Dear Reviewer #1;

Thank you very much for reviewing the manuscript. We sincerely appreciated your valuable comments and suggestions, which significantly improve the manuscript. We used blue colors in the revised manuscript to indicate where we made changes to comply with your comments. Thank you once again for contributing to the improvement of the manuscript.

Below, we respond point-by-point to your specific comments and indicate where changes have been implemented in the revised manuscript.

The paper introduces an innovative integration of multi-objective PSO and Pareto optimality for the joint inversion of MT and Rayleigh Wave Dispersion (RWD) data. The approach is rigorous, the synthetic tests are thorough, and the field application is well-motivated by the geotectonic context. However, several methodological, structural, and interpretative gaps (lacunae) diminish the clarity, reproducibility, and scientific robustness of the work. Addressing these issues will greatly enhance its publishability and reader understanding.

> **R:** We sincerely thank you for your thoughtful and constructive assessment of our manuscript. We appreciate your positive evaluation of the novelty and rigor of integrating multi-objective PSO and Pareto optimality for the joint inversion of magnetotelluric (MT) and Rayleigh wave dispersion (RWD) data, as well as your recognition of the synthetic tests and the motivation of the field application within the geotectonic framework.
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> We also acknowledge your important concern that several methodological, structural, and interpretative gaps may reduce clarity, reproducibility, and overall scientific robustness. We fully agree that addressing these lacunae is essential to strengthen the manuscript and improve its publishability. In the revised manuscript, we have substantially improved these sections to increase clarity, reproducibility, and the strength of the interpretation. Specifically, we have:
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> - Added a dedicated noise-sensitivity analysis with multiple noise levels, and discussed how noise affects stability, uncertainty, and the shift of the Pareto front.
> - Introduced a layering-sensitivity test by repeating the joint inversion under several layer-number configurations to demonstrate how the recovered models and misfit behavior change with discretization and clarifying how many layers are effectively resolvable.
> - Included a benchmarking subsection in which we compare the proposed Pareto–MOPSO results against a traditional derivative-based Gauss–Newton scheme under the same dataset and comparable stopping criteria, providing an additional validation and performance reference.
> - Updated and strengthened the all data-processing workflow by employing more recent and reliable processing procedures, and we clarified the corresponding processing steps in the manuscript to improve reproducibility.

- Expanded the uncertainty characterization beyond presenting selected posterior PDFs by reporting layer-wise P10–P90 percentile envelopes derived from the Pareto-optimal ensemble, which compactly summarizes uncertainty and non-uniqueness across all layers.
- We replaced compatible/incompatible with coupled/decoupled to better reflect the physical meaning of the joint inversion results. Because resistivity and shear-wave velocity have different sensitivities and no universal constitutive relationship, the term compatible may suggest a stronger one-to-one correspondence than is warranted. In contrast, coupled more appropriately describes intervals where both datasets show structurally coherent behavior within the same layer, whereas decoupled refers to intervals where their responses diverge or are controlled by different sensitivities. We believe this terminology better reflects the physical meaning of the joint inversion results and avoids overinterpretation of the relationship between the two properties.
- Extended the synthetic test design by adding a second decoupled scenario: in addition to the original case (seismic velocity being insensitive to low resistivity), we now include the complementary case where the resistivity model is insensitive to low seismic velocities, thereby providing a more balanced and comprehensive assessment of decoupled sensitivities.

We sincerely thank you for the constructive comments, which have led to substantial improvements throughout the manuscript. The revised version has been comprehensively strengthened, not only through point-by-point corrections but also through broader revisions to the terminology, interpretation, and overall framing of the study. In this sense, we believe the manuscript has evolved into a more coherent and significantly improved version, and we hope it will be evaluated in its fully revised form. We also believe these revisions will substantially improve the manuscript and address your concerns in a clear, transparent, and reproducible manner.

1. The paper references previous work using GA, cross-gradient, and petrophysical coupling. However, it does not quantitatively compare the proposed method with existing Pareto-GA techniques (e.g., Moorkamp et al. 2010), standard cross-gradient structural coupling, or traditional nonlinear inversion methods (Occam, Gauss–Newton). Without such benchmarks, it is difficult to evaluate the extent of the improvement the method offers. The authors could conduct at least one comparative test using a conventional inversion scheme to demonstrate computational or accuracy benefits.

R: Thank you for this important point. We agree that the advantages of the proposed multi-objective Pareto approach should be demonstrated relative to established inversion strategies. We have therefore added a new subsection (7.3) to present the benchmark comparison using a conventional deterministic nonlinear inversion of Gauss-Newton (GN) scheme, and we now quantify both accuracy and computational cost on the same synthetic model (coupled case). Please see the new subsection.

Regarding cross-gradient coupling: cross-gradient constraints are formulated for 2D/3D structural similarity (based on spatial gradient cross products) and in a purely 1D setting the cross-gradient term becomes degenerate (does not provide a meaningful coupling measure) as we already mentioned in lines 51-53. We therefore focus on benchmarks that are well-defined in deterministic Gauss–Newton-type inversion.

