Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-240-AC3, 2021

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "GO_3D_OBS – The Nankai Trough-inspired benchmark geomodel for seismic imaging methods assessment and next generation 3D surveys design (version 1.0)" *by* Andrzej Górszczyk and Stéphane Operto

Andrzej Górszczyk and Stéphane Operto

agorszczyk@igf.edu.pl

Received and published: 25 January 2021

Anonymous Referee 1

The manuscript "GO_3D_OBS - The Nankai Trough-inspired benchmark geomodel for seismic imaging methods assessment and next generation 3D surveys design (version 1.0)" by Górszczyk and Operto is an excellent, useful and timely contribution to the field of geophysics and, in particular, for seismic imaging and inversion. The

Printer-friendly version

Discussion paper

overall goal is the design and building of a detailed 3D benchmark geophysical model with a visco-elastic parameterization that represents a subduction zone. I am actually impressed by the level of detail and meticulousness put in all the steps of the work. It integrates a large number of consistently designed and conformally shaped bodies, layers and structures that represent highly realistic geological and tectonic features present at many subduction zones. Despite the large degree of structural complexity, the model is constructed following a well-designed and logically structured sequence of steps so that the final result preserves an astonishing level of "geological realism". The steps of the process are designed to sequentially incorporate an increasing level of complexity and details to the model but, at the same time, they are isolated and flexible enough to change any of the attributes of the model or to construct a different one if it were necessary. My main concern is that, while detailed, the technical information provided in the manuscript is not sufficient for a motivated reader to reproduce the model itself. The description of the relationships and equations applied to perform the transformations at each step is only general and not specific for the different units. The editorial team should consider whether this is acceptable or the issue should be addressed (probably by adding a large volume of supplementary material). In any case, I would like to thank and congratulate the authors for the thorough and rigorous work, which I find particularly useful for the years to come. In summary, I consider that it deserves to be published after a minor and limited revision only. I have a number of minor comments and observations made while reading the manuscript, although I note that a few of them are partially addressed in other sections of the manuscript.

Dear Referee,

Thank you for your positive assessment of our work and for your constructive comments. Please find hereafter our answers.

Best regards
The authors

Minor comments:

Title: The reference to Nankai trough is perhaps too specific for a title. Consider changing it for a general reference to a subduction setting, it could be more effective to attract wider readership attention. The specific reference to Nankai can be included in the abstract and text.

We thank you for this comment. We agree that referring to Nankai Trough in the title is too specific according to the objective of the paper which is to propose a crustal benchmark representative of a subduction setting. We change the "GO_3D_OBS - The Nankai Trough-inspired benchmark geomodel..." to "GO_3D_OBS - The multiparameter benchmark geomodel..."

Line 9: Did you ever consider adding anisotropy? Why did you decide not to?

We refer to this issue in Section 4.5. At the current stage of development, we wanted to release the isotropic version of the model to reach as large a community as possible before considering more complex parametrization. Extension towards anisotropy is certainly one of the main directions of development. However, it will require an in-depth geological analysis to assign the most suitable anisotropy in terms of symmetry class and strength to each structural units. Also, anisotropy is not well documented in the deep crust.

Line 24: I would add joint refraction and reflection travel-time tomography in either 2D (Korenaga et al., 2000) or in 3D (Melendez et al., 2015)

Korenaga, J., et al, 2000. Crustal structure of the southeast Greenland margin from joint refraction and reflection seismic tomography, J. geophys. Res., 105, 21 591–21 614.

Meléndez, A., et al., TOMO3D: 3-D joint refraction and reflection traveltime tomography parallel code for active-source seismic data – synthetic test, Geophys. J. Int., 203, 158–174, 2015

Thank you for this suggestion. We add these references related to refraction-reflection tomography.

Line 110: Exploration of alternative options for robust objective functions also in Jimenez-Tejero et al (2018)

Jimenez-Tejero, C., et al. Appraisal of Instantaneous Phase-Based Functions in Adjoint Waveform Inversion, IEEE Transactions on Geoscience and Remote Sensing, 56, 9, 5185 - 5197, 2018

We appreciate this suggestion. However, in this place, we refer to the design of more robust types of distances in FWI rather than application of the L^2 norm to some specific attribute of the signal - in this case instantaneous phase.

Line 134: "of subduction zones"

We correct the sentence.

Line 135: "As an experimental"

We correct the sentence.

Line 144-145: "The empirical components impose the physical parametrisation (...) in terms of the magnitude of subsequent parameters and relations between them" > It is unclear to me what you mean here. Could you rephrase or clarify it further?

