Dear Editor and Reviewers,

We would like to sincerely thank you for the time and effort dedicated to evaluating our manuscript and for the detailed and constructive comments. Their feedback has significantly helped us improve the clarity, structure, and overall quality of the manuscript.

Our revision has primarily focused on addressing the main points raised by the reviewers, which can be summarized as follows:

- Clarification of the distinction between the 2D geodynamic modelling and the 3D seismological forward modelling framework;
- Reduction and restructuring of sections that overlapped with our previous publication (Rappisi et al., 2024);
- Improved clarity and consistency in terminology, particularly regarding the use of terms such as "realistic", "validation", and "3-D geodynamic model";
- Clarification of the workflow from geodynamic simulations to LPO-derived elastic tensors and synthetic waveform generation;
- Improved referencing and integration of figures within the main text;
- Extension of the limitations section, including a more explicit discussion of waveform duration and associated implications for observable seismic phases.

Below, we provide a detailed, point-by-point response to all comments raised by the Editor and the Reviewers. For Reviewer A, we explicitly address the comments highlighted in red in the annotated PDF, which correspond to the substantive scientific and methodological suggestions. The comments highlighted in green primarily concern minor grammatical and stylistic corrections; these have all been fully incorporated into the revised manuscript without individual itemized responses, and we gratefully acknowledge their value in improving the readability of the manuscript.

We now proceed with the detailed responses to each comment.

Francesco Rappisi,

On behalf of all authors

Reviewer A

This report presents a great deal of advanced work constructing geodynamic models that incorporate anisotropic structure with robust seismogram modeling. The approach of using geodynamic models and synthetic seismograms to validate the resolution of seismic tomography is unique and well done. Since a lot of the technical detail about the modeling was presented in Rappisi et al., (2024), the only substantial comment I have about the report is the limitations section. I think the points presented there could be expanded upon and can point readers to relevant citations, especially if others wanted to modify SPECFEM to include/improve upon those parameters.

The pdf attached includes comments, minor grammatical fixes, and sentence restructuring suggestions. Green indicates grammer and wording suggestions. Red means areas the need further explanation or citation suggestions.

Recommendation: Accept Submission

(Red comments from the pdf ⬇)

1) Line 84: Adding a sentence saying that structure in the Y direction is uniform or something would be beneficial

   We have clarified that the model is invariant along the latitudinal (Y) direction (lines 91-97). Furthermore, similar clarification sentences have been added through the text (e.g., lines 143-149, 167-175), and “3-D” has been removed from the title.

2) Line 94: Is this consistent with what we know about subduction initiation? Would be nice to have a citation supporting this parameter choice...

   We agree that the choice of a pre-defined dipping weak zone should be better justified and supported by appropriate references. We have revised the manuscript to clarify this point and added relevant citations. In particular, we now refer to Toth & Gurnis (1998), who demonstrate through numerical modelling that subduction can initiate along pre-existing weak zones in the lithosphere, especially when they are favorably oriented. Their results also highlight the role of the dip angle in controlling the evolution of the system. We also refer to Baes et al. (2011), which show that natural tectonic settings (e.g., STEP faults) can lead to the development of dipping shear zones that localize deformation and facilitate subduction initiation. We believe that these additions provide both a numerical and a physical basis for the introduction of a 30deg dipping weak zone in our model. See lines 102-113.

3) Line 129: repeating sentiment. condense?

This part has been rephrased and redundancies have been reduced here and through the text.

4) Line 138: Once again, having a sentence to emphasize the uniformity in the y-direction. There was a similar sentence in Rappisi et al., 2024 addressing the intentional omission of azimuthal anisotropy, which also feels relevant here.

   While this aspect was already described in the first paragraph of the *Geodynamic Model* section (see comment 1), we agree that it should be reiterated here for clarity.

5) Line 144: Worth including the range of teleseismic distances used for reference.

   Teleseismic distance ranges included (line 153).

6) Line 149: Would be nice to include the number of total stations to quickly assess the coverage of the synthetic array relative to a more realistic seismic deployment.

   We have revised the manuscript to explicitly include the total number of seismic stations (1302) in the description of the array geometry (line 151).

7) Line 234: Wording could change to help readers remember this is a 2D model that may or may not accurate represent true anisotropic structure, but provides an additional scenario that is consistent with observational studies.

