Solid Earth Discuss.,
https://doi.org/10.5194/se-2017-128-AC4, 2018

Interactive comment on "Structure of the Central Sumatran Subduction Zone Revealed by Local Earthquake Travel Time Tomography Using Amphibious Data" *by* Dietrich Lange et al.

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Received and published: 6 March 2018

Dear Dr. Koulakov,

We thank you for your careful and constructive review. In the accompanying revision notes below we listed in detail on how we incorporated your comments and suggestions. Your original review text is shown in black and our answers in blue colour.

Yours sincerely,
Dietrich Lange and co-authors

This study deals with a very interesting region of Central Sumatran subduction zone. In some previous studies (e.g. Koulakov et al., 2016), it was proposed that the repeated supereruptions of Toba were controlled by the subduction of the Investigator Fault Zone (IFZ) that separate two plate segments of different ages and possibly brings to the mantle an anomalous amount of water. The topography of the forearc along the IFZ line behave differently than in other subduction segments along the Sumatra coast that probably indicates that the IFZ greatly controls the accretion process. I expected that the IFZ should be the most prominent structure in the area considered in this study, and I am a little bit disappointed that IFZ-related structures are almost not revealed in the tomography results. I think the authors should pay more attention to this problem.

Since a fracture zone is characterized by oceanic material on both sides of the fracture the expected density contrast and hence velocity contrast is small. From a conceptual standpoint the most significant difference in velocity contrast originates from different oceanic plate ages on both sides of the fracture zone. Regarding the seamounts found on the IFZ very little is known about their deeper velocity structure related to the IFZ. We already state in the text (Page 19) that for smaller scales there are along-strike variations known for the Sumatran margin:

"Based on MCS data, Henstock et al. (2016) identify an isolated 3 km basement high close to the 2005 slip termination as well as along-strike variations of basement relief. Such features are large enough to affect the rheological behaviour of the plate interface such as coupling but are below the resolution of our local earthquake tomography."

From the methodological point of view, this paper is an excellent example of the SIMULP-based description similar to dozens of previously published papers based on this tomography code. All the steps of the traditional SIMILP workflow have been carefully completed and described. The problem is that many statements taken as an axiom by the SIMULP users seem to me not grounded and adequate. The criticism presented below relates to all SIMULP-based studies, not to this particular case. Therefore it would be unfair from my side to insist on changing everything in this specific paper. However, I would be happy if some of my arguments will be taken into account during the revision and will be useful in future studies.

We feel that these fundamental points risen by the reviewer might be a bit out of scope for our paper. As you already mention SIMULPS/SIMUL2000 is a very established inversion code used for more than two decades for many local earthquake inversions across different scales and virtually all geological settings. For many of these regions an independent velocity model was known from active seismics which SIMUL2000 could reconstruct, obviously with lower resolution than the reflection seismic data.

The major problem of the algorithm is defining the parameterization grid according to the expected resolution, so that the grid spacing is equal to the size of minimum resolved anomaly. This is a completely wrong strategy. If the size of anomaly is compatible with the grid spacing, such anomaly would appear completely different if its center coincides with one node or it is located between nodes.

In this case, the solution will be grid dependent, which is a serious flaw of tomography. Such one-node-based anomalies will be completely changed if, for example, you shift the grid a half step. To avoid such grid dependency, we should define the grid spacing much smaller than the size of anomalies, so that every resolved anomaly is based on several nodes. The stability of the inversion should be controlled not by grid spacing, but by smoothing and regularization in inversion. We can see such grid-dependency in the results presented in this paper. For example, in the Vp/Vs ratio section in Figure 5, we see that at X=-100 km, there is shallow blue and deep red; in the next column at X=-70, there is shallow red and deep neutral; then at X=-40, there is heavy red anomaly etc. It is clear, if the points were shifted to half step and installed at -85, -55, -25, the anomalies would be completely different.

