

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/nhess-2021-236-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on nhess-2021-236

Anonymous Referee #1

Referee comment on "Extreme Storm Surge estimation and projection through the Metastatistical Extreme Value Distribution" by Maria Francesca Caruso and Marco Marani, Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-236-RC1>, 2021

The manuscript aims at assessing the performance of the Metastatistical Extreme Value Distribution in estimating high quantile of extreme sea level also including future sea-level rise. The topic discussed is relevant and important to improve the resilience of coastal systems facing the effect of a changing climate. However, few aspects discussed in the manuscript need to be revised and discussed more in-depth. The terminology and notation used need also improvement to ensure consistency throughout the manuscript and avoid confusion in the readers.

From the title of the manuscript, the reader expects to read a study about extreme storm surge. However, the study's objectives (Lines 66-68) refer to extreme sea level. Later on, Line 150, the Authors say that they will investigate the variable $h(t)$ being the sum of tide and storm surge, so sea-level without mean sea level. I would encourage the Authors to clearly state the variable of interest and the variable used when performing the analyses, see also other comments below.

Information regarding MEVD, which is the main method investigated in the manuscript, is limited. The Authors say that this method guarantees "the least amount of a-priori assumption" (line 56). However, the following assumption must be made: $F(x, \theta)$ in Eq. 2, the threshold for the ordinary values, the estimation window for parameter estimation, the time-lag to ensure independence between ordinary values. How then is this method the one with the least amount of a-priori assumptions? I suggest clarifying further the advantages of the MEVD compared to the other two methods investigated. Moreover, additional information should be discussed: how the threshold for the ordinary value was selected (line 121 says "as small as possible"); how the 5-year estimation window was selected; why the 30-day lag time for the independence of the ordinary value is so different compared to the values found in the literature (lines 173-179); and how $F(x, \theta)$, which turns out to be a GDP (Line 267), is different compared to the classical GDP

I do see the value in implementing the cross-validation procedure to assess the predictability power of the distribution selected as representative of the observations. At the same time, I see the cross-validation as an additional measure of goodness of fit rather than the main one. The NDE only tests if the one quantile associated with the return period T_r of interest is well captured. What about the other quantiles? Is the

distribution representative of the entire sample? Also, how the observed quantile $h(\text{obs},p)$ is calculated? Which sample (M,S, or V) is used? The Q-Q plots are mentioned only in the results section and they are only performed for the 30 years in-sample test. In my opinion, the Q-Q plots put the NDE into perspective and should be included as goodness-of-fit method. Also, it would be useful to have them in the main manuscript. I do understand that the space is limited, maybe the Authors could consider including in the main manuscript only the ones related to the MEDV.

In the section Return Period, the definition of Equation 4 needs to be further discussed. Even if the Authors replace (h) with (z-msl), Equation 4 is still the return period of (h), and not the return period of the (z), as indicated by the Authors. Mean sea level (msl) shows a clear linear trend and such trend is recognizable in (z). Similarly, in Equation 5, the distribution G is the distribution of the variable (h) and not the variable (z) as reported in line 341. This has an implication in Figure 5. I assume that the y-axis in Figure 5 "water level" refers to the variable (z). This variable (z) is time-dependent, while in Figure 5 it seems like the statistical properties of (z) are constant. I would have expected something similar to the effective return level plots, to show the effect of sea-level rise. How (msl), which is time-dependent, is added to (h), which is not time-dependent, to derive Figure 5? I suggest clarifying the transition from the analysis on the variable (h), a random variable, to (z), which presents a linear trend due to (msl). I also suggest being more precise with the notation and the terms used throughout the manuscript. It is very difficult to understand the variables the Authors refer to because are often called with many different terms, e.g., total sea level, water level, extreme sea level...

The Authors say that "MEVD proves to be a good model for the extreme sea levels" (line 288) and that "MEVD-based estimates outperform the traditional approaches" (line 301). I do fail to see what the Authors describe. In the QQ-plots Figure S2-6, MEVD in the in-sample analysis has, in general, the highest variability, especially compared to the GEV. In the out-of-sample, MEVD looks better for lower quantiles, but it has quite a large variability for higher quantiles, compared to the other distributions. Overall, it is difficult to quantify which distribution performs best. This is also reflected in the NDE plots, Figure 3, where the differences between distributions are minimal.

Point by point comments:

- Line 92. Please revise the notation. $\Pr(M_n \leq x) = F(x)^n$ where M_n is the maximum of a sequence of independent random variable X. See also Coles 2001 (line 415)
- Line 154. Additional discussion is needed concerning the fact that $h(t)$ can be considered a stochastic variable even though a determinist component is included. Also, a literature review on indirect and direct methods (Line 149) for extreme sea level is missing.
- Lines 133. The Authors discuss the negligibility of tide-surge interaction. Does this condition hold in the case of Punta della Salute which is located within the Venice Lagoon?
- How the GDP threshold is selected and tested?
- It would be very interesting and useful to appreciate the difference between the performance of the distribution functions to see the sample of maxima used for fitting the distributions.
- Lines 205-209. My suggestion is to revise this paragraph. The terminology is confusing. I believe the Authors here are discussing the variable (z), in which storm surge is a component.
- Lines 220-221. The Authors say that the tidal and storm components do not change over time as mean sea level. How did the Author check that no trend is detected in the variable h?

- Section 3: Was the trend test performed only on the annual maxima or also on the samples of maxima used to compute the GPD and the MEVD?
- Line 281: Storm surge or storm surge and tide?
- Line 285: what is L?