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.
>
> 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:
>
> - 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.

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.

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.

Dear Lorenzo Schmitt,

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.

This paper deals with the joint inversion of Magnetotelluric (MT) and Rayleigh wave dispersion (RWD) data for one-dimensional resistivity- and velocity-depth models. In a new approach, the multiobjective particle swarm optimization (PSO) and the Pareto optimality method are utilized together, whereas in the past, global optimization algorithms with Pareto optimality were mostly integrated to jointly invert various geophysical data. The paper is theoretically clear in terms of a comprehensive description of the two applied methods, the creation of the synthetic dataset, and the references to previous applied joint inversion approaches. Despite some minor issues that need improvement, such as figure labels, inconsistent wording in the text, and mathematical symbols in the equations, I found the article quite straightforward to understand up to the description of the field datasets. One can already tell that the authors have established a valuable scientific basis for their application of joint inversion. However, the following chapters on one-dimensional joint inversion of MT and RWD inversion for a synthetic and real data example should be significantly improved in order to be accepted for publication. I suggest here a major revision of the paper and will present below my comments and questions that arose during my review and need to be revised.

R: We sincerely thank you for the careful reading of our manuscript and for the constructive and encouraging assessment of our work. We appreciate your recognition that 1) the theoretical background and methodological description are clear, 2) the synthetic dataset design is comprehensively explained, and 3) the proposed integration of multi-objective PSO with Pareto optimality provides a valuable scientific basis for joint inversion of MT and RWD data.

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 will substantially improve these sections to increase clarity, reproducibility, and the strength of the interpretation. Specifically, we have:

- 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 decoupled 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.

Specific comments and questions:

MT:

Line 174-175: Why is the phase not considered as an input parameter for the inversion? MT is fundamentally based on the complex impedance tensor. The apparent resistivity is derived from the magnitude of the impedance tensor and the phase (derived from the argument of the complex impedance tensor) constrains the geometry and vertical resistivity gradients, which the apparent resistance alone cannot imply. By discarding the phase, valuable information about the depth localization of certain structures, such as conductors, will be lost and can lead to a non-uniqueness of the inverted resistivity-depth model. If this is the case, particularly due to heterogeneities near the surface, which usually lead to a static shift and are taken into account by the phase, the second dataset PWD must provide a strong structural constraint as compensation.

> **R:** We thank for this important comment. We would like to clarify that we do include MT phase in the inversion. The joint inversion is performed using three datasets simultaneously: MT apparent resistivity, MT phase, and the RWD curve. Therefore, the MT response is not reduced to apparent resistivity only; instead, both amplitude- and phase-related information are jointly fitted over the entire period range.
>
> Please see the revised sentence in Lines 183-186.

Line 174: The apparent resistivity is not a local or absolute value since it reflects a frequency-dependent, depth-averaged resistivity of the subsurface over the electromagnetic diffusion depth.

> **R:** Thank you for this important clarification. To avoid any misunderstanding, we revised the text accordingly in Line 183.

Line 209-210: Some things about the Electromagnetic (EM) fields have to be clarified here. Electric and magnetic fields are generated simultaneously by external EM sources, e.g. ionospheric/magnetospheric currents, as written in the paper. The propagating time-varying electric and magnetic fields interact with the conductive Earth, where they induce secondary currents within the Earth. The secondary currents can then be measured at the Earth's surface.

> **R:** Thank you for the clarification. We agree that external EM sources generate both electric and magnetic fields simultaneously, and that these time-varying fields interact with the conductive Earth and induce secondary currents. We revised the text to explicitly describe the primary (external) source fields and the induced (secondary) currents measured at the surface in Lines 224-225.

Line 211: The MT period range has to be changed. The typical MT period range is 10^(-4) s (or 10 kHz) to 10^4 s.

> **R:** Agreed. We corrected the period range in the manuscript to 10^{-4} to 10^{4} s to reflect the commonly used MT bandwidth, in Line 226.