We disagree that this would be completely different. In case we would shift the grid spacing for the 2D vp/vs inversion by 50% of the node spacing the very localized anomaly (based on the nodes at 0 and 5 km depth) at Figure 8 (previously Figure 5), panel B x=-100, Y=-5 would still reveal reduced vp/vs values, but might be a bit more smeared out between two nodes since the velocity between the grid nodes in SIMUL2000 is estimated from spline interpolation. We cannot follow the statement that the grid spacing is too coarse since every significant anomaly in Figure 8 is based on more than 2 nodes. Of course, SIMUL2000 does not allow to change the grid spacing during the inversion steps. However, we tried different grid spacings (and for the 2D model different azimuths of the profile) to assure that the inversion results are robust. In fact, we could not find significant differences of spatial distribution of anomalies within the resolution given by the checkerboard test (e.g. 2x2 nodes for the 2D velocity model, equals 30 x 10 km for the central part of the velocity model, Figure 5)

One of the inherit challenges of local earthquake tomography is the spatially heterogeneous resolution originating from the heterogeneous ray coverage. The suggested much smaller grid spacing compared to the size of the anomalies, let's say 5 km grid spacing, would allow an apparently higher resolution. However, such a small grid spacing enhances the danger of oscillating anomalies, all of which might not be resolved. Therefore we prefer to continue with an approach where we use in the final paper a grid spacing which is about two times than the best expected resolution. During the data processing we tested from sparse to fine grid spacing and carefully choose the final grid spacing based on our expected resolution.

We added the word carefully to the main manuscript in the method section:
"After carefully testing different spacing parameters for 2D and 3D inversions in all three directions, we selected the node spacing as a compromise between resolution and stability of the inversion. "

Another problem of the SIMULP workflow is using the trade-off curve for estimating optimal damping parameters. This curve is calculated from a series of inversions with different damping values in the first iteration. Why should it be valid for the inversions in multiple iterations? It is clear that number of iterations also affect the stability of the inversion and, therefore, connected with damping. For example, a fixed damping may provide an overdamped solution in one iteration and underdamped solution after 10 iterations. It is obvious that an optimal damping value estimated from the L-curve for one iteration is not optimal for ten iterations. In addition, I have never seen any study supported by modeling results that confirmed that the value in the corner of the L-shaped trade-off curve does really provide the best damping. At the same time, I know opposite examples showing that the best damping values may be far from the corner point.

We follow the philosophy that the best model in local earthquake tomography is:
a.) most simple ("smooth") model
b.) the model that explains the data best
and exactly this is the physical reason for the damping curve. A damping value far away from the corner point does not match both criteria. We admit that the data and model variance is just taken from the first inversion and SIMUL2000 just uses one damping value for all inversions. If the changes in the first iteration are, for example, only about one tenth of the total, then the concerns would be somewhat be justified. However, for the inversion runs the first iteration was always much larger than in subsequent iterations, and changes getting progressively and quickly smaller. Therefore the first iteration step is representative of the total damping because it includes the largest changes.

We added a sentence to the manuscript to clarify how exactly the damping value was chosen and how the velocity models relate to the choice of damping value:

"SIMUL2000 uses one damping value for all inversion steps and the model and data variance for the trade-off curve is taken from the first inversion step. We made various inversions with different damping values and found that the spatial distribution of anomalies stays similar, but with varying amplitudes of the anomalies. "

This stable behaviour of the inversion we relate to the choice of grid spacing (see paragraph above), which prevents the occurrence of oscillations in the spatial domain.

