Comments on DeGelder et al. submitted to ESurf

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.

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 $x = P'(z|g)$.

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 σ^2, then the likelihood for each elevation can be expressed as:

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

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

$$\log L(g) = \log \prod p(z|g) = -\sum \frac{\left(P'(z|g) - D(z)\right)^2}{2\sigma^2} + C(constant)$$

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.

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.

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.

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.

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.

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.

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 $V = \{v_i = (t_i, z_i | i = 1,2, \ldots, n)\}$

>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.

>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.

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

Are heights also fixed?

>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?

>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.

>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?

>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.