

Interactive comment on “Study of the threshold for the POT method based on hindcasted significant wave heights of tropical cyclone waves in the South China Sea” by Zhuxiao Shao et al.

Anonymous Referee #1

Received and published: 14 March 2019

The manuscript "study of the threshold for the POT method ..." is scientifically interesting, in that it explores methodologies for extreme value analysis in presence of tropical cyclones, where the choice of too low thresholds can lead to excessively broad extreme tails, and to irreasonably high return values for high return periods.

However the quality of the manuscript could be improved a lot. First of all, the way the threshold is selected is unclear, and I don't understand many details of what the authors do. Sometimes the text is exceedingly technical, verbose and full of repetitions. Other times relevant aspects of the research are omitted or unclear. Furthermore, the quality of the English is far from optimal.

[Printer-friendly version](#)

[Discussion paper](#)



I would therefore suggest a careful review, that should substantially change the way the methodology is exposed. I would also suggest to have the manuscript copy-read by a motherlanguage English.

Below a list of more detailed comments.

- the title is long and too technical. One has to read it a couple of times to understand the argument. Why not something simpler, like "extreme value analysis of tropical cyclone waves in the Southern Chinese Sea" ?
- lines 14-20 of the abstract: the authors could simply write "A 40-year (1975-2014) hindcast of tropical cyclone waves is used to study the extreme wave heights, employing a Generalized Pareto Distribution (GPD) approach". The rest are details that could be left in the discussion.
- end of the abstract, "initial sample": as far as I can understand, here and elsewhere for the authors the "initial sample" is what I would call the "sample", and for the authors the "sample" is what I would call the "peaks over threshold".
- page 4, line 18: in what way the return levels in AM are unreasonable? And why?
- page 5, line 4: .. which shows that it is possible to study the extreme significant wave height of tropical cyclones
- page 5, lines 5-10: the meaning of these lines is rather unclear.
- page 5, line 7: peak significant wave height, maybe the "maximum significant wave height" would be better, as it is in both space and time.
- page 5, lines 18-19: the words "threshold selection method" are repeated twice in the same sentence (the authors here and elsewhere should avoid so many repetitions)
- page 5, line 20, the acronym ATSME is not introduced.
- page 6, lines 3-5: this is not necessarily true: one could use some automatic tech-

[Printer-friendly version](#)

[Discussion paper](#)



nique to decide when the r.l. is not changing.

- page 6, line 10: "the subjective definition still exists in the atsme". It is not entirely clear why
- page 6, lines 17-18: "in section 4 the sampling method is described".
- page 6, line 19: "section 5 discusses the characteristics of ..."
- section 2: this is be the right place to summarize the technique used by the authors to estimate the threshold.
- page 9, line 2: from "nine-hundred" to "tropical cyclone" there is something wrong in this sentence
- section 3.2, a figure with the position of the 22 locations would be useful
- page 10, line 10 and elsewhere, for me this is the sample. The values beyond the threshold are the peaks over threshold
- page 11, line 4, I would add here that the extrapolation here is done fitting the peaks over threshold with a GPD.
- page 11, line 5, you extrapolate only for high return periods, correct?
- page 11, lines 15-20. What technique did you use? ATSME, then the method should be better summarized somewhere, e.g. in section 2.
- page 11, line 19: using ATSME the threshold range depends on the return period? This should be also explained in section 2, and how you can choose a single threshold (I guess, you can take the lowest of the upper limits of the ranges?).
- page 12, line 7, peak -> maximum
- section 5. This (lengthy) discussion does not entirely explain the (interesting) bimodal shape showed by the sample. Is it possible that the 2 modes correspond to 2 different physical characteristics of the TC in this area? Do you have this shape everywhere, or

[Printer-friendly version](#)

[Discussion paper](#)



only in a few locations?

- page 16, line 1, remove waves
- page 16, line 11 "at location 1 above the threshold"
- page 16, lines 12-17, remove the list of values, as they are already in table 2
- page 17, the meaning of figure 6 is not entirely clear. What do the author mean with the word "empirical"?
- section 5: in the end it is not entirely clear how the authors selected the threshold. They used ATSMC to select a range for each r.p., and then how did they choose a single threshold? Is it simply the separation point between the modes of the distribution? But that would not be general, as not all the distribution of TC extremes are bimodal.
- section 5: maybe a map with the 22 locations reporting, for example, the 100-year return level would be useful and informative.
- the conclusion could be a little shorter and less technical.
- especially in the conclusion there are several error on English, that should be corrected (the conclusion is a key part of the manuscript).
- line 15: is this 2-modal distribution a consequence of the sampling technique, or is it general, with a physical explanation?

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-349>, 2019.

Printer-friendly version

Discussion paper

