Reviewer 2 Comments to Author:

This manuscript is interesting, novel and well written, and I like it. It presents a new method that inverts the topography (mainly widths) of marine terrace staircases, simultaneously solving for sea-level history, uplift rate and some other parameters. The method seems to work pretty well on the three test datasets (one synthetic, two real).

Thank you, we appreciate the comment.

I agree with the comments of Anonymous Referee #1 — notably that for a paper focused on methodology there are quite a few details lacking. It's possible to find some information on inversion parameters in the code in the GitHub repo, but the repo is confusingly set up, with instructions focusing on how to run it on one HPC at one institution. I would recommend including discussion of the inversion parameters in the text and if possible, making the code more user friendly.

Fair point. Considering the first part, we refer to the Response_Reviewer1 document where we specified the changes made to clarify the methodology. For the second part, we improved the GitHub to make things easier to find.

I also agree that it would be good to present posterior distributions for inverted parameters: perhaps a table showing prior ranges for uplift rate, wave base height etc., together with the 95.4%-confidence ranges from the posterior distributions. It would be also nice to put some 95% contours on Figure 3e-g.

Instead of a table we opted to add the confidence ranges visually in both Figures 3 and 5.

A paragraph discussing the results of the Corinth inversion in terms of geological plausibility would strengthen the paper… Do the different posterior distributions on initial slope and erosion rate make sense given the local geomorphology of each profile swath? Relative differences in nearby sediment supply might influence erosion rates, for example.

We added a couple of lines in the text to discuss this (Lines 324-326), as it is indeed an interesting point to emphasize that erosion rate and wave base depth all overlap between the 3 profiles.

"Although we might expect lateral differences in these rates given variability in sediment types, catchment area and coastal orientation, the broad ranges for the posterior distributions indicate we cannot quantify these lateral differences from the profile morphology alone. "

I don't have much to say about section 6.2, but I agree with Anonymous Referee #1 that it comes as a bit of a surprise (especially given the title) and could be shortened.

We feel like the implications of the Corinth inversion are very interesting, and merit a deeper discussion beyond the methodological advances that this paper presents. We think some readers will appreciate such a discussion, and have thus decided to keep this section as detailed as it is. We did change the order of sections 6.1 and 6.2, as to end the discussion in a broader context.

To emphasize more that we find the discussion of Corinth a key part of the paper, we modified the title, which is now;

"Bayesian reconstruction of sea-level and hydroclimates from coastal landform inversion: application to Santa Cruz (US) and Gulf of Corinth"

and added several sentences throughout the paper to help that emphasis (Lines 76-81, 225-229, 293-296, 346-349, 355-356, 503-506).

Finally, a couple of typos:
A colon is missing between "Andersen et al., 2010" and "De Gelder" in the caption of Figure 1.

Fixed as suggested. (Line 94)

There is a "d" missing in "and" in the 4th line of page 6.

Fixed as suggested. (Line 182)

Anyway, nice paper!

Thank you!

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 by the reviewer,

Kind regards,

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