Dear Editor, dear Reviewer,

We provide hereunder an overview of how we address the comments, with explanations and references, on a point-by-point basis. Reviewers' comments are in black and our response in blue. In the manuscript, all the new/changed text is highlighted in yellow.

In addition to the changes directly in relation to the Reviewer's comments, we would like to emphasize that we also added a new figure to the main text (without being asked for it). We felt like the last panel of the former Fig. 6 could benefit from a more illustrative representation to clarify the different hydroclimatic modes. As such we have made a new Fig. 7, combining the former panel with 2D sketches with 3D illustrations of the landscape. As this figure does not present new data, only a (we think) better representation of the data, and the new figure does not alter any conclusions of the paper, we hope that the Editor is ok with this new figure.

Reviewer 3 Comments to Author:

De Gelder et al. developed a method to estimate past sea-level changes (on a 10 ka - 400 ka scale) from the topographic profiles of Pleistocene marine terraces through a Bayesian inversion using a topographic evolution model based on coastal erosion.

Conventionally, analyses of the relationship between marine terraces and sea-level change history have either assumed a known sea-level curve to estimate coastal uplift rates or inferred past sea-level fluctuations by first determining the formation ages of terraces. This study allows for significant flexibility in sea-level changes and uplift rates, enabling reliable estimations based solely on topographic profiles. This approach is highly innovative.

The analysis in the Gulf of Corinth, in particular, showed a remarkable achievement of the authors' method, as it enabled an independent estimation of sea-level change history in a unique environment that was not always connected to the open ocean, separate from the well- known eustatic sea-level change curves. The tectonic and geological interpretation of the estimated sea-level change history is also well-structured and logically sound.

Therefore, I have no objections to the subject, analysis, results, and interpretation presented in this manuscript being accepted in the future. However, particularly in the Methods section, the authors seem to have omitted important details and are not providing sufficient information for the readers. Below, I will point out the inadequacies in the descriptions and offer suggestions for improvement.

Thank you, we really appreciate the time and effort put in to this review, and can also agree with the reviewer that some clarifications will further improve the manuscript.

1. Vague descriptions in Methods
Section 2, Marine Terrace Sequence Inversion, describes the methods used in this study, namely the topographic evolution model as a terrace formation model and the MCMC method as a Bayesian inversion approach. In general, reports of nonlinear inversion clearly describe the following three steps to make the approach more comprehensible:

1) Mathematical formulation of the observation equation

2) Evaluation of the posterior likelihood of a proposed model

3) Implementation of the nonlinear inversion

Step (1) essentially defines what the model parameters are, what the observations are, and how they are related. A general formulation is:

$d = f(g) + e$

In the context of this study, d represents the topographic profile, g is the sea-level change curve, uplift rate, and other control parameters, and $f()$ corresponds to the REEF code. Since $f()$ is a complex nonlinear model, mathematical expression may not be necessary. Instead, it would be clearer to indicate that the observed topographic profile (e.g., $z = P(x)$) is a function of the model parameters, which can be written as $z = P(x|g)$. Also, since this study compares the horizontal positions as the observed data, an appropriate representation could be $x = P'(z|g)$.

We agree that the proposed distinction would make the methods section more clear, so we added some introductory sentences, and improved the description of section 2.1 following the reviewer's suggestions (Lines 99-110), that now read:

"In the following sub-sections, we describe the inversion of marine terraces in 4 parts: 1) the unknown model parameters and their relation to observed terraces, 2) the Bayesian formulation of the inverse problem, 3) the Monte Carlo algorithm to approximate the probabilistic solution and 4) the bounds of the uniform prior ranges.

2.1 Model parameters
As a general formulation, we can consider:

$$d = g(m) + \varepsilon \qquad (1)$$

Where d describes the vector of observations, in our case the topographic profile of a terrace sequence, and m is the set of unknown model parameters to be inverted for: uplift rate (U), erosion rate (E), wave-base depth (z_0), initial slope (α) and sea-level history described by a set of nodes (see below). The function g describes the numerical erosion model that links these model parameters to the topographic profile, here the REEF code (Husson et al., 2018; Pastier et al., 2019). Data errors here are given by a random variable ε that describes the inability of the forward model g(m) to explain the observations d."*

We did decide to keep the mathematical expressions for the forward model as well. It was specifically requested by another reviewer, and we can imagine that other potential readers of the manuscript would also appreciate a brief description of the wave erosion model.

