

### **Referee 1**

I thank the authors for considering the questions raised during the review. I think the paper is relevant to NHESS and could be published, provided the article becomes more cautious on the two points below:

We are grateful to the reviewer for the comments on our work.

- long term coastal dynamics: many coastal morphodynamics scientists will disagree with the sentence below: "It has have also been demonstrated that the changes in the beach profile play a minor role in the shoreline retreat due to sea level rise and waves. On top of the above reasons, we can hardly avoid the simplifications can be hardly avoided, as numerical approaches reproducing the long term morphological response of the beach do not exist so far." The papers quoted by the authors in the previous sentence and earlier in the manuscript clearly demonstrate that we don't know so much about beach morphodynamics, but that some modeling approaches exist (e.g. Ranasingh et al 2012). I think that at least this sentence should be reconsidered.

We have modified the sentence according to this comment. We have also quoted the new reference provided in the context as indicated

- language on uncertainties: saying that the  $\pm 1\text{-}\sigma$  uncertainty is an "upper uncertainty limit" is not appropriate: the IPCC sea-level report (Ch13) and its supplementary material provide the adequate language to be used in this case

I hope this review is useful.

We agree. This was specified in the text but we have now added a new sentence to highlight that these uncertainties quoted here correspond to  $\pm 1\text{-}\sigma$  (page 8, lines 27-28).

### **Referee 2**

The authors have revised the manuscript according to the suggestions of both referees. In this revised version, they have made efforts for improving the quality; however, some concepts are still unclear and in some sections the use of technical and English language should be revised.

We have now followed new reviewers' comments and the manuscript has been further modified. We hope that the new version will be satisfactory for reviewer 2 as well.

### **Referee 3**

This paper examines the impacts of future sea level rise and a changing wave climate on shoreline positions of two beaches in the western Mediterranean. The authors' analysis is sound; however some details in the methodology were omitted and need to be clarified. This is an important topic that fits the scope of NHESS and I suggest publication after moderate revisions.

We thank the referee for in depth her/his review.

#### **1. Section 1: Additional papers that could be referenced:**

Passeri et al., 2015, doi: 10.1002/2015EF000298

Gutierrez et al., 2011, doi 10.1029/2010JF001891

Plant et al., 2016, doi: 10.1002/2015EF000331

While the authors are neglecting coastal erosion in their projections, I think it is important for them to mention that this study goes beyond "bathtub" approximations of sea level rise (see Passeri et al., 2015) - a bathtub approach would simply assume that future coastal retreat would be at the 1 m contour for 1 m rise in sea level. Rather, the authors are dynamically simulating waves and water levels under a changing climate and SLR to determine the future shoreline position. This provides additional novelty for the paper.

We thank the reviewer for pointing this out. We have added a specific comment on this respect together with the references provided.

**2. Section 2.4: What is the overland extent of the SWASH model? Does SWASH resolve wave runup? This would be necessary to accurately compare the wet-dry shoreline with the video footage. Since you are looking at the wet-dry interface as proxy for the shoreline, how does SWASH resolve wetting and drying processes? This should be mentioned in the model description.**

We have extended the model description in section 2.4 to address the issues raised by the reviewer.

**3. Section 2.5: Is the SWASH model forced with tides? How is sea level rise incorporated into the models?**

Regarding the tides, these are very small in the Mediterranean Sea so we have neglected their impact in our assessment. Also, since we are not working in a wide continental shelf, tidal changes are not foreseen as a result of sea level rise.

Regarding the why in which sea level rise has been incorporated, we have done so by including the corresponding still water levels into the model runs corresponding to the selected range of sea level rise projections. This information has been included in section 2.5 for clarification.

**4. Section 4: A brief description of the PETRA model should be included. I would also consider moving this to the methodology since it is what you are basing your assumption of a constant beach profile on.**

We have followed reviewer's advice and have now incorporated the description of the PETRA model in section 2.4 of the methodology.

**5. Section 4: This is the first mention of the wall backing the beach. A better description of the study area is needed at the beginning of the manuscript. Also, do the beaches have dunes/what is the elevation of the dune or berm? Dune height has been linked to long- term shoreline change (see Plant et al., 2016) and would affect the inundation extent. The authors conclude that coastal retreat is lower in Playa de Palma due to a steeper beach slope - again, this is the first mention of the beach slope. By moving the discussion of the PETRA model to the methodology, this would help to better describe the study area. Lastly, are these beaches nourished? If so, this could help to justify neglecting coastal erosion.**

We have extended the description of the beaches in the beginning of the Data and Methods section, including the reference to the steeper slope of Playa de Palma and the fact that both are urban beaches backed by walls and promenades (thus without presence of natural environments such as dunes).

Regarding the beach nourishment, unfortunately there is no data available to be discussed. We found out that both beaches were actually nourished in the 1980s but we have no further information about it.