2. In the synthetic results, especially Figures 6 and 8, seismic velocity is shown to be highly sensitive to noise, causing a shift of the Pareto front toward the MT axis. However, the work does not quantify noise thresholds at which model instability occurs, provide uncertainty analysis beyond posterior PDFs, or discuss robustness relative to the SNR typical in real MT/RWD surveys. It would be helpful if a noise-sensitivity analysis with multiple SNR levels could examine how stability depends on noise distribution.

R: Thank you for this important comment. We agree that the manuscript should more clearly demonstrate how the joint inversion stability depends on noise level and noise characteristics. In the revised manuscript, we have therefore added a dedicated subsection on noise-sensitivity analysis relating the tested noise levels (5, 10, 20, 40) (Section 7.1.2). In this new section, we repeat the synthetic joint inversions under multiple noise levels and explicitly discuss how increasing noise shifts the Pareto front and affects the stability of the recovered models by reporting ensemble-based uncertainty measures (e.g., layer-wise variability using percentile envelopes such as P10–P90).

We believe these additions provide a clearer, quantitative view of noise-driven instability and improve the robustness discussion in the revised manuscript.

3. The MT data reveal tensor skewness and non-1D features (page 17, Figure 10), and the geology is complex and heterogeneous. However, the paper relies solely on 1-D inversion for both MT and RWD. This could lead to misinterpretation because MT responses sometimes reflect 2D or 3D structures, and RWD curves across multiple paths naturally incorporate 2D structural heterogeneity. The joint inversion presumes a co-located subsurface structure, which is unlikely in tectonically complex regions. Please provide a more substantial justification for using 1-D modeling or include a preliminary 2D MT inversion or phase tensor analysis to demonstrate lateral homogeneity along the path.

R: Thank you for raising this important concern. We agree that the MT dataset indicates non-1D behaviour (e.g., tensor skewness in Fig. 10) and that the geology is heterogeneous, such that both MT responses and RWD can be influenced by lateral variations. We therefore emphasize that our goal is not to claim that the Earth beneath the study area is strictly 1-D. Rather, our objective is to recover an effective 1-D (laterally averaged) structure that is appropriate for (i) the available station geometry and (ii) the inherently path-averaged nature of RWD data.

To strengthen the justification and reduce the risk of misinterpretation, we have revised the manuscript in two key ways:

1. Explicitly framing the 1-D models as “effective” averages.

We now clarify that the inverted 1-D MT resistivity model represents an effective, laterally averaged response around the site, and that the 1-D Vs model represents an effective path/Fresnel-zone average rather than a strictly co-located point model. This wording is now stated clearly in the Methods and reiterated in the interpretation.

2. Restricting the inversion to a period band with reduced high-dimensional influence based on phase-tensor diagnostics.

Following re-processing of the MT data and additional phase-tensor evaluation (as also suggested by the reviewers), we find that the responses begin to show clear high-dimensional effects already at periods of approximately $T \gtrsim 1$s, not only at $T \gtrsim 10$ s described in previous version of the manuscript. To avoid fitting strongly hig-dimensional contaminated data with a 1-D model, we therefore limit the MT inversion to periods ~ $T \leq 1$ s, where the phase-tensor diagnostics indicate more quasi-1D/2D behaviour and where MT and RWD are most compatible for joint interpretation.

In addition, we clarify how the common depth sensitivity is established: the joint interpretation depth range is guided by the MT skin-depth considerations over the retained period band and by the sensitivity of the Rayleigh-wave dispersion curve to crustal depths over the corresponding period range. We believe these revisions provide a more defensible and transparent rationale for using 1-D joint inversion in this tectonically complex setting, while clearly stating the limitations and the effective/averaged meaning of the resulting models. Please see the revised sentences in the section “7.4 Field data”.

4. The model uses 16 layers and 31 free parameters, but the MT and RWD datasets contain only about 20 periods each (page 11). This results in an underdetermined inversion. Although the constraint term Φc helps, there is no quantitative justification for selecting 16 layers, such as resolution analysis (e.g., sensitivity kernels, model covariance), or discussion of how many layers the data can practically resolve. Include a model resolution study or adopt an adaptive layering strategy, similar to an "Occam-style” smoothness approach.

R: Thank you for this valuable comment. Counting observables, the original setup used 40 data points (20 MT ρa + 20 RWD) to estimate 47 model parameters (16 layers for both resistivity-depth and velocity-depth models and their 15 layers), i.e., it was formally underdetermined. In the revised manuscript we also include MT phase (20 φ), increasing the dataset to 60 observables, which makes the inversion formally overdetermined in a least-squares sense. However, we agree that the effective number of independent constraints is lower due to data correlation and parameter trade-offs. In the revised manuscript, 1) we directly introduced only a smoothness-term as the sole additional constraint on the third axis of the Pareto archive, 2) we therefore performed a dedicated layer-sensitivity test in subsection 7.1.3, where we repeated the joint inversion with different numbers of layers and compared the resulting data fits and model behavior. This analysis demonstrates the effective resolvable model complexity for our MT (ρa, φ) and RWD data combination, and shows that increasing the number of layers beyond our selected parameterization yields only marginal improvements in misfit while increasing model non-uniqueness. We follow the layering sensitivity test on the field data in subsection 7.4.1 and 7.4.2. Thank you for this important comment.

