Dear reviewers,

We would like to sincerely thank both reviewers for the time and effort dedicated to evaluating our manuscript. We were somewhat surprised that both reviewers seemed unaware of the type of submission they were evaluating, as our manuscript was submitted as a Technical Paper, not a regular Research Article. We, however, considered and answered their detailed comments accordingly, which still greatly helped us to improve the clarity and relevance of our work.

We summarize here the main changes of the article:

- In response to Reviewer C's suggestion, we have modified the title of the article to better reflect its content and purpose
- The abstract has been rewritten to more clearly state the context and major contributions of the paper to convince people that our work brings something new and interesting, even if it is not a scientific paper.
- The introduction has been revised (**lines 34-69**) to better highlight both the need for large-scale meshes for modeling seismic cycle deformation and the current lack of such meshes in the literature.
- **Section 5** has been completely rewritten to take into account the change in the viscosity of the asthenosphere suggested by Reviewer C.
- The discussion has been largely expanded the emphasize the importance of using a suitably large mesh, at the scale of planet Earth and its mantle **(Sect 7.1)**, the interseismic modeling **(Sect. 7.2)**, and the impact of the slab geometry **(part of Sect. 2, lines 136-146)**.

On behalf of my co-authors,
Best regards,

Hugo Boulze

Comments to Reviewer A

(1) Mesh generation is important in geodynamic modeling, and different groups use different methods to generate meshes. However, providing a mesh, at least as the main focus of a paper, is not necessary.

We disagree with this comment. Many studies using a mesh do not provide their mesh in the supplementary materials. Therefore, their results are not reproducible, and benchmarking (an important activity in numerical modelling) is impeded. In addition, building a geometrically exact mesh of large size and density where necessary is a complex and delicate undertaking, as we show in this paper. As a result, this type of calculation is largely beyond the reach of a community that lacks the expertise to create such a mesh, but has the knowledge and skills to use it. Making a mesh available fills this gap. We have added **Section 7.1**, in which we enumerate (non-exhaustive list) meshes that do not have the same spatial coverage as ours, and in what cases these meshes can be valid or not.

(2) The manuscript reads more like a technical report than a scientific paper. I appreciate the detailed notes the authors have summarized in the manuscript, but these should already be well understood by most modelers. The notes are useful mainly in cases where incorrect use of conditions leads to misinterpretation

We don't understand this comment. The paper has precisely been submitted as a technical report to Seismica. Is it possible that the reviewer was unaware of this fact? Please, see Seismica guideline:

Software Reports: *The goals of Software Reports in Seismica are to document new codes, to facilitate community use of them, and to ensure reproducibility of their outputs. Software Reports should include a main paper, plus a user manual and source code that should be uploaded to a public domain repository. The main paper should describe the scientific context, the methods employed, and detail aspects such as test case simulations, model verification, evaluation and performance. Including the examples and test cases mentioned in the main paper as tutorials within the repository is strongly recommended. All code repositories must be privately accessible by the editors and reviewers upon submission, publicly accessible upon acceptance, and the codes included are subject to peer review. See Availability of data, materials, and code for more information.*

(3) The authors used slab 2.0 to generate the slab interface. This is a good way. But I see no improvement in this usage compared to previously published models.

We don't understand this comment. The purpose of our work is to build and share a suitable high-performance mesh for modeling the seismic cycle in a subduction context. A specific slab geometry is required for this, and we chose a widely used, standard model (Slab2.0) to ensure robustness and reproducibility, independent of other modeling choices and large-scale availability of the model (which is not the case for other local geometries published). Creating a new slab model is a completely different field of research, relying on techniques which are not used here (for ex., seismicity), and our goal was to use the most recent large-scale geometry available. We would be interested if the reviewer could cite comparable meshes at this scale.

(4) The authors defined their own rheological structure. Based on published results on data and modeling, the validity of the rheological structure defined in the manuscript itself seems to be a question to explore.

We don't understand this comment. We do not propose a specific rheological structure at this point; it can be fully tuned and explored, and will be the subject of future publications. We are also exploring them in the frame of future *scientific studies,* which our group is currently working on (refs 1,2,3,4), relying on this 3D mesh.

Comments to Reviewer C

General Comments/Suggestions

G1. General description of the paper provided by the reviewer. No answers expected.