I have serious concerns about performing synthetic modeling. The good synthetic modeling should provide the realistic assessment for the resolution capacity and, therefore, it should adequately simulate the real workflow that is used in case of processing of experimental data. In passive source tomography, the most difficult problem is the trade-off between source locations and velocity model. For example, if a source is located between positive and negative velocity anomalies, the initial step of source location in the 1D velocity model would shift the coordinates and origin time so that the residuals would be close to zero. In turn, it will make problematic recovering the velocity model. It is clear that if we start recovering of synthetic model from the step of source location in the 1D starting model (as we do for the experimental data), the result would appear not as nice as in the case when we use the residuals directly calculated from synthetic model. Similar difficulties take place in the case of deep sources. Shifts of source coordinates and origin times "kill" any residuals that would allow us to restore layered structures, such as in the lower panels in Figure 6. The problem of the SIMULP workflow is that in synthetic modeling, they start restoring anomaly without performing the step of initial source locations. The residuals directly computed from the synthetic models provide very nice restoration of anomalies. However, such modeling is not related to realistic resolution capacity, which is strongly perturbed by the trade-off between the source and velocity parameters.

It is not correct that the events are not located prior to the inversion in the synthetic test. Please see the SIMULPS manual (Evans et al., 1994), page 13, paragraph *nitmax*, where it is stated:
"Events input as "earthquakes" (Unit 04) are first relocated, which may not be what you intended"

We re-check the re-location of events prior to the inversion with the SIMUL2000 code for the synthetic tests, and indeed the events are relocated (as described in the manual) with performing the step of initial source locations.

Another problem of synthetic modeling in the SIMULP workflow is that the anomalies are predefined in the same nodes as used for inversion. Successful restoring the anomalies centered in the nodes with spacing of 30x10 km gives an impression that the existing observation scheme would allow us to resolve such size of anomalies in the case of experimental data. Obviously, it might be true only if the anomalies perfectly centered with nodes. However, if an anomaly of 30x10 km size is located between nodes, it would obviously not be recovered, or strongly smeared. As the locations of real anomalies in the nature are not known, such modeling gives wrong assessment for the resolution. The shapes of synthetic models should be completely independent of the parameterization grid. In this case, we will recover not only amplitudes of anomalies (as in the present case), but also their locations and shapes, that is much more complicated.

As discussed above the inversion grid spacing is already above the limit of resolution. The synthetic models shown in Figure 7 (previous Figure 8) do not follow a simple geometry and are not fully aligned with the inversion grid. Figure 7, right panel, anomaly at 5 km depth shows a synthetic restoration test with two obliquely to the grid-orientated anomalies. One anomaly is at shallow depth (5km), the other one directly below (following the downgoing prolongation of the IFZ) at the depth of the plate interface. Of course one could make arbitrary anomalies with complex shapes, but since the anomalies are not known this would make the synthetic modelling overcomplicated. Furthermore if we would “smear” out an existing input anomaly across different grid nodes (e.g. not only using constant velocity perturbations of +/-5%) the restoration test would make a judgment of stability and spatial resolution more difficult.

Other specific comment on the paper. I did not understand the meaning of the 2D modeling performed in this study. Was it based on all data in the area? Does it mean that velocity along Y-coordinate is presumed to be constant? If yes, I would hardly expect any stable solution because of the existence of significant heterogeneities along

the trench line (for example, due to the presence of IFZ).

We modified one sentence section 4.1:
“We tested the dependency of the 2D inversion (e.g. constant values along the y-axis oriented parallel to the trench) on the 1-D input model in order to estimate the stability of the inversion and its ability to converge.”

The inversion for the 2D model is very stable. We already discuss in section 4.1 the different 1D velocity models used for the inversion of the 2D models. The 2D inversion is not strongly dependent on the 1D starting model indicating a robust inversion result. In case of prevailing 2D velocity structure (such as for most subduction zones) the 2D velocity inversion are usually more stable compared to the 3D inversion (e.g. see Haberland et al., 2006 and references therein). Since we did not find strong lateral variations along-strike, the observation of a stable 2D inversion is the expected behaviour. As we discuss in the paper there are limited lateral variations of the 3D vp model at the resolution of our inversion.

