3.3 descriptions might be condensed without harming the manuscript scope.

I summary I recommend that the manuscript may be accepted after some clarifications are made, so that the scope and the description of the methodology, which is the core of

the contribution, are further explained, and that the role of the initial model is integrated in the discussion.

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