Line 212: My comment in lines 209-210 indicates that EM waves are not only generated by magnetic fields. The sources of EM fields are electric charges and electric currents in the ionosphere or magnetosphere, such as lighting, substorms or solar winds. Sources are external to the Earth and internal currents are induced responses.

> **R:** We agree. We modified the wording to state that MT source fields are produced by electric currents/charges in the ionosphere/magnetosphere and atmospheric processes (e.g., lightning), rather than "magnetic fields" alone, in Lines 220-223.

Line 213-216: The internal sources are not worth mentioning since they are not responsible for EM fields. The magnetization of a rock has nothing to do with MT diffusion processes.

> **R:** Agreed. We removed the discussion of internal sources and rock magnetization from this section, and we now emphasize that MT predominantly relies on external sources, with the Earth's response appearing as induced currents.

Line 222-223: This sentence needs to be changed. The impedance tensor is not measured directly since the impedance is deduced from field measurements. As mentioned in my other comments before, the fundamental principle of the MT method is the estimation of the frequency-dependent impedance tensor from simultaneous measurements of the electric and magnetic fields at the Earth's surface. The Earth modifies the ratio and phase between the electric and magnetic fields.

> **R:** We revised the sentence to state that the fundamental principle is the estimation of the frequency-dependent impedance tensor from simultaneous electric and magnetic field measurements, and that the Earth modifies the amplitude ratio and phase between these fields, in Lines 233-237.

Line 223-224: How is the apparent resistivity derived from the complex impedance tensor? Perhaps the formula for apparent resistivity should be mentioned here, or at least that apparent resistivity is frequency dependent.

> **R:** We added the standard relationship indicating that apparent resistivity is computed from the magnitude of impedance and is period/frequency dependent. Please see the revised equations 3 & 4.

Line 226: Use the correct MT term "phase" and not "phase information". (This should be set as a standard in the entire paper.)

> **R:** Implemented. We replaced "phase information" with the standard MT term "phase" throughout the manuscript for consistency.

Line 229-231: Consider a rewriting of the sentence to: "To estimate the impedance tensor, the time series were segmented into overlapping windows and transformed to the frequency domain using the FFT. Power and cross-spectra were then computed and stacked, from which the impedance tensor elements were estimated."

R: Thank you. We adopted the your suggested wording (with minor adaptation) describing windowing, FFT transformation, power/cross spectra computation and stacking, and subsequent impedance estimation, in Lines 241-243.

Line 487-489: The authors have to mention at least once that for 1D MT modeling and inversion, a scalar or rotationally invariant impedance derived from the complete impedance tensor is used, since the impedance tensor itself is not used for modeling.

R: Thank you for this comment. Although we partially indicate that we used effective impedance tensor of Berdichevsky et al. (1989) in section 3, our explanations seem to be short to explain the case. Therefore, we revised the sentence in Lines 179-180, according your comment.

Line 484-485: As described in the paper, the MT data exhibit a 3D character beyond 1 s (not 10 s, as stated). The phase tensor ellipses become elliptical and change direction. In addition, the minimum phase angle changes abruptly in a short frequency/period range. Furthermore, the GULC station is located very close to the sea, which leads to a splitting of the apparent resistance curves of the two modes TE and TM at the longer periods of 10 s and above, here presumably Zxy and Zyx. This sea effect is caused by the strong conductivity contrast between seawater and the resistive crust/upper mantle, which can lead to a misinterpretation of the resistivities in the depth range of the lithosphere-asthenosphere boundary due to unreliable strong conductors in the resistivity-depth model. These periods must then be excluded for a 1D inversion. Therefore, the following questions must be answered more precisely in the paper:

1. How could the quality of the MT field data be improved by the ProcMT processing software? Are there many noise influences/effects visible in the recorded field data?

2. How do the main components of the impedance tensor look like? Are they close to zero, or large (hint to 3D MT data)?

3. Were the data rotated into a specific geoelectric strike direction after the processing to minimize the main diagonal elements of the impedance tensor and assume a 2D subsurface structure? Or were the MT field data treated directly as 1D (which would not be the accurate, at least for periods greater than 10 s, as can be seen from the phase tensor plots)?