Why the Vp/Vs ratio is only shown for the 2D model, and not for the 3D model? It would be interesting to see the variations of the Vp/Vs ratio in the map view. I expect that if the IFZ is saturated by water, it would be seen in the Vp/Vs distributions.

As already stated the S arrivals are characterized by a higher noise level due to the tropical conditions and anthropogenic noise sources (e.g. large trees for the stations in the tropical forest, traffic close to settlements).

We added to chapter 3 (Local earthquake tomography):
“We inverted 3D velocity models for vp/vs ratios. However, due to the limited data variance reduction of the 3D inversion of vp/vs, and the low quality of S onsets, the 3D vp/vs inversion is not robust. Therefore we only discuss vp/vs ratios of the 2D inversion.“

And modified the sentence in chapter 2 (earthquake data):

"The main sources of noise on the records were tree movement, rain due to the tropical environment and anthropogenic noise (e.g. traffic), affecting in particular the horizontal components."

We added to the first sentence of section 5.4 (vp/vs model of the forearc):
"As discussed (chapter 3) S-onsets are of lower quality due to tropical conditions and anthropogenic noise. Therefore, we only present the 2D vp/vs inversion results (Figure 5, 8b, previous Figure 5 and 6)."

In horizontal sections, it would be better to present relative anomalies instead of absolute velocity. In the present case, it is hard to see any nuances in dominantly green, yellow or red colors corresponding to absolute velocities at specific depths. The traditional "blue-white-red" scheme would provide much clearer images for the velocity variations.

On the use of the absolute velocity values:
Relative anomalies show the differences in respect to a given reference model. Since anomalies relative to a reference model are more difficult to understand we prefer absolute velocities, in agreement with other papers showing absolute velocities of local earthquake tomography. Obviously, figures using relative velocities enhance spatial variations in the velocity model. Due to the results of our checkerboard tests we think that these small variations are not resolved and prefer to show absolute velocities, which better reflect the resolution capabilities of our inversion.

On the choice of the colour scale:
We currently use the colour scheme rainbow which is a widely used colour scale for absolute vp velocities. This colour scale was previously used for various local earthquake tomography publications from different groups (e.g. DeShon et al., 2006, LET Nicaragua, doi: 10.1111/j.1365-246X.2005.02809.x; Pesicek et al. 2010, inversion of regional–global data, doi: 10.1111/j.1365-246X.2010.04630.x; Hicks et al., 2012, LET Central Chile, doi: 10.1016/j.epsl.2014.08.028). We optimized all superimposed

drawings (coastlines, Sumatran fault, events, Mentawai fault etc.) for optimal visibility. Therefore, we did not apply changes to the colour scales.

In vertical sections, in addition to absolute velocity, it would be helpful to present also the relative anomalies. In some cases, they appear to be very informative for interpretation.

See previous point.

P8L3: Does the 76% of reduction correspond to absolute residuals (L1) or squared values (L2)?

This corresponds to squared values.

Figure 6 is mentioned in the text prior Figure 5.

Previous Figure 5 is now Figure 8 and Figures 8,7 and 6 were renamed to Figures 7,6 and 5.

Was there any 3D synthetic modeling for the Vp/Vs ratio? Why the synthetic recovery results in the 3D case are shown only for the Vp, and not for the Vp/Vs?

We did extensive 3D synthetic modelling and checkerboard tests. As discussed above the resolution of the 3D vp/vs velocity model is poor and the residual reduction of the 3D vp/vs inversion is small. Therefore, we did not include the 3D vp/vs velocity model with their corresponding synthetic models and checkerboard tests. The 2D vp/vs checkerboard test is shown in Figure 5, right panels.

In Figure 8, the contours of the initial synthetic model should be highlighted.

The contours of the initial synthetic model in Figure 7 (previous Figure 8) are already highlighted in red and green colour using the same colour scale as the recovered models. The caption states already:
"Red and green lines indicate the 5% contour lines of the input anomalies."

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