Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-240-RC1, 2020

Geoscientific Model Development Discussions
Open Access

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

Anonymous Referee #1

Received and published: 27 November 2020

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 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.

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.

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

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

Line 110: Exploration of aternative 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 Line 134: "of subduction zones"

Line 135: "As an experimental"

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?

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

Line 154: "as follows"

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

Line 166: "was designed"

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

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

Line 241: "of another"

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

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

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

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

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

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

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 slighlty modified versions of the conversion laws in different units/sectors, too? Would this have any effect at all on FWI?

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

Line 366: thermal effects, too?

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?

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?

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.

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

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.

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.

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?

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 ne 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.

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