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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.

I.Yu. Koulakov (Referee)

koulakoviy@ipgg.sbras.ru

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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.

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.

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.

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.

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.

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.

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).

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.

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.

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.

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

Figure 6 is mentioned in the text prior Figure 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?

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

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