Note that here and throughout the manuscript, we changed the symbols for erosion rate (from ER to E*), initial slope (from IS to α), uplift rate (UR to U) and wave base depth (WB to z_0), to be consistent with the original papers describing the REEF model (Husson et al., 2018; Pastier et al., 2019)

Step (2) is the process of evaluating the likelihood by comparing the modeled observations with the true observation data. I guess the calculation the authors conducted was: When the true observed profile is represented as $x = D(z)$ and the errors are assumed to follow a Gaussian distribution with variance $\sigma 2$, then the likelihood for each elevation can be expressed as:

1 $(P'(z|g) - D(z))2$ $p(z|g) = \sqrt{2\pi\sigma 2}$ exp $(- 2\sigma 2\ )$

and the total log-likelihood over the entire observation range is

$(P'(z|g) - D(z))2$

log $L(g)$ = log $\prod p(z|g) = - \sum 2\sigma 2 + C(constant)$

This formulation assumes that errors at different elevation points are independent. However, since it is mentioned that correlations between nearby heights are considered, the authors should appropriately modify the formulation to account for these dependencies.
Regarding steps (1) and (2), I felt that the manuscript provides explanations in text that cover the necessary content. However, the explanations were not presented in a structured manner, and the lack of formal definitions for the observation data and model parameters led to unnecessary confusion.

We tried to clarify this 'step 2' (section 2.2) following the reviewers suggestion. We re-arranged some text and added the appropriate equations (Lines 133-163).

For Step (3), since the implementation of Bayesian inversion and MCMC requires the definition of model parameters and the model likelihood evaluation, it would be more effective to introduce them after (1) and (2). While mathematical expressions would be preferable, the current explanation is likely sufficient. One suggestion is to clearly distinguish between aspects common to general Bayesian inversion and MCMC sampling and those specific to this study.

We re-arranged the sentences from the previous version for a better structured explanation in section 2.3 as well now (Lines 166-178).

Additionally, if prior probability distributions or constraints are imposed on the model parameters, they should be described here. From the subsequent analysis, it appears that a strong prior constraint is applied to the uplift rate and sea-level curve.

We added a sub-section about the prior constraints now (Lines 180-193):

"2.4 Bounds of the uniform prior
For the different unknown model parameters, imposed prior constraints can be either restrictive, open or anything in between. Within the synthetic tests (section 3) we left the sea-level nodes open, and the other parameters either fixed (Fig. 2) or open (Fig. S3). For the Santa Cruz benchmark tests (section 4) we left the erosion rate, wave base depth and initial slope parameters relatively open, but placed a more restrictive range on the uplift rate of 1.3-1.65 mm/yr, so that the chronostratigraphy of the modeled terrace sequence would match published ages (Perg et al., 2001). We put soft prior constraints on the sea-level history, by restricting possible solutions to the red boxes in Fig. 1c, which represents a cautious interpretation of the ensemble of previous sea-level studies. We adopted a similar strategy for the Corinth terraces (section 5), with the erosion rate, wave base depth and initial slope parameters left relatively open, but stronger prior constraints on uplift rate ranges to respect previous findings on the chronostratigraphy. We tested models with the sea-/lake-level nodes either completely open between 15 and -150 meter elevation (Fig. S4), and with stronger prior constraints only on the highstands. During these highstands the Gulf of Corinth undoubtedly experienced marine conditions (McNeill et al., 2019), so the sea-level elevations can be expected to fall within the red boxes of eustatic sea-level defined in Fig. 1c, whereas lowstands are likely lacustrine."

2. Roles of each model parameter
The role of each model parameter in the inversion analysis does not seem to be explicitly stated. In this inversion, in addition to the sea-level history, model parameters such as IS, ER, WB, and UR are introduced. Upon first reading, I thought that both the sea-level history and the average uplift rate were being estimated as target parameters. However, it seems that the average uplift rates were pre-estimated from terrace ages and fixed in a certain range in the practical cases. While this approach is valid, it would be clearer to explicitly state that the uplift rates were treated as a known parameter and incorporated as a prior information. This would help avoid any confusion about its role in the inversion process.
Similarly, for sea-level curves, it appears that relatively strong constraints (red rectangles) are imposed in some time ranges where data are available (MIS-5e for example). However, the manuscript does not clearly convey how much the authors weight this prior information. In other words, there are two possible roles for each node:

1. The prior knowledge from other research constrains past sea levels within a range of several tens of meters and stabilize the overall solutions.