5. The paper identifies Region A: Low resistivity and stable velocity as hydrothermal alteration; Region B: Low resistivity and low velocity as melt or fluids; and Region C: Low resistivity only as mineralization in the lower crust. These interpretations are plausible but not directly supported because no Vp/Vs, attenuation, or anisotropy data are used; no comparison with seismic tomography or MT 2-D sections is made; and no petrophysical temperature or pressure modeling is presented. Therefore, the interpretation might be overstated. If possible, include supporting geophysical evidence such as receiver functions, heat flow, or seismic tomography, or frame these interpretations more cautiously.

> **R:** We thank for your constructive comment. We agree that interpretations based solely on resistivity and shear-wave velocity can be non-unique, and that additional constraints (e.g., Vp/Vs, attenuation, anisotropy, receiver functions) would strengthen discrimination between fluids, melt, and alteration/mineralization.
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> In the revised manuscript, we have substantially updated the field datasets and the complete modeling workflow from start to finish. In addition, we have reframed the interpretation to avoid overstatement and to emphasize that the identified regions represent plausible scenarios consistent with the joint MT–RWD results rather than definitive petrophysical diagnoses.
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> The revised manuscript now provides (1) a fully updated, internally consistent joint inversion framework, (2) explicit cross-references to spatially and depth-consistent independent evidence from the literature, and (3) a more cautious, uncertainty-aware interpretation. We believe these changes address your concern that the original interpretations might be overstated. Please see the all sections of the 7.4.

6. The study reports that one station requires three hours for 1,000 iterations (page 22), but it does not compare runtime with other algorithms, analyze scalability to 2D or 3D, or provide guidance on the number of particles versus computation time. These shortcomings indicate poor computational performance. Add a section on computational performance that discusses trade-offs, scaling predictions, and parallelization strategies.

> **R:** Thank you for this important comment. We agree that computational performance and practical trade-offs should be discussed more explicitly. In the revised manuscript, we have therefore added a dedicated subsection on “7.3 Benchmark comparison and computational performance”, where we (1) report the computing infrastructure and software environment, (2) provide guidance on how the runtime depends on the key PSO parameters (number of particles), (3) benchmark the Pareto–MOPSO workflow against a conventional Gauss–Newton scheme and (4) scalability to 2D/3D. We believe these additions address your concerns by providing a transparent discussion of performance.

7. There are no comparisons to borehole resistivity logs, local seismic velocity profiles, or previous MT/seismic surveys in the Biga Peninsula (many exist). This weakens confidence in the geophysical interpretation. Integrate or at least reference available independent constraints.

> **R:** Thank you for this comment. This concern is closely related to the issue raised in Comment #5 regarding previous studies. As we explained in our response to Comment #5, we cited cross-references to independent evidence from the literature that is both spatially and depth-consistent.

8. Some methodological steps lack clarity, such as how the initial particle positions were selected. How was convergence evaluated besides by iteration count? Why were c1 = c2 = 2.05 specifically chosen? How is the utopia point mathematically determined? These issues need to be addressed in the work.

> **R:** Thank you for pointing out these points. We agree that several methodological details should be stated more explicitly to improve transparency and reproducibility. Therefore, in the revised manuscript, we defined (1) initialization of particle positions in Lines 307-309, (2) convergence/stopping criteria beyond a fixed iteration count in Lines 317-319, (3) the rationale and reference for choosing $c1=c2=2.05$ in Lines 304-306 and (4) Mathematical definition of utopia point in Lines 332-334.

9. Several figures (e.g., Pareto fronts, PDFs) lack explanations for colorbars, layer boundaries, and clear distinctions between the POS and mP* models. Error bars for observed data are missing, and there are inconsistencies in notation, such as ΦMT and ΦRWD appearing in different fonts. The terms “mP*,” “mP*-model,” and “PO-model” should be standardized. Layer numbering is inconsistent between the text and figures. Enhance figures to publication quality by improving annotations and standardizing color schemes.

> **R:** Thank you for this detailed comment. We agree that several figures required clearer annotations and stricter standardization to reach publication quality. In the revised manuscript, we have systematically revised all figures and captions and implemented the following improvements: (1) colorbars and annotations, (2) Layer numbering, (3) Clear distinction (to prevent confusion and inconsistency, we standardized the terminology throughout the manuscript and, where there was a risk of ambiguity, we removed unnecessary abbreviations). (4) Notation and font consistency, (5) Standardized style and color scheme.

10. Provide pseudocode for the algorithm.

> **R:** Thank you for this helpful suggestion. We agree that providing pseudocode improves clarity and reproducibility. In the revised manuscript, we have explicitly referenced the pseudocode description in Lines 330-331. For completeness and to keep the manuscript concise, the full pseudocode is provided in the Supplementary Material as Figure S1.