We correct the sentence. We wanted to say that the physical properties in the units are defined according to the results of previous studies and empirical relations between the different parameter classes.

Line 147: What do you mean by "realistic structure variations"?

We correct the sentence. We mean realistic variations of the geological structure. We also check that these variations induce significant 3D effects in the wavefields such that the detrimental effects of the 2D assumption in seismic imaging can be assessed more accurately (Figure 10).

Line 154: "as follows"

Printer-friendly version

Discussion paper

We correct the sentence.

Line 163: "features which were interpreted" > either "features that were interpreted" or simply "features interpreted" ("which" goes after a comma)

We correct the sentence.

Line 166: "was designed"

We correct the sentence.

Line 184: " It currently approaches the subduction zone and simultaneously undergoes the thrusting process" > How can it do both things at the same time? Approaching prior to thrusting is closer to what is shown

It might be true that the thrusting process of Zenisu ridge began once the ridge approached closer to the accretionary wedge. We wont argue about this detail, however in Mazzotti et al. (1999) it is proposed that Zenisu ridge is a compressive structure originated from the N–S shortening of the volcanic Izu-Bonin arc resulting from the kinematic discontinuity along the border of the arc with the Shikoku basin. It is therefore related to the collision of the the Izu arc with central Japan. On the other hand Chamot-Rooke and Le Pichon (1989) proposed, that after the breaking of the crust along Zenisu ridge the subduction of the Philippine Sea Plate proceeds and while the break-point moves towards the trench the thrust is tilting.

Line 228: Equation 1 would require some extra explanation. As it expressed, the right hand sides give always the same value. I mean, explain a bit how do coefficients a,b,c,d,e vary in different domains

We reformulate Equation 1 to make it more strict. We also add subscripts "n" to the coefficients a, b, c, d, e to underline that their definition depends on the node "n". Those coefficients are set up and tuned separately for each node in Figure 2a such that, after the projection, they follow the shape of the pre-designed geological structures while guaranteeing the conformity of these structures. In the next paragraph, we discuss possible dependencies between the functions used to project the nodes belonging to a given interface. An illustration of the projection functions that build the main faults in the model is also presented in Figure 3a.

Line 241: "of another"

We correct the sentence.

Line 293 and Fig. 4b: What are the criteria to set the gradient matrix values in the different units? Most have vertical gradients reflecting compaction or lithological burden but others do not seem to. Please elaborate a bit on this

We discuss this issue earlier in the text. "The spatial variation of the parameters within the same unit can be related to increasing depth in the mantle, layering of the crust, low-velocity zones in the subducting

sediments, compaction in the prism or damage zones around the faults etc.". For example, to implement the LVZ within the layers of subducting sediments (subduction channel) - that extend over the whole distance of the model - we need to introduce gradients, which mimic horizontal rather vertical parameter variations. Similar comment applies to the tilted layers representing underplated material where the implemented gradients follow the slope of these structures.

Line 294: Why is there no -vertical- gradient in the shallow sediments? It is typically a place where changes of properties with depth by compaction are strongest

The shallow sedimentary layers are relatively thin compared to the other large scale units. Therefore, even for the final model grid size (that is 25 m), a single sedimentary layer is defined by just a few grid points to implement the smooth velocity variation inside this layer. However, the compaction effect in the sediments is introduced by defining the gradually increasing velocities inside the successive layers. There are six thin sedimentary layers in the trench and five in the forearc basin, which lead to quite realistic velocity increase with depth after application of the small-scale stochastic components.

Equation 3: G1=Gn*3.3 (remove parentheses)

We change the expression in Equation 3.

Line 315: Vp=7.8 km/s is far too high for oceanic L3

Printer-friendly version

Discussion paper

We agree that the endpoint V_p in L3 is high. We therefore decided to regenerate the model (all parameters) and the associated modeling examples for the sake of consistency of the manuscript.

Line 326: "subducting volcanic ridges" or "subducting seamounts"

We correct the sentence.

Line 339: What do you mean by "second-order parameters"? Which are the "first-order" ones?

We rewrite the sentence. By second-order parameters, we mean those parameters (density, attenuation) that have a small influence on the kinematic of wave propagation (traveltimes) - and therefore on the results of the seismic imaging. In contrast, wavespeed mainly controls traveltimes, hence making their signature in the data dominant for tomography and waveform inversion applications.

