Second review of "Importance of non-stationary analysis for assessing extreme sea levels under sea level rise"

The authors have replied to my previous comments and have carried out the appropriate changes. In this second review, I'm providing minor comments related to the lack of explanation in some parts of the paper (mainly methods) and grammar. However, I may note two major comments (1 and 2) to the authors and the editor:

(1) The authors claim that one of the objectives of the present work is to assess "*which parametric method best accommodates non-stationarity conditions*". The discussion and conclusion sections are written as if the results can be extrapolated to any other location (except for one sentence in line 338). However, the non-stationary conditions change with the conditions of the area of study. As I noted in the first review of this paper: "*As is, the manuscript conclusions are appropriate for the study case (Punta Della Salute) only. In order to provide a quasi-standardized method for non-stationary analysis, the paper will benefit from applying the analysis to a larger set of tide gauge records, so the authors will be able to assess whether the conclusions can be extrapolated to other areas of study. Likewise, readers will be able to decide which method should be used according to the conditions of each case: tide range, relative relevance of surge vs tide in extremes – as in Dixon et al 1999-, location, record length, etc. In this sense, Haigh et al 2010 showed that differences between return levels estimated using different methods highly depend on the record length and on the presence of outliers (when using direct methods).*"

In the second version, the authors included a new location, the Marseille tide gauge, which indeed shows that results change from one location to another (lines 246 to 247; 271 to 272). The inclusion of this second tide gauge is not, however, sufficient to generalize the results. The comparison of different extreme value distributions has been performed previously. The novelty of this work relies on the fact that they use a set of methods that have not been compared yet (to the best of my knowledge). Although this provides some novelty, I strongly believe that the paper can make a difference in the literature by performing the analysis using a comprehensive set of locations (globally ideally).

Concluding, I suggest the authors choose one of two options 1) a local study in which the results are limited to the two tide gauges used. If this option is chosen, the narrative of the paper should be modified to reflect this, or; 2) include a full set of tide gauges so that differences in conditions (record length, tidal range, surge vs tide, etc.) are included in the analysis and the results can be extrapolated.

(2) The main objective of the paper is to evaluate the performance of various extreme value distributions to account for non-stationarity, so it is very methodologically focused. Acknowledging this, the exposition of the methods is short and poor. A discussion of the

pros and cons of each method would fit the paper. Also, the uncertainties of the models should be discussed in the results and conclusions.

(3) The article will benefit from a rewrite to avoid the "telegram style" taking particular attention to punctuation and typos, which are frequent throughout the paper (e.g., lines 247, 278, 299). It will also benefit from the integration into the narrative of the new tide gauge results. As is, the Marseilles tide gauge seems to be shoehorned into the paper.

Abstract

- Lines 6 and 7 need to be referenced. Coastal flooding is a very important hazard derived from mean sea level rise, however, many other climate change-related hazards pose important impacts (e.g., temperature). Also, the storm surge intensification has caused some debate in the literature.
- The abstract needs to be updated to include the results from the Marseille tide gauge. For instance, line 9 states that 96 years of data is used, which is not true for the case of the Marseille tide gauge (line 103).
- Line 15. What do you mean by "Actualized"?

Introduction

- Lines 34 to 41. The authors start the saying: "*Assuming stationarity when data are non-stationarity has several practical implications. First…*" However, they only indicate one: the use of return levels for designing structures.
  Also, the information contained in lines 37 to 42 is pretty much the same. I recommend to re-write this part to avoid redundancies and empty wording (i.e., a message that seems to contain meaningful content but does not).
- Lines 42 to 50. This is a miscellaneous paragraph:
  - (i) It starts with "*Several methods were proposed to cope with non-stationary conditions*" but the authors do not offer an introduction to these methods.
  - (ii) It continues with the importance of long-term records to identify non-stationarity. However, they don't include further information on that, for instance, what is the minimum record length to assess non-stationary conditions.
  - (iii) It highlights that mean sea level is not the only source of non-stationarity. Aren't the authors considering mean sea level as the only source of non-stationarity as well? Also, the different sources of non-stationarity are indicated here and on lines 33 to 34.
  - (iv) It is stated that there are no "*clear indications on which approach suits better non-stationary conditions*". What do you mean by "approaches"; the detrending techniques, the extreme value distributions (which you speak about in the next

paragraph as a second challenge), the extreme value parameters that vary with mean sea level?

I believe this paragraph and also the introduction section will benefit from an in-depth revision from the authors.

- Line 61: "*Given the above knowledge gaps"*: Depending on the definition of "approach" (see my comment above), I only see one gap: identify the best model to account for non-stationarity (derived from mean sea level) in extreme sea levels from the set of models presented in the paper.

Methods

- Lines 95 to 96. Do you interpolate the half-hour and 10 minutes data to hourly data? Working with different time resolution data can influence your results.
- Line 125. How do you define "stability" in this case? More explanation should be provided to ensure the reproducibility of your analysis.
- Line 145: "*This property can drive the selection of an appropriate threshold u*", how?
- Lines 153 to 154. I would extend this explanation a bit more. What message do you want to deliver to the reader by saying that the PP is called nonhomogeneous when you include covariates in your model?
- Line 161. You might want to use a better reference for the independency between surges and tides. Marcos et al 2009 just describe the method they use in their paper but do not provide a theoretical background for that assumption.
- Lines 163 to 164. Other works have used the RJPM fitting the extreme value distribution to the values above a threshold (Batstone et al., 2013; Baranes et al., 2020; Enríquez et al., 2022) instead of using the highest measured surge.
- Line 170. Wöppelmann et al (2014) found 24 main tidal constituents in Marseille (Table 2 in their paper), since you are citing them here, why do you use 21 tidal constituents, and what are those?
- Line 174. The calculation of the empirical return period in the RJPM is still unclear.
- The RJPM needs further explanation.

Results

- Line 249 to 250. "*The location is included in the scale parameter of the GOD that does not improve the fit (Fig. 3)*". This needs more explanation.
- Lines 254 to 259. Are these results for Punta della Salute only or for both locations?
- The uncertainties are not discussed in the Results.
- A similar figure as Fig. 3 should be included for Marseille.
- Yellow area in the figures are difficult to see.
- Some reasons why the analyses of Marseille tide gauge seem to be shoehorned in the paper:

1. Marseille results are missed in the Abstract section
2. Lines 63 to 65
3. Section 2.1: 17 lines speaking about the Venice lagoon and the Punta della Salute station versus almost 2 lines about Marseille.

Conclusions

Conclusion section is too short. Which extreme value model should we use at the two tide gauges if we want to analyze non-stationarity? Which one result in less uncertainty?