2. No substantial constraints are imposed, and past sea levels are estimated as the solution purely from topographic data.

In the case of the Gulf of Corinth, it seems that highstands are treated as the former case (strong prior constraints), while lowstands are treated as the latter. Since the manuscript attempts to display both cases within the same framework, it may cause confusion for readers.
In the case of MIS 7a (200 ka) in the Gulf of Corinth, the strong convergence occurs near the edge of the constrained region (red rectangle). This makes it unclear whether the estimated sea level was derived from the inversion of topographic data or simply influenced by the prior constraints. In contrast, for MIS 9a and 9b, the sea level appears to converge at the center of the broadly defined prior range, demonstrating that the model itself has a strong capacity to constrain sea-level variations. If the goal is to highlight the broader applicability of this model, especially in regions where past sea-level changes are poorly constrained, applying too strong priors could lead to an underestimation of the model's capabilities.
To improve the approach, two possible alternations can be considered: (1) Removing all vertical constraints on node heights and reconstructing sea levels purely from topographic

data (essentially applying the same constraint as in the synthetic test). However, if the inversion would not converge under this condition, then an alternative approach is: (2) Explicitly distinguishing highstands as constrained model parameters when they are supported by strong prior information.
In nonlinear inversion, various model parameters are incorporated, but not all of them are directly determined as the solution. Some serve as auxiliary parameters to stabilize the solution. The distinction between these roles may not be sufficiently clarified in the Methodology and Results sections.

Even in its current form, this study presents an excellent analytical approach, methodology, and results. However, a clearer articulation of what the analysis aims to resolve would elevate the research to an even higher level. If time allows, it would be valuable to examine the results when vertical constraints on sea-level heights are removed (or I guess the authors have already tried it but didn't converge within a reasonable solution). However, even without additional analysis, the current results can be interpreted as allowing some flexibility in highstand elevations, effectively treating them as soft constraints rather than fixed values.

We understand the confusion, and have now clarified that in section 2.4 (see comment above). At an earlier stage we also did perform tests with the Corinth nodes left completely open, but realized the solutions would not provide us with a lot of insight within the posterior distributions. Hence the, in our eyes justified, compromise to 'guide' the high stand elevations with stronger prior constraints.
We've added those initial tests with every node free in Fig. S4 now, and apart from section 2.4, we also clarified the choice of priors in section 5 (Lines 330-333):

"For the marine periods we follow the more restricted eustatic sea-level ranges defined in Fig. 1bc (red boxes), as the resulting posterior sea-/lake-level ranges would remain similar to the prior ranges for tests in which we also gave a lot of freedom to nodes for the marine periods (Fig. S4). "

Overall, I find the contents of this study to be highly commendable. One particularly noteworthy aspect of this approach is that joint inversion of marine terrace data that experienced the same sea-level changes but different uplift rates provides a strong constraint on the sea-level history. This study analyzed 1–3 transects per region. Was this limitation due to computational amounts or the availability of observational data? If the limitation is within computation, it could serve as a strong motivation for future research into improving the forward algorithm, such as REEF.

The limited amount of transects was mostly to keep things simple, and the main purpose of this paper was to test the model on increasingly complex settings. We clarified this now in the discussion (LInes 507-508): *"For simplicity, as the aim of this paper was to test the model on increasingly complex settings, we only inverted 1 profile for Santa Cruz and 3 for Corinth, but it is easily possible to explore with more profiles at those locations in future work."*

Regarding the interpretation of the Gulf of Corinth results in Section 6.1, I do not have specialized knowledge of the region's tectonics and environments, so I will refrain from making definitive judgments. However, the discussion appears to be consistent with observations from previous studies, and the interpretation of the topographic evolution history is innovative. Within the context of this paper, the strong constraints placed on sea- level changes that differ from the eustatic changes of the open ocean are particularly important. I think this aligns well with the purpose of the newly developed method.

We appreciate the comment, thank you.