G2. The model uses Slab 2.0 (Hayes et al., 2018), that is a well-accepted subduction geometrical model. However, such a model has higher uncertainty in the geometry of the slab at the shallower portions of the Nz-SoAm megathrust , as well as the deeper ones due to lower seismicity in those regions. Such uncertainties need to be addressed in the manuscript (as part of the discussion), as these are relevant when building a subduction model to relate megathrust slip behavior and surface displacements (see Moreno et al., 2009, as well as Ragon et al. 2019 and references therein).

See our response to G3.

G3. Regarding the previous point, the authors chose Slab2.0 as the geometric model to construct the FEM presented in the manuscript. For the subduction of the Nazca plate beneath the South American plate, Slab2.0 is primarily constrained by seismicity and trench location. As a result, the geometry is relatively well resolved beneath the continental margin and in offshore regions close to the coastline, but it becomes progressively less reliable in offshore areas approaching the trench, where seismicity is sparse. While Slab2.0 was the de facto standard at the time this FEM was being developed, a more recent and improved geometric model of the megathrust interface (NSAM) was published by Contreras-Reyes et al. (2025). Although NSAM is limited to the offshore region, it is primarily constrained by active-source seismic data, offering significantly higher resolution near the trench. I acknowledge the substantial effort invested in constructing the FEM model, and I am not suggesting that the authors incorporate NSAM into the current version. However, given the open-source and evolving nature of the FEM, the manuscript should include a discussion comparing Slab2.0 and NSAM, and ideally noting that NSAM became available when the FEM was ready for submission. Such discussion very relevant (see previous paragraph) as the new study shows important discrepancies with Slab 2.0.

We agree that certain regions of the Slab2.0 model are poorly constrained, particularly where seismicity is sparse. More recent and refined slab geometry models, such as the one mentioned by the reviewer, indeed represent significant improvements. However, these improvements primarily affect the shallower portion of the slab — within the first 50–100 km landward of the trench — corresponding approximately to depths shallower than 40–50 km. To assess the potential impact of such differences relative to Slab2.0, we conducted a sensitivity test in which we introduced a 10 km variation in slab topography between the trench and 40 km depth. The results of this test show a negligible impact on large-scale continental deformation. Detailed outcomes of this sensitivity analysis are presented in the Supplementary Materials **(Fig. S10)** and discussed in **Section 2 (lines 136–146)**. In line with the reviewer's related comment regarding the definition of the trench below, we acknowledge that differences in trench geometry may influence offshore deformation, particularly near the trench. However, such differences have only a limited impact on the continental deformation that is the focus of this study.

G4. While the technical aspects of the publication are strong, the scientific claims about the usability and predictability of the FEM mesh needs further improvements and clarifications. As an example, it needs a more thorough discussion of the impact on model predictions of the needed simplifications of the model. For instance, a more thorough validation against observed GNSS positional time series. As some of the co-authors have done work with post-seismic GNSS time series in Chilean earthquakes (e.g.,Klein et al, 2016), I guess it is not too difficult to make a straight comparison of the characteristics of post-seismic positional time series that are dominated by viscoelastic deformation (beyond comparing co- to post-seismic displacement ratios in a given/fixed time window). Note that I do not expect things to "match perfectly" because of data uncertainty, influence of megathrust after-slip, and the fact that it will be a comparison of a forward model against data (not the prediction of an inversion). But for material properties used in previous studies, the predictions should be "fairly similar". For instance, for sites far enough from coastline (thus, less influenced by afterslip) such comparison can be made and perhaps compare the results with several values of the relevant parameters (e.g., viscosity). For sites closer to after-slip influence, perhaps model predictions can be compared to deviations from previously estimated secular linear trends after a relocking or reloading has occurred (and the influence of afterslip is minimal).

We don't understand this comment. While we genuinely appreciate the remark that "the technical aspects of the publication are strong," we do not fully grasp the request for comparison with GNSS observations. First, this work was submitted to the Technical Report section of Seismica. Second, tuning parameters to fit observations is a delicate and complex task. Often, the pursuit of a "best fit" and the discussion of the scientific implications of how that fit was achieved tend to overshadow important technical considerations. Our aim here is precisely to bring attention to those technical aspects — such as mesh size or depth — which are often treated as minor details but are, in our view, critical in many studies. The goal is to establish a robust technical framework on which solid scientific analysis can later be built and published in appropriate journals. One point on which we do fully agree is the feasibility and value of benchmarking. We would very much like to move in that direction in collaboration with the various groups working with FEM. In fact, we believe that sharing a common mesh would be an excellent starting point to enable just that.