   We have revised the sentence to clarify that the results are obtained within a controlled synthetic framework based on a 2D geodynamic model extended into a 3D representation, to avoid implying a direct representation of true Earth structure while preserving its value as a benchmarking scenario (lines 254-257).

8) Line 248: Citation suggestions that investigate the effect of anelastic structure on surface wave amplitudes (since the minimum period makes those the most applicable) [Ruan & Zhao 2010; Ruan & Zhao 2011; Zhou 2009; Savage et al., 2010].

   We have revised the *Limitations* (lines 268-284) section to include the suggested references and to better support the discussion of neglected physical processes. In particular, we now cite studies addressing (i) the effects of seismic attenuation on surface-wave amplitudes (Ruan & Zhao, 2010, 2012; Savage et al., 2010), (ii) ocean-bottom related effects (Janiszewski et al., 2023), and (iii) scattering from small-scale heterogeneities (Shearer, 2015; Spetzler et al., 2002).

9) Line 249: Issues like tilt and compliance noise which are a function of ocean topography and infragravity waves on the ocean floor should be cited here. [Janiszewski et al., 2022]

See comment 8

10) Line 250: Citation suggestions to address stochastic noise and elastic focusing effects. [Shearer 2015; Spetzler et al., 2022]

See comment 8

Reviewer B

General comments:

In their manuscript, Rappisi and co-authors present a 2D geodynamic model of ridge-to-slab evolution. Based on this geodynamic model, they simulate cpo/lpo evolution. The resulting density and elastic tensor fields are then reflected and replicated to construct a three-dimensional domain. For this domain, they generate approximately 50,000 synthetic seismograms recorded at more than 1,300 virtual stations. The main objective of the study is to provide these results to the geophysical and seismological community as a reference scenario for benchmarking, for example, seismic tomography codes.

Title: I suggest avoiding the term "3-D" in the title, as both the geodynamic modelling and the cpo/lpo evolution computations were performed in 2D. Referring to the model as "3-D" could therefore be misleading for readers.

We have revised the title to remove the term "3-D" in order to avoid potential ambiguity, as the underlying geodynamic model and LPO computations are based on a 2-D framework extended into a 3-D domain for seismic modeling.

Abstract: The abstract is well written and provides an overview of the methodology used to create the dataset. However, it could be improved by better highlighting the significance of the dataset and providing a more detailed explanation of its potential applications (benchmarking seismic tomography codes, …).

We have revised the abstract to remove redundant statements regarding the benchmarking purpose of the dataset in the opening sentence and to improve the overall flow. The intended applications are now described more explicitly in a single dedicated sentence, avoiding repetition and clarifying the broad scope of potential uses.

Introduction: The introduction provides a clear overview of the motivation and context of the study. However, some statements appear somewhat optimistic in their wording and may overstate the contribution of the work. I recommend revising the language to ensure that the scope and significance of the study are described in a balanced and precise manner. I would also like to encourage the authors to discuss recent studies, such as Kruse et al. (2024), which follow a comparable workflow.

We have revised the *Introduction* to moderate several statements that were previously phrased in a more assertive manner. In particular, we softened claims regarding the impact of anisotropy in benchmark models and the general implications for inversion strategies, to ensure a more balanced and precise formulation.
In addition, we have added a discussion of Kruse et al. (2024), which we agree represents a highly relevant and closely related study. Their work combines geodynamic modeling with anisotropy and synthetic waveform analysis in complex tectonic settings, and we now explicitly acknowledge this connection in the Introduction (lines 55-65).

2 DATA DESCRIPTION AND DEVELOPMENT

2.1 Geodynamic Model
In this section, the authors present parts of the methodological approach used for the 2D geodynamic model, together with some information on how the lpo computations were performed. In general, this section appears somewhat unstructured and largely reproduces content from Rappisi et al. (2024). I strongly recommend restructuring this section by removing redundant content and instead including information that is not provided in Rappisi et al. (2024).

Done, more details are in the comments below

2.2 Seismic Dataset
In this section, the authors provide a clear and detailed description of how the synthetic waveforms were computed.

3 DATA ACCESS
In this well-structured and concise section, the authors provide, to my understanding, all the information necessary to use their dataset.

4 DATA USAGE
In this well-structured section, the authors provide a clear overview of the previous use of the dataset presented here and also describe several other potentially important applications. In addition, they provide a clear explanation of the limitations of the seismological forward modelling.