Other minor comments are below:

>L121: We use nodes interpolated through a cubic spline scheme (Fig. 2b; light blue). Each node has values for age and elevation. The set of nodes can be expressed as $V = \{v_i = (t_i, z_i | i = 1,2,...,n)\}$

Changed as suggested (Lines 126-128):

"We use nodes interpolated through a cubic spline scheme (Fig. 2b; light blue), in which each node has values for age and elevation, and the set of nodes can be expressed as:

$$v_i = (t_i, z_i \,|\, i = 1,2,\ldots,n) \qquad (4)"$$

>LL132–134: ~, or left free within chosen ranges, as done for the 133 Santa Cruz and Corinth examples below.
The meaning of the analysis fundamentally depends on the size of the chosen range here: If a wide uniform prior distribution is assigned, the parameter remains flexible, and the inversion result represents one possible solution constrained by the observation. If a narrow range is imposed, the parameter effectively becomes a prior constraint. Looking at the posterior probability density distributions, it appears that IS and ER behave as free parameters, while UR acts more like a prior constraint.

We hope to have clarified this now with the method section on prior ranges (Lines 180-193).

>LL149–150: In this way, the terrace width is used as an observed parameter.
This sentence is highly misleading. Upon first reading, I thought that the terrace width w was extracted from the topographic profile, and then the model directly outputs terrace width by some means and evaluated the misfits. However, it turned out that the horizontal distance between the modeled and observed topographic profile was used as the model error, and, as a consequence, the terrace width emerges as a primal feature.

We agree this sentence was misleading, and have removed it

>LL166–168: For the inversion, we fixed the nodes at 78, 6 and 0 ka, ~ Are heights also fixed?

We clarified this now: *"we fixed the elevation and ages of the nodes at 0, 0 and -30 m and 78, 6 and 0 ka, respectively,"* (Lines 207-208)

>LL354–355: The major peaks in our reconstructed sea/lake-level curve occurred during interglacial sea-level highstands, when sea level in the Gulf of Corinth was similar to eustatic sea level (marine mode M).
Didn't the model setup constrain the ages for highstands?

Yes it did, so it is not a surprise, but for the structure and flow of this section we wanted to mention it anyway. We have clarified *"as direct consequence of our choices in the ranges of priors,"* (Lines 379-380).

>LL452–459: In the cases of Corinth and Santa Cruz we assigned relatively narrow windows for the uplift rate, ~
This paragraph addresses the concerns I had. The discussion here is well thought out, but it would be beneficial to mention also in either the model setup or the introduction that a strong prior constraint is imposed on the uplift rate, along with its meaning.

Fair enough, we have clarified that within the method section on prior ranges now (Lines 180-193).

>LL470–472: It suggests that estimating paleo sea-level based on the comparison of a present-day landform to a paleo-landform (Rovere et al., 2016), may be too simplistic in many cases, at least for erosive marine terraces.
Personally, I believe that joint analysis using multiple profiles provides a very strong constraint, and I am optimistic about its potential. In Discussion, there is little mention of joint analysis. Why not emphasizing this aspect and highlight the future applicability and robustness of this approach?

We did mention the joint analysis in the following paragraph already: *"One of our key findings is that inverting multiple profiles simultaneously provides much better paleo sea-level constraints than focusing on individual profiles (Figs. 2, 5)."* (Lines 511-512), but have now also emphasized it in this paragraph: *"To reduce uncertainties, we demonstrated in this paper that a joint inversion of multiple profiles is a powerful way forward."* (Lines 505-506)

>Figure 2.
It was explained that the MIS3 age is not constrained because it is currently below sea level. Wouldn't it be possible to represent this range in Fig. 2? I mean a diagonal line with the slope of UR projected from the present sea level, indicating that the triangular area below this line is submerged and not reflected in the terrestrial topography. This addition would direct the reader's attention to the upper right parts of Fig. 2b, d, and e, reinforcing the impression that the model has strong constraining power.

We understand the idea, although the reviewer probably refers to the MIS1 terrace (not MIS3). We considered adding such a line, but find it to be complicating the figure rather than clarifying. As mentioned in Lines 223-225:

"*Although the MIS 1 terrace is not inverted, there are some limitations to the magnitude and rate of sea-level rise between MIS 2 and MIS 1 (Fig. 2b), probably because this period determines how much of the MIS 3 terrace is eroded at its distal edge.*"

So to some extent the sea-level changes below such a diagonal line, from 0 m at 0 ka as proposed by the reviewer, would still have an impact on the terrestrial topography.

We very much appreciate the time and effort by the reviewer to go through our manuscript, as well as the good suggestions made to improve the manuscript.

We hope the applied changes will be appreciated,

Kind regards,

Gino De Gelder, Navid Hedjazian, Laurent Husson, Thomas Bodin, Yannick Boucharat, Kevin Pedoja, Tubagus Solihuddin and Sri Yudawati Cahyarini