Line 351: Brocher's (2005) is an empirical relationship with significant uncertainty/error bounds. Applying the exact same relationship (same polynomial conversion law) in all units and sectors sounds like too "perfect". In particular this approach "fixes" Vp/Vs. Wouldn't had been better to define and apply slightly modified versions of the conversion laws in different units/sectors, too? Would this have any effect at all on FWI?

Indeed, the Brocher's compilations are derived from other empirical relations and they are burdened with a certain level of uncertainty. However,

since they are expressed as the best-fitting 5th order polynomials, they can generate significant variations in V_p/V_s and V_p/rho ratio from one unit to the next, which is far more realistic than using constant ratios. While it would have been even more realistic to introduce additional deviations (random or geology based) to these relations, they would not have had significant impact on the results of the FWI. A notable exception is however the subduction channel where on top of the Brocher's relations we implement additional small-scale variations of the elastic effects to represent fluid overpressure, fluid diffusion and dry zone according to the interpretation of a migrated section across the Gulf of Guayaquil.

Line 352: "on laboratory measurements, (...)"

We correct the sentence.

Line 366: thermal effects, too?

We add this suggestion to the list.

Line 371: Same as in Vs(Vp): wouldn't it be better to allow for a range of variation in the shape of conversion laws to allow for heterogeneous Qp/Qs?

As we mention before, although this would lead to a more realistic model in terms of parametrisation, we do not expect it to impact the waveform inversion. In addition, the estimation of Q_p and Q_s from the field data is uncertain itself, and therefore adding small deviations to the conversion

laws which we apply can still produce a Q model which falls into this uncertainty range and provide the negligible changes into the wavefield generated in this model. By negligible we mean the changes which are too small to influence the large-scale 3D waveform inversion at the current stage of development.

Line 390: Small-scale perturbations: Interesting approach, it makes all sense introducing small-scale perturbations, although the selection of the size and shape of the SEs seems rather arbitrary in some cases. On the other hand, couldn't it happen that in some instances the values of the stacked SEs align so that the size of the magnitude is larger than 1? I mean up to 4 or -4 if you are stacking four SEs? Or you re-normalize between (-1,1) after stack?

We use the primitive disk-shaped SE with variable magnitude, since it is easy to control its spatial-scale ratios. This gives a certain level of control on the size and the shape of the final small-scale perturbations. Alternative approach could employ, for example, fractal functions although we did not investigated such an implementation in 3D. Indeed after the stacking the magnitude of the perturbations significantly exceeds the (-1,1) interval. This is because the SEs strongly overlap with each other and for a given SE in the 3D space we have 26 nearest neighbour SEs. Additionally for different structural units we use the sum of the stacks resulting from stacking of the SEs of different scales. Therefore before using the final stochastic matrices to apply the small-scale perturbations we re-normalise the full 3D stack between -1 and 1. We add this information into the body of the manuscript.

Line 427: "The overall distribution of the energy added to the background medium (...)

is close to normal" > It would be nice to illustrate this with a figure.

We add the inset into the Figure 6b presenting the normalised histogram of the introduced velocity perturbations. We also rewrite the sentence referring to the energy distribution inside the wavenumber spectrum.

Line 443: How are warping matrices in figs 7a and 7b build?

To obtain those matrices, we first generate the matrix of the same size with random values. In the second step, we interpolate between the uniformly sub-sampled elements of this random matrix using splines. The spatial scale of the final shifts in both matrices in Figure 7a and 7b is controlled by the sub-sampling - namely the dense/sparse sampling leads to small/large scale of perturbations. We add this information to the manuscript.

General comment: I wonder whether it is necessary that you give precise information on the expressions used at each step of the process (projection, gradient, physical parameters, stochastic perturbations, warping) applied at each unit, etc. I mean, if you do not do this, your results (in this case, the model) are not fully reproducible. At least not with the information provided.

Before we started thinking about this project, we identified the lack of such a geomodel in the geophysical imaging community. Our idea was to fill this gap and freely release GO_3D_OBS as a benchmark to potential users. Through this, we wanted to provide a tool that helps understanding better the potential and limits of high-resolution seismic imaging methods at the crustal scale and hence stimulate the geophysical community to

apply more routinely these techniques. Importantly, this benchmark gives the opportunity to compare the results of different imaging approaches provided the model remains unchanged and available in the form that we present here. As an illustration, the purpose of the synthetic models routinely used in exploration geophysics is not to reproduce or modify them but to provide benchmark for comparing different techniques. Therefore, the ultimate goal of this study is not to discuss a workflows for geomodel building (which is here developed from scratch and would require a tremendous amount of supplementary materials, manuals and data) but to propose a benchmark devoted to the assessment of seismic imaging techniques.