4. Which component of the impedance tensor was ultimately used for the 1D inversion? Zxy or Zyx, since it is not mentioned?

R: We thank the reviewer for this detailed and technically important comment. We agree that the MT data show a clear increase of high dimensional behavior beyond ~1 s (as indicated by the phase tensor ellipticity, orientation changes), and that a near-coastal site (GULC) can be affected by sea-water conductivity contrast, and biased deep resistivity estimates if treated in a 1-D framework. In the revised manuscript we have therefore updated the complete MT processing workflow and tightened the period selection and inversion strategy accordingly. All related details are now explicitly described in the revised Section “7.4 Field data”.

1) How could the quality of the MT field data be improved by the processing software? Are there many noise influences/effects visible?

In the revision, the MT data were not processed with the older ProcMT version; instead, we processed the time series using the current SigMT processing tool to enable improved data quality control through robust processing steps. Therefore, we revised the sentence in Line 241.

2) How do the main components of the impedance tensor look like? Are they close to zero, or large (hint to 3D)?

We address this explicitly in Section 7.4 by summarizing the tensor behavior. Consistent with your observation, the impedance tensor exhibits high-dimensional characteristics beyond ~1 s, reflected by the phase tensor diagnostics and by the reduced reliability of a 1-D assumption at longer periods. We therefore avoid over-interpreting the long-period response in terms of 1-D deep structure.

3) Were the data rotated into a geoelectric strike direction (2D assumption), or treated directly as 1D?

Given the observed high dimensional effects and the coastal influence at longer periods, we did not enforce a 2-D strike rotation as a prerequisite to interpret TE/TM separately. Instead, we adopted a conservative approach suitable for 1-D inversion: we restricted the inversion to periods ≤ 1 s, where the phase tensor indicates a near-1-D character.

4) Which impedance component was used for the 1D inversion (Zxy or Zyx)?

We clarify this point in the revised text. As stated in the manuscript (see Lines 179-180 in the revised version and Section 7.4]), we did not invert Zxy or Zyx individually; instead, we used the determinant impedance (and derived apparent resistivity/phase) as the MT observable for the 1-D inversion.

General:

Line 255 & 282: The logarithm should be applied on the variable and not on the unit, such as log10(rho) [Ωm] and log10[1, 5] Ωm

R: Agreed. We revised the sentence in Lines 300 & 301.

Line 265: Why did the authors choose 31 model parameters with 16 layers for the models, even though the MT and RWD data sets only cover 20 periods? Normally, the layers should be set to a minimum, which also reduces the computing time, and then continuously increased after each iteration through adaptive layering. How many layers can the modeling algorithm resolve, which has not yet been shown?

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.

Line 293: How are these values for c1, c2, k and chi defined? Are they derived from previous tests or inversions with the MOPSO and Pareto optimality method?

R: Thank you for the question. The parameters c1, c2, k and chi, and the constriction factor are not derived from additional trial inversions in this study, but are taken from the constriction-factor PSO formulation proposed by Clerc and Kennedy (2002), which is widely used to ensure stable swarm dynamics and convergence.

Therefore, we revised the sentence in Line 305 to explicitly state this definition and to clarify that these values follow Clerc and Kennedy (2002), not a case-specific calibration.

Line 319: How do the PDFs look for the others layers, e.g. in the resistivity-depth model? Since the PO-model shows greater deviations from the synthetic model, here at a depth of 7 or 10 km for the resistivity-depth model. And if the authors consider the 8th layer of the velocity-depth model to be incompatible, then the difference for the lowest resistivity at a depth of 10 km in the noise-free resistivity-depth model is already incompatible.

1. Have you tried the 1D inversion algorithm on a very simple three-layer model such as in Moorkamp et al. (2010) and verified the robustness and reliability of the algorithm? It appears that the inversion code has difficulty fitting the lowest resistivities, even in the case of field data that can be imaged most sensitively using the MT method. As mentioned earlier, this can happen when the phase of the MT data is not used in the inversion.