G5. The authors need to conciliate the manuscript title/abstract with its actual content. While title and abstract suggest a tool for viscoelastic modeling throughout the seismic cycle, the actual implementation discussed in the manuscript only entails co- and post-seismic modeling. Therefore, either the contents of the manuscript need to be expanded to explicitly address inter-seismic processes (with appropriate examples for validation), or the title /abstract/discussion/conclusions should be revised to reflect the actual content of the manuscript. It is true, that some mentions are made to other studies that include inter-seismic processes, but this is not enough given the claims made in the title/abstract/conclusions.

We disagree with this comment, though we understand the source of the misunderstanding. In this paper, we have demonstrated that Chile_Mesh_v1.0 is suitable for modeling both coseismic and postseismic deformation. While interseismic deformation is not explicitly addressed here — as it remains an active area of research requiring a dedicated study (refs 1,2,3) — the mesh can also be used for interseismic modeling.

We acknowledge that interseismic modeling involves specific challenges and assumptions, particularly concerning the rheological behavior of the asthenosphere, and whether elastic or viscoelastic rheologies should be used. These questions lie outside the scope of the present study. Nevertheless, regardless of the chosen rheology, the computation of interseismic deformation essentially relies on evaluating the response to a unit slip along the plate interface and applying the backslip formulation (Savage_1983). To clarify this point, we have added a new **Section 7.2** discussing the use of Chile_Mesh_v1.0 for interseismic modeling. We have also modified the paper's title to better reflect the broader applicability of the mesh.

G6. The authors need to improve the discussion comparing their finite element model with previously available ones in the area. Although there are no FEM meshes with the extent of this model, it seems the article needs a more thorough comparison in terms of the advantages and disadvantages of local v/s regional models. Some of these points are already tackled but are kind of scattered across the manuscript. This kind of discussion is important to clarifying the scope of the proposed novel FEM.

We agree with this comment. We have created **Section 7.1,** which regroups the previously scattered comments on this topic.

G7. The authors point to a Github repository to make accessible the codes related to the finite element mesh being published. However, Github repositories are not persistent repositories. Being this a technical report in which a FEM and related tools are published, I strongly recommend that authors archive their codes (and examples with PyLith) in a permanent repository such as Zenodo, which admits versioning, as well as synchronization of the versions with releases made at the Github repository. This step is essential to ensure long-term accessibility and reproducibility of the published resources, which are core principles of FAIR data policies.

We fully agree with this comment. Here is the DOI provided to our GitHub using Zenodo: https://doi.org/10.5281/zenodo.15858559. We provide a review version. A 1.0 will be released when the paper is published. The paper has been modified accordingly.

Point-to-point comments and suggestions:

Lines 49 to 52. Please add Hormazabal et al (2023) to the list of references of works that have developed Finite Element Meshes to study post-seismic deformation induced by viscoelastic relaxation for various megathrust earthquakes in Chile. Hormazabal et al (2023) does so when analyzing post-seismic deformation of the 2015 (Mw 8.3) Illapel earthquake

Thanks for the suggestion. Done **(line 39)**.

Lines 63-64: Please clarify (with the appropriate test/example) if the FEM mesh can be used for inter-seismic modelling, and how such modelling will be implemented. (see comment G.5.)

Thanks for the suggestion. Please see our response to comment G5 and the newly added **Section 7.2** of the manuscript.

Lines 80 – 83: What about comparing with other definitions of the trench? (e.g. P. Bird)

The response is quite the same as G3: it certainly has an impact on offshore data, close to the trench, but a limited impact on continental deformation (see test of a 10km variation of the slab depth topography in **Fig. S10 and lines 136-146)**.

Lines 87-89: How such discrepancies affect the model predictions?. In the manuscript says "the interpretation of the model predictions must be made with caution". Can you please be more specific? Is the model useful in such scenarios? A more thorough discussion must be made regarding this warning, especially considering the use of land-based geodetic observations (e.g., GNSS, InSAR) and seafloor observations that may be available in the future (e.g., Absolute Pressure Gauges -APG-, Acoustic GNSS, and possibly Distributed Acoustic Sensing (DAS) observations in the future.