Figures:

Two of the three figures (Figs. 2 and 3) are not referenced anywhere in the manuscript. Although their general context can be inferred, all figures should be properly introduced and discussed in the text.

All the figures are now referenced in the text and properly discussed (e.g., in section 2.2.1). See lines 152, 155, 190 and195.

Recommendation:

Altogether, the manuscript could be of great interest to the seismological community (particularly for seismic tomography) for benchmarking purposes and fits well within the scope of the journal. I recommend publication after the above (general) and below (detailed) comments have been addressed, which may require a moderate to major revision.

Detailed comments:

Abstract:

1) Line 13: The geodynamic model described here is two-dimensional rather than "three-dimensional". Referring to it as "three-dimensional" is therefore misleading and should be revised.

We have revised the manuscript accordingly by removing the term "3-D" from the tile, the abstract and throughout the text where it could be misleading. We now explicitly clarify, where appropriate, that the model is constructed from a 2-D geodynamic simulation replicated along the third dimension, resulting in a laterally homogeneous (pseudo-3D) structure. For example, see lines 91-97, 143-149, 167-175.

2) Line 22: What exactly do you mean by "geophysical inversion methodologies"? Please clarify.

We clarified this point in the abstract by explicitly listing representative applications (e.g., tomography, sensitivity analyses, and wave propagation studies). We also replaced the expression "geophysical inversion methodologies" with more general and precise wording to better reflect the broader range of potential uses of the dataset. See Abstract at lines 13-24.

Introduction:

3) Line 39: The term "simplified physical assumptions" is somewhat broad. If you are referring to the use of isotropic elastic properties (e.g., in benchmarks such as Marmousi), this could be stated more explicitly, as these models can still exhibit significant structural complexity.

We have clarified this point by explicitly referring to the common assumption of isotropic elastic properties, while acknowledging that many benchmark models still include significant structural complexity. Lines 39-41.

4) Lines 41 – 45: The statement that ignoring anisotropy in benchmarks limits their capacity to evaluate real-world inversion scenarios appears somewhat strong. Please consider qualifying this statement.

We have accordingly revised this part of the introduction by softening the original statement and adding quantitative evidence, for example, from previous studies that document the magnitude of errors associated with neglecting anisotropy (lines 43-51). Similar revisions have been consistently applied throughout the manuscript (e.g., Section 2.1) to ensure that all related claims are properly qualified and supported by the literature.

5) Lines 49 – 52: While I agree that the benchmarks that you discuss above are not derived from geodynamic models, the statement that they lack a geodynamically consistent context appears somewhat broad. Benchmarks, such as Marmousi and GO_3D_OBS, are designed to represent geologically plausible structures. The claim that this disconnect reduces their realism and capacity to reproduce complex structural and mechanical interactions seems somewhat strong. Please provide appropriate references to support these statements or consider rephrasing them.

We have softened the original statement by removing the implication that such benchmarks “lack” realism or structural complexity. Instead, we now distinguish more explicitly between benchmarks that are constructed to reproduce geologically plausible structures and those derived from fully geodynamic simulations, and we avoid wording that could be interpreted as a qualitative hierarchy between the two approaches. Lines 55-65.

6) Lines 52 – 53: Here, it is not clear whether the claimed “growing need” represents a broadly recognized requirement or primarily reflects the authors viewpoint. I therefore suggest rephrasing or clarifying this statement and providing references if available.

We have revised the sentence to avoid implying a specific community-wide consensus. Lines 55-65.

7) Line 56: I suggest not using the term “3-D geodynamic model” (please refer to my comment on the title or on line 80).

Done

8) Line 58: What do you mean by "realistic conditions"? Please clarify.

We have replaced “realistic conditions” with “physically consistent conditions” to avoid potential ambiguity. The revised wording is intended to emphasize that the setup is dynamically and physically coherent within the modeling framework (i.e., internally consistent geodynamic structure and physics), without implying that it represents a direct or fully realistic representation of Earth structure, as the model characteristics are already described in detail elsewhere in the manuscript (e.g., in section 2.1).

9) Line 60: I am somewhat confused by the use of the term “validation” throughout the manuscript (lines 22, 29, 34, 60 and 254). In light of, for example, Babuska & Oden (2004), please clarify in the manuscript – perhaps in Section 4 – why “validation” rather than “verification” is considered appropriate in this context.

We thank the reviewer for this insightful comment and for highlighting the potential ambiguity in the use of the term “validation” in light of Babuška & Oden (2004). To avoid any possible confusion, we have revised the manuscript and replaced the term “validation” with more precise terminology, including “benchmarking”, “testing”, and “evaluation”, depending on the context.

10) Line 63: Why do you consider the term “realistic” appropriate here? Please clarify, perhaps in Section 2, or consider rephrasing, for example to “valuable synthetic framework”.

We rephrased to ‘valuable synthetic framework’ (line 73).

11) Line 67: Please consider replacing the term “real-world example” with a more accurate description (see my previous comment on “realistic”).

We have replaced “real-world context” with a more precise description (lines 78-79).

12) Lines 69 and 73: Why do you consider the term “realistic” appropriate here? Please clarify, perhaps in Section 2, or consider rephrasing (see my previous comment on “realistic”).

We have rephrased this paragraph. In addition, all overly strong claims throughout the manuscript have been moderated and reformulated in a more balanced way, in line with this comment as well as the more general concerns raised above

13) Lines 69 – 70: Please rephrase this sentence. You present a single geodynamic model, not multiple “models”. I suggest something like:

“This work addresses the current lack of publicly available datasets of high-quality synthetic waveforms directly derived from a geodynamic model.”

We have rephrased as suggested (lines 80-81).

14) Lines 72 – 74: Please rephrase the sentence claiming that this is the first benchmark model to couple a dynamically consistent geodynamic simulation with realistic, LPO derived seismic anisotropy. This statement may be misleading, as other studies, such as Kruse et al. (2024) or Di Leo et al. (2014), have also combined geodynamic simulations with predictions of seismic anisotropy. Consider clarifying the contribution of your work in this context.

We have revised the statement to remove the implicit claim of novelty and now explicitly acknowledge previous studies (e.g., Di Leo et al., 2014; Kruse et al., 2024) that have similarly integrated geodynamic simulations with anisotropy and synthetic waveform analysis. E.g., see lines 55-65 and 81-85.

2 DATA DESCRIPTION AND DEVELOPMENT

2.1 Geodynamic Model

Line 80: Please avoid using the term "3-D geodynamic model". The numerical modelling was performed in 2D and extending it into a third dimension does not make it fully 3D. A truly 3D geodynamic model would, for example, be able to self-consistently develop features such as toroidal flow, which is not possible in a 2D model. Referring to the model as "3-D geodynamic model" is therefore misleading.

The term "3-D geodynamic model" has been removed.

Lines 80 – 88: To my understanding, the lpo computations with D-Rex were performed based on the velocity and velocity gradient tensor fields of the original 2D model setup (40°x700 km, 1001x351 nodes). While I understand that the density and elastic tensor fields need to be reflected and replicated to construct a 3D domain for the seismological forward modelling, it is not clear why this procedure is described here in the context of the geodynamic model. This description may give the impression that the geodynamic model itself is 3D, which is not the case. I suggest moving this part to Section 2.2.1 where these quantities are used or otherwise clarifying its role in the current context.

We agree that the original wording could give the misleading impression that the geodynamic simulation itself was fully 3-D. We have clarified that both the geodynamic and LPO calculations were performed on a 2-D ridge-to-slab section, while the replication along the third dimension was introduced only afterwards to construct the pseudo-3D computational domain used for the seismic forward modelling. We moved this description to section 2.2.1 as suggested. See lines 167-183.

Lines 89 – 127: I appreciate the detailed description of the initial model setup, the constitutive relations and the material parameters. However, I recommend substantially shortening or removing this section, as it largely reproduces content from your previous work. You could instead include a brief statement such as:

"Detailed information on the initial model setup, constitutive relations, and material parameters can be found in Rappisi et al. (2024), and only a concise summary is provided here."

In addition, I suggest addressing the following questions here:

Where on Earth can such a geodynamic setting be found?
What are the key features of the geodynamic model (e.g. regions of significant deformation, potential upwelling of the transition zone, …)?
Why is it reasonable, for this setting, to neglect 3D features such as toroidal flow and replicate a 2D model into the third dimension?
Which aspects of your model are "realistic" (please refer to my comments on the Introduction), and what are the general limitations of your geodynamic model?

Following this comment, we substantially revised the description of the geodynamic setup. We now explicitly state that the geodynamic and LPO simulations were performed in 2-D, while the final seismic domain was constructed through replication into a pseudo-3D geometry for forward waveform modeling. We clarified the purpose and limitations of this approximation, including the intentional neglect of the heterogeneity in the latitudinal direction and fully 3-D flow components (e.g., toroidal mantle circulation), in order to provide a controlled framework for isolating first-order effects of slab geometry, mantle deformation, and anisotropy on seismic wave propagation.

We also added text clarifying that the model is not intended to reproduce a specific tectonic region, but rather an idealized yet geodynamically plausible ridge–subduction system (lines 143-149). In addition, we now highlight several first-order features reproduced by the model (e.g., mantle upwelling beneath the ridge, strong deformation in mantle wedge and sub-slab regions, and strain-induced seismic anisotropy), while also explicitly stating the main simplifications and limitations of the framework.

Finally, we reduced redundancy with our previous work by shortening parts of the methodological description and referring readers to Rappisi et al. (2024) for full details on the constitutive relations and rheological parameters (see Section 2.1).

Line 128: What do you mean by "models"? Please clarify.

We replaced "*models*" with as "*simulations*"

Line 129: What do you mean by “realistic”? Please clarify.

We replaced *“realistic”* with *“LPO-derived”*

Lines 129 – 131: Why is it considered “critical for evaluating inversion sensitivity and imaging artifacts” to account for seismic anisotropy derived from geodynamic models? Please clarify.

We have clarified the statement by adding a supporting sentence with appropriate references demonstrating that seismic anisotropy derived from geodynamic/LPO models has a first-order effect on seismic observables and can significantly influence inversion results and imaging artifacts if neglected (lines 129-134).

Lines 131 – 132: “… with a regular grid spacing of 5 km.” Do you refer here to the initial 2D model setup? Please clarify.

We clarified that the 5 km grid spacing refers to the original 2-D geodynamic model setup (lines 135-136).

Lines 135 – 138: This section is generally difficult to follow. In particular, the reported final Eulerian domain (80°x700 kmx40°) appears inconsistent with the description of the replication along latitude from -40° to +40°. Please clarify the actual spatial extent of the domain and the number of grid points in each direction and consider rephrasing the paragraph to make the workflow more transparent. In addition, as noted in my comment on lines 80 – 88, I suggest moving this part – after appropriate revision – to Section 2.2.1. Instead, please provide here the set of D-Rex parameters (including nCRSS) used for the lpo computations and briefly explain why these parameters were chosen. In addition, please clarify here if the transition zone is assumed to be isotropic or anisotropic.

In the revised manuscript, we have added a concise description of the D-Rex parameterization used for the LPO computations. We moved this part to section 2.2.1 and made it clearer. We clearly state that the model is anisotropic down to 700 km depth (lines 135-142).

Figure 2: Please clarify in the manuscript how and under which assumptions Vsh, Vsv, Vph, and Vpv were estimated. In addition, I suggest presenting only the (reflected) 2D results rather than the replicated third dimension, as the latter may give a misleading impression of the model. Furthermore, the figure is not referenced anywhere in the manuscript, and this should be corrected.

We have clarified in the manuscript (when introducing Fig. 2) how Vsh, Vsv, Vph, and Vpv are derived from the elastic tensor, together with the definition of the anisotropy parameter η. The text now explicitly states the assumptions used to compute these quantities without introducing additional formulas (lines 184-196).

2.2 Seismic Dataset

Line 144: “and” should be removed here.

Removed

Lines 146 – 148: It is unclear whether the domain extension fully resolves the issue of events located outside the original model. Please clarify.

Clarified

2.2.1 Seismological Forward Modeling

Line 164: While SEM may be capable of simulating “realistic 3-D media”, please avoid referring to the SubRidge model as “realistic” without also discussing the limitations of both the geodynamic model and the D-Rex model.

In the revised manuscript, we have avoided the use of the term “*realistic*” in reference to the SubRidge model, both in this section and throughout the manuscript. Following this and related comments, we also clarified more explicitly the scope of the model, the geodynamic features it is intended to reproduce, and its main simplifications and limitations.

Lines 175 – 198: A substantial part of this section appears to be nearly identical to the supplementary material of Rappisi et al. (2024) but it is not explicitly referenced. This redundancy can be confusing for readers. I strongly recommend either citing the original source or removing the duplicated content.

In the revised manuscript, we explicitly clarified that the seismic forward-modeling workflow follows the implementation presented in Rappisi et al., 2024. We retained only a concise methodological summary (lines 201-202).

Figure 3: This figure is never referenced in the manuscript. This should be corrected. In addition, it is not clear in the bottom panel what is being shown (viscosity field? density field?). Please clarify.

The figure is now referenced in the manuscript. We clarified that the lower panel shows the source–receiver geometry together with an illustrative slice of isotropic shear-wave velocity (Vs) extracted from the geodynamic model.

3 DATA ACCESS

Lines 208 – 210: Please specify the unit of the elastic tensor components here.

Cij in GPa specified in the manuscript (line 229).

4 DATA USAGE

Line 236: Why is the “viscosity structure” mentioned here? To my understanding, the viscosity field is not part of the dataset. Please clarify.

Corrected

Editor Suggestions

1) Figure 3: This figure could be improved by showing the full record section (all stations) for a single event instead of a single time series for one station. This way, the readers might be able to better identify the presence of their seismic phases of interest based on their moveout.

Fig. 3 has been replaced with a new version where we also show a set of seismograms generated by the same event (panel b). We decided not to plot all the ~1000 traces as it would make the figure too complex.

2) Line 216: "however they have been created mimic similar events" grammar error: correct to"however, they have been created to mimic similar events"

Corrected

3) Regarding Figure 2, please clarify (either in the caption or in the main text) how the 'isotropic' velocities are being defined. In Rappisi et al., 2024, the isotropic wave speed is defined as half the sum of Vsh^2 and Vsv^2. However, many readers will assume that 'isotropic' refers to the Voigt average, which does not have an equal weighting between Vsv and Vsh.

Now clarified in the main text (section 2.2.1, when introducing Fig. 2). See lines 184-196.

4) As Reviewer 1 points out, it is important to clarify the (max/min) epicentral distance range of the receivers.
At some epicentral distances, certain phases may not be observable within the 1-hr length of the records that are provided (e.g. fundamental-mode surface waves at some periods may arrive later than 1 hr after the origin time at epicentral distances > 110 deg or so).

Therefore, the authors should consider acknowledging (in the limitations section) that the 1-hr length of the time-series will not allow analyses involving major-arc surface waves or free oscillations.

We have now explicitly stated in the manuscript the maximum epicentral distance covered by the simulations (line 153). In addition, we have added a sentence in the Limitations section addressing the constraint imposed by the 1-hour record length (lines 276-280).

5) Line 146: "As shown in Fig. 1, 8 events fall at the edges or out of the 80°x40° geodynamic model, however we note that before..." You can split this into two sentences -> "As shown in Fig. 1, 8 events fall at the edges or out of the 80°x40° geodynamic model. However, we note that before..."

Done

6) Line 193: "The final spectral-element domain is $(\Phi,R,\Theta) = (80° \times 6371 \text{ km} \times 80°)$, with $n\phi = 384$ and $n\theta = 192$, hence horizontal resolution of ~0.2° and ~0.4° along longitude and latitude, respectively." This sentence can be rephrased: "The final spectral-element domain is $(\Phi,R,\Theta) = (80° \times 6371 \text{ km} \times 80°)$, with $n\phi = 384$ and $n\theta = 192$, resulting in a horizontal resolution of ~0.2° and ~0.4° along longitude and latitude, respectively."

Rephrased as suggested

7) Line 198: "We use SEM waveforms in SAC format (Goldstein and Snoke, 2005)." -> "We store the SEM waveforms in SAC format (Goldstein and Snoke, 2005)."

Done

8) This is not essential for publication, but since the authors already include the STATIONS and SOURCES files, they could also consider including the SPECFEM 'Par' files in the data repository

We thank the Editor for this suggestion. The Par files are indeed available within our code repository and correspond to standard configurations commonly used in SPECFEM3D workflows. Since they do not contain any case-specific modifications, we believe that the current documentation is sufficient to fully reproduce the simulations. For this reason, we would prefer not to modify the current data repository structure.

9) Line 209: "elastic tensors are oriented according to a local (or radial) Cartesian reference system where x axis points East" -> "elastic tensors are oriented according to a local (or radial) Cartesian reference system where the x axis points East" (missing 'the')

Done