2. Can you additionally compare your joint inversion results with other 1D non-linear inversion codes for MT and RWD data? It would be beneficial to include an example in the paper that illustrates the accuracy of the 1D joint inversion models compared to other inversion methods using the same synthetic data.

R: Thank you for these detailed and constructive comments. We have revised the manuscript to address each point, improve the robustness discussion, and clarify how uncertainty and non-uniqueness are assessed.

(1) PDFs for other layers and interpretation of deviations

We agree that inspecting uncertainty across all layers is important. Instead of presenting separate PDFs for every layer (which would considerably expand the manuscript), we now summarize the solution distribution for each layer by reporting the P10–P90 (10th–90th percentile) interval of the accepted Pareto/ensemble models. This P10–P90 band is plotted for all layers and therefore provides a compact, layer-by-layer view of uncertainty, non-uniqueness, and the range of admissible models. We clarify in the revised text that the P10–P90 envelope is used as an uncertainty proxy and that deviations at specific depths are interpreted in the context of this band rather than from a single preferred solution alone.

(2) Robustness tests and the role of MT phase

We appreciate your concern regarding fitting the lowest resistivities and the possibility of non-uniqueness when phase is not included. We would like to clarify that in the revised workflow MT phase is explicitly included in the inversion together with MT apparent resistivity and the RWD dispersion curve. Incorporating phase provides additional constraints on vertical resistivity gradients and depth localization and reduces ambiguity compared to using apparent resistivity alone.

Regarding robustness: rather than restricting validation to a single three-layer example, we performed a broader layering-sensitivity analysis (section 7.1.3) that directly tests stability with respect to parameterization complexity. This test demonstrates that the main recovered features remain stable across different discretizations while quantifying the conditions under which deeper low-resistivity features become less well constrained.

(3) Comparison with other inversion methods

We agree that benchmarking improves confidence. In the revised manuscript we added a dedicated subsection as Section “7.3 Benchmark comparison and computational performance” comparing Pareto-MOPSO joint inversion results with a conventional derivative-based Gauss–Newton solution using the same synthetic setup.

(4) Layering choice and adaptive layering suggestion

We agree that the number of layers should be justified by resolvability. Instead of implementing adaptive layering, we added a layering-sensitivity subsection (Section 7.1.3) where we repeat the joint inversion for four different layer-number parameterizations and evaluate data fit, stability of recovered structures, and uncertainty behavior. This explicitly demonstrates how model complexity affects the solution and supports our selected discretization as a practical balance between stability and flexibility.

Line 421-460: Here, the various geological structures of the Earth are identified using resistivity- and velocity-depth models. Region A can be attributed to volcanic rocks undergoing hydrothermal transformation. These findings are supported by earlier observations of the southeastern part of the

Biga Peninsula, which were determined using seismic noise tomography. The other regions B, C, and < 5 km depth are explained by references to scientific studies of similar magmatic environments that could be responsible for mantle-derived melt, mineralization in the lower crust, and the sedimentary layer from the surface to a depth of 2 km on the Biga Peninsula. However, no connections are made to existing borehole logs or geophysical studies, such as Vp/Vs or S-wave tomography, previous MT surveys, and heat flow or fluid modeling studies that could support the statements about these different regions of the Earth. Some supporting studies of the research area would help to provide a more solid basis for the interpretations. In addition, the inclusion of the MT stations GURE and KULC in the geological map of Figure 4 could improve understanding of why more geothermal fields are found at the GURE site (in connection with the more permeable continental sediments). If possible, the location of the geothermal fields could also be mapped here.