Please see our response to G3.

Lines 107-113: Please see comment G3. Please also provide a citation for the 1960 earthquake.

Please see our response to G3. A citation has been added for the Valdivia earthquake **(lines 129-130)**.

Line 107, I suggest to change "it has a thickness of 50km" to "we assign it a thickness of 50 km".

Done **(lines 134-135)**.

Line 111, perhaps a figure may be referenced that shows all earthquakes referenced in the manuscript.

Thanks for the good suggestion. We have added Valdivia, Maule, Iquique and Illapel rupture extension on the map of **Fig 1**.

Lines 112-113. Please explain how the extrapolation was made.

Thanks for the good suggestion. We have added a sentence in the revised manuscript **(lines 132-135)**

Line 144: I suggest to delete the word "simply".

Done.

Line 187: "we then remesh all the geophysical zones". What kind of meshing algorithm was used?. What type of element?.

We use linear thetraedral elements: Only the size of the element changes. We use MMG software for this step (add in the manuscript - **line 220**).

Line 201: in the section Boundary Conditions, it is not clear how far the lateral boundaries of the mesh are from the usable portions of the mesh (those portions modeling geophysical processes). Is there padding? What is a characteristic size of the padding? please clarify.

The model domain is sufficiently large to study deformation associated with the seismic cycle at the scale of the South American continent. Outside the continent, the interpretability of the model is inherently limited due to the presence of the Pacific and Atlantic Oceans. In general, the domain size is selected so that deformation becomes negligible near the model boundaries. For the synthetic Mw9 event considered in the manuscript, displacements already decrease significantly well before reaching the domain limits (5500km from the trench at the latitude 36°S): the coseismic drops below 1 mm beyond 4800 km from the trench. In the postseismic phase at 5 yrs, horizontal velocities are below 1 mm/yr at 2800 km (3900 km at 95 yrs).

Line 207: please annotate the units of the density.

Done. **(line 241)**

Lines 214-227: Please be more specific that the FEM provided can represent the static slip distribution.

Done **(line 249)**

Line 264: Please change "fictive" by "synthetic".

Done **(line 311)**

Lines 286-288: Despite previous evidence for a viscosity of about 10^18 Pa*s, the examples in this study were carried out with a viscosity one order of magnitude greater. Why?, please justify your election, or present the results for both viscosity values.

We agree with this comment. As a technical paper, our viscosity model is not constrained by geodetic observations. That said, viscosities commonly used to explain postseismic deformation typically range from 10^{18} to 10^{19} Pa·s. While 10^{19} Pa·s is not unreasonable, especially for long-term deformation (ref 1,2 and 3), it is more commonly applied to timescales beyond 5 years. Given that lower viscosities (~10^{18} Pa·s) are more representative of short-term postseismic processes and are used in most studies cited in this paper, we now show results for 10^{18} Pa·s. **Figures 4,5,6,7,8,9,10 and Section 5** have been modified accordingly.

Important: this change in viscosity does not affect any conclusions of the paper since the choice of rheology affects the time-dependent evolution of velocity, but not the influence of the model on boundary conditions.

References

(1) Boulze, H., Fleitout, L., Klein, E., & Vigny, C. (2024, April). Decoding inter-seismic deformation: Insights from viscoelastic modeling. In *EGU General Assembly Conference Abstracts* (p. 2389).

(2) Boulze, H., Fleitout, L., Klein, E., Vigny, C., & Garaud, J. D. (2023, May). Modeling surface deformations during the seismic cycle along the Chilean subduction zone. In *EGU General Assembly Conference Abstracts* (pp. EGU-12158).

(3) Boulze, H. (2023). *Observation et modélisation du cycle sismique le long de l'interface de subduction chilienne* (Doctoral dissertation, Ecole normale supérieure PSL).

(4) Klein, E., Boulze, H., Vigny, C., Fleitout, L., & Garaud, J. D. (2023, May). Is the Pampean flat-slab responsible for the differences in post-seismic motions between Maule and Illapel earthquakes?. In *EGU General Assembly Conference Abstracts* (pp. EGU-7301).