Moreover, some components of the model, which are based on the large randomly generated matrices, prevent to readily build the geomodel without access to the heavy amount of intermediate data, which are used as I/O during subsequent steps of the geomodel building. We want to mention here, that due to the size of the files containing the model itself, we encountered problems in finding open-access data-repository, and therefore providing even more data is beyond our abilities.

On the other hand, we believe that the different steps implemented to build the geomodel from scratch are described in enough details to build similar models and/or inspire future studies on geomodel building.

Figure 9: The figure is excellent, although I do not think that it is the best way to show the effect of variable attenuation. It would probably help showing a few individual traces (seismograms) showing details of the effect for different sectors of the model, offsets and recording times. It would help visualizing not only amplitude but also phase differences.

Figure 10: Same as with fig 9. Showing a few well-chosen traces (additionally or alternatively to the whole records in this fig) could also help visualizing differences and

effects. Comment also valid for fig 11m where differences are even slighter.
Line 530: "modelling example, which (...)" or "modelling example that (...)" Figure 12: Same comment as in the previous two figures concerning comparison of several individual traces.

We correct the sentence in Line 530. Regarding Figures 9-12 presenting different waveform modeling scenarios, we decided to keep our way of data comparison. We understand and appreciate the suggestion about the direct comparison of individual traces recorded at different location of the model. Such an analysis would certainly provide a more precise insight on the footprint of different approximations on wave propagation and on the subsequent inversion. This would however deserve a complete and exhaustive study and a detailed description, which we believe is beyond the main scope of this manuscript. Our aim here was to present an overview of different factors or approximations that can affect imaging techniques (related to the physics, the 2D approximation in complex media or the modeling scheme) and to check to first order that our benchmark can be used to reproduce these effects.

Line 577-578: An extra question to be considered: what can be gained from joint inversion of spatially coincident OBS and MCS data? Would it somehow mitigate the need of "densely sampled" OBS acquisitions?

Thank you for reminding us about the benefit of coincident MCS+OBS acquisitions. Indeed, combining OBS and long-streamer MCS acquisition still remain beneficial since they provide images of the crust at different (complementary) scales and depths, in particular because it remains challenging to push FWI at very high frequencies (beyond 15Hz) due to

computational cost and error accumulation during the nonlinear iterations. From more methodological viewpoints, the final FWI model from the OBS data can be used as an initial model for the subsequent FWI of the MCS data to further increase its resolution at shallow and intermediate depths (taking advantage of the higher frequency content and the higher fold of the MCS data relative to OBS counterpart). Therefore, the shallow FWI model inferred from MCS data can substitute the FWI model inferred from OBS data in shallow areas when the latter is polluted by aliasing artefacts resulting from coarsely sampled OBSs.
Also, the MCS data can be migrated with the velocities estimated by FWI to tentatively image reflectors at depths where migration-based velocity analysis are ineffective due to insufficient reflection move-out, hence prolongating at greater depths the depth-migrated images (Gorszczyk et al. 2019). Indeed, performing 3D towed-streamer surveys in Academia seems out of range due to the lack of equipment and the cost of these surveys (in particular, at the scale of a margin). Only, a coarse grid of MCS lines can be viewed today, which remain highly beneficial and complementary to 3D OBS acquisition.
As the OBS deployment geometry, the shooting strategy during 3D OBS experiments will need to be optimized to maintain reasonable acquisition time while optimizing imaging resolution. Definitively, the GO_3D_OBS geomodel should help to optimize the design of the next generation 3D academic surveys considering the limited pools of OBS and the limited acquisition time made available to the academic marine geophysics community.

Line 590: Uncertainty estimation: While it is true that uncertainty analysis has commonly been overlooked, it is becoming more and more common in recent times. Several schemes have been proposed and it is now routinely done in many travel-time tomography studies. As an example, the description of a formal Monte Carlo sampling scheme-based analysis can be found in Korenaga & Sager (2012). I'd say that the actual situation deserves a reference in this section.

Korenaga, J. & Sager, W.W., 2012. Seismic tomography of Shatsky Rise by adaptive importance sampling, J. geophys. Res., 117, B08102, doi:10.1029/2012JB009248.

Thank you for mentioning this reference. We extend the Uncertainty estimation section in the updated manuscript.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-240, 2020.