> **R:** Thank you for this constructive suggestion. In the revised manuscript, we comprehensively re-evaluated the model results using the improved data sets and substantially updated the related interpretations. These revisions are now presented in detail in Sections 7.4.1, 7.4.2, and 7.4.3, where the revised model features, their geological significance, and the outcomes of the sensitivity tests are discussed more explicitly. In addition, we now explicitly discuss the geothermal characteristics of the GURE area in Lines 844–847, in order to better relate the interpreted structures to the known geothermal setting of the study area. Furthermore, in the revised Figure 6, we added the locations of the MT stations as well as the geothermal wells, so that the spatial relationship between the geophysical observations and the geothermal field distribution can be more clearly understood.
>
> We therefore kindly ask you to reassess these subsections in their revised form, as they now provide a significantly improved and more robust framework for the interpretation of the resistivity and seismic velocity structures.

Line 472-480: Instead of focusing the quantitative analysis of the sensitivity test on a single value, a representation of the actual data fit between the synthetic and modeled data (apparent resistivity and phase velocity) as shown in Figures 11 a) and b) would more clearly highlight the obvious changes to a better or worse data fit. A number for the data fit often says nothing about the actual influence of a substitution in the model parameters. This raises two further questions:

According to which criteria are the model parameters in the table selected?

What is the depth interval of the substituted model parameters, e.g., between 5 and 10 km depth for region A? (A visualization of the changes in the resistivity- and velocity-depth models would improve understanding about which sections of the depth models were changed for the sensitivity tests.)

> **R:** We thank you for this constructive and important comment. In response, we substantially revised the sensitivity-test analysis in the manuscript. A new subsection

(Section 7.4.3) has been added, in which the sensitivity tests for Zones A, B, and C are presented not only through single misfit values, but also through figures showing the full modeled data fits for MT apparent resistivity, MT phase, and RWD responses across the relevant period/frequency ranges. These visualizations make the effect of each parameter substitution on the actual data fit much clearer.

In addition, Table 1 has been revised to include the updated objective-function values and their percentage changes. We also clarified the criteria used to select the substituted model parameters and added model panels showing the original and modified resistivity- and velocity-depth structures, so that the affected depth intervals can be directly identified. We believe these additions have considerably improved the clarity and interpretability of the sensitivity-test section.

Line 502-507: All these descriptions about the computing time of the PSO, the utilized computation infrastructure and the software must be mentioned in advance, for instance, directly in the "MOPSO and Pareto optimality parameters" section.

R: Thank you for this suggestion. We agree that computational environment and timing information should be introduced earlier for transparency. In the revised manuscript, the key information on computational infrastructure and representative run-times to the Pareto-MOPSO has been clarified in Section 7.3. In addition, the detailed GN–MOPSO comparison is maintained within the subsection on benchmarking, with the objective of ensuring comprehensive documentation. This ensures that the reader knows the software/hardware context and performance discussion.

In general, a final chapter entitled "Conclusions" should be added to the paper, explaining and summarizing the main results and advantages of Pareto MOPSO in the joint modeling of MT and RWD data. In this context, lines 101–105 read like a concluding statement and can be included in the "Conclusions" section.

R: We sincerely thank the reviewer for this important remark. We fully agree that a dedicated Conclusions section is essential for clearly summarizing the main findings and the advantages of the proposed Pareto–MOPSO framework. We apologize for this omission in the original submission. In the revised manuscript, we have added a final chapter entitled "Conclusions", where we concisely synthesize the key methodological contributions and the main results from both the synthetic and field examples, and explicitly highlight the practical benefits of Pareto–MOPSO for joint MT–RWD modeling.

There is no section on "Data Availability" in the paper, or can the data only be published upon request?

R: Thank you for pointing this out. We agree and have added a dedicated Data Availability section in the revised manuscript. Therefore, we added "Data Availability" in the revised manuscript.

Technical corrections:

Line 29: The abbreviations should be written out once in the main text: Magnetotelluric (MT) and Rayleigh wave dispersion (RWD)

> **R:** Thank you for the suggestion. We note that the abbreviations were already defined in the Abstract; however, we agree that they should also be defined once in the main text for clarity and consistency. We therefore updated the first occurrence in the Introduction to read “Magnetotelluric (MT)” and “Rayleigh wave dispersion (RWD) in Lines 36-37.

Inconsistent wording (one has to be chosen for the entire paper): e.g. modelling to modeling; dataset or data set

> **R:** Thank you for pointing this out. We agree that consistent terminology is essential. We have revised the manuscript to use a single spelling style throughout: “modeling” and “dataset” (single word), and we corrected all inconsistent occurrences accordingly.

Line 103: incorrect word

> **R:** The we have corrected the word as south-eastern in Line 105

Line 107: Add PSO in the section title

> The section title was revised.

Line 211: Maintain a consistent format for all units throughout the entire paper, here seconds or s.

> **R:** Thank you for pointing this out. We agree that consistent terminology is essential. We have revised the manuscript to use “s”, and we corrected all inconsistent occurrences accordingly.

Line 310: Should be 15%

> **R:** We corrected all inconsistent percentages (%)

Equation 1 & 6: The tensor product (Kronecker product) is not required here. Instead, element-wise multiplication with the Hadamard product must be inserted into both equations. Otherwise, the output would be a tensor that would increase the dimension, and the result would not be a vector in the model space.

> **R:** Thank you for this important correction. We have therefore revised the equations by replacing the tensor product with element-wise multiplication between the random vectors and the corresponding difference vectors. Please see the revised Equation 1 & 8.

Equation 3: The symbol for apparent resistivity in the equation does not match the one used in the following text. Mathematical symbols are used in different notations in the paper, such as φ in line 140.

R: Thank you for pointing this out. We agree that the notation was inconsistent in the previous version (e.g., the apparent resistivity symbol in Eq. (3) and the phase symbol ϕ used elsewhere). We have carefully reviewed the entire manuscript and standardized all mathematical symbols and notations throughout, including Eq. (3) and the subsequent text, to ensure consistent use of the apparent resistivity (ρ_a) and phase (φ) symbols across the paper.

Figure 1: The search space is defined by an x-component on the x-axis. But what is on the y-axis? Shouldn't it be x1 on the x-axis and x2 on the y-axis?

R: Thank you for the remark. We agree that panel (a) depicts a 2-D projection of the search space, and the axes should be labelled accordingly. We revised the figure by labelling the axes as $x1$ and $x2$ (i.e., a 2-D projection of the model space) and clarified this in the caption.

Figure 2: The label b) is missing in the caption.

R: Thank you for this comment. We added the label and revised the Figure 2 caption.

Figure 3: The apparent resistivity label in subfigure 3) looks distorted.

R: Thank you for noting this. In the revised manuscript, we have redesigned the synthetic-data figure set under the new concept, and the updated Figure 3 has been regenerated with corrected typography (axis labels and symbols). Therefore, the distorted label issue has been fully resolved in the revised figures.

Figure 4: The color bar for the topographic map has no labels, e.g. “Elevation (m)”.

R: Thank you for the comment. We have revised Figure 4 by adding a label to the color bar, “Elevation (m)”, to clearly indicate the topographic units. Please see the Figure 6.

Figure 9: The labels a) and b) are either incorrectly assigned in the caption or in the figure itself.

R: Thank you for pointing this out. We have corrected this by swapping the a) and b) labels so the subfigures are now consistently assigned.

Figure 10: The phase unit in the label should be specified in degrees and not as the phase symbol. Please use “phase” instead of “phase angle” here.

R: Thank you for this comment. We have corrected this issue consistently in all relevant figures. The phase label is now given as “Phase(deg)” instead of the phase symbol or “phase angle.”

All subfigures are labeled (a), (b), etc. in the figure captions. However, the labels for the subfigures are a), b) and are also referred by these names in the text, which is inconsistent.

R: Thank you. We have standardized the subfigure notation throughout the manuscript. Subpanels are now labeled as a), b), … in the figures and captions, and they are referenced consistently in the running text as Figure. Xa, Figure. Xb, etc